This is a digital copy of a book that was preserved for generations on library shelves before it was carefully scanned by Google as part of a project to make the world's books discoverable online.

It has survived long enough for the copyright to expire and the book to enter the public domain. A public domain book is one that was never subject to copyright or whose legal copyright term has expired. Whether a book is in the public domain may vary country to country. Public domain books are our gateways to the past, representing a wealth of history, culture and knowledge that's often difficult to discover.

Marks, notations and other marginalia present in the original volume will appear in this file - a reminder of this book's long journey from the publisher to a library and finally to you.

Usage guidelines

Google is proud to partner with libraries to digitize public domain materials and make them widely accessible. Public domain books belong to the public and we are merely their custodians. Nevertheless, this work is expensive, so in order to keep providing this resource, we have taken steps to prevent abuse by commercial parties, including placing technical restrictions on automated querying.

We also ask that you:

+ *Make non-commercial use of the files* We designed Google Book Search for use by individuals, and we request that you use these files for personal, non-commercial purposes.

+ *Refrain from automated querying* Do not send automated queries of any sort to Google's system: If you are conducting research on machine translation, optical character recognition or other areas where access to a large amount of text is helpful, please contact us. We encourage the use of public domain materials for these purposes and may be able to help.

+ *Maintain attribution* The Google "watermark" you see on each file is essential for informing people about this project and helping them find additional materials through Google Book Search. Please do not remove it.

+ *Keep it legal* Whatever your use, remember that you are responsible for ensuring that what you are doing is legal. Do not assume that just because we believe a book is in the public domain for users in the United States, that the work is also in the public domain for users in other countries. Whether a book is still in copyright varies from country to country, and we can't offer guidance on whether any specific use of any specific book is allowed. Please do not assume that a book's appearance in Google Book Search means it can be used in any manner anywhere in the world. Copyright infringement liability can be quite severe.

About Google Book Search

Google's mission is to organize the world's information and to make it universally accessible and useful. Google Book Search helps readers discover the world's books while helping authors and publishers reach new audiences. You can search through the full text of this book on the web at `http://books.google.com/`

No.

BOSTON
MEDICAL LIBRARY
ASSOCIATION,
19 BOYLSTON PLACE.

THE

MONTHLY JOURNAL

OF

MEDICAL SCIENCE.

BOSTON MEDICAL LIBRARY ASS'N.
OCT 25 1886

VOL. XII.

(THIRD SERIES, VOL. III., JANUARY TO JUNE 1851.)

WITH FOUR LITHOGRAPHS, AND NUMEROUS WOODCUTS.

EDINBURGH: SUTHERLAND AND KNOX.
LONDON: JOHN CHURCHILL, PRINCES STREET, SOHO.

MDCCCLI.

MURRAY AND GIBB, PRINTERS, EDINBURGH.

ALPHABETICAL LIST

OF THE

CONTRIBUTORS OF ORIGINAL ARTICLES.

ADAMS, F., LL.D., Banchory.

ALISON, W. P., M.D., F.R.S.E., F.R.C.P.E., Professor of the Practice of Medicine in the University of Edinburgh.

ARNETH, Dr F. H., of Vienna.

BEGBIE, JAMES, M.D., F.R.S.E., Fellow of the Royal College of Physicians of Edinburgh.

BELL, CHARLES, M.D., Fellow of the Royal College of Physicians of Edinburgh.

BELL, G. HAMILTON, Surgeon, Fellow of the Royal College of Surgeons of Edinburgh.

BENNETT, JOHN HUGHES, M.D., F.R.S.E., F.R.C.P.E., Professor of the Institutes of Medicine and Clinical Medicine in the University of Edinburgh.

BRAID, JAMES, M.R.C.S., Edin., &c., Manchester.

BRAMWELL, JAMES P., M.D., Perth.

CHRISTISON, R., M.D., V.P.R.S.E., Professor of Materia Medica and Clinical Medicine in the University of Edinburgh.

CRUIKSHANK, JAMES F., Dalmellington, Ayrshire.

CUMMING, G. V., M.D., Garrison-Surgeon, Vizagapatam.

GAIRDNER, W. T., M.D., Fellow of the Royal College of Physicians of Edinburgh, Pathologist to the Royal Infirmary of Edinburgh.

GLOVER, R. M., M.D., F.R.S.E., Lecturer on Materia Medica in the Newcastle-on-Tyne School of Medicine, &c.

GOODSIR, JOHN, F.R.SS.L. & E., Professor of Anatomy in the University of Edinburgh.

GRAY, JAMES, M.D., Perth.

GREGORY, WILLIAM, M.D., Sec. R.S.E., F.R.C.P., Professor of Chemistry in the University of Edinburgh.

HOWIE, JAMES H., M.D., Shandon, Gareloch, late House-Surgeon, Glasgow Royal Infirmary.

KEITH, WILLIAM, M.D., M.R.C.S., London, Surgeon to the Royal Infirmary, Aberdeen, and Lecturer on Clinical Surgery, there.

KIRKWOOD, A., M.D., Surgeon to the Berwick Dispensary, and to the Infirmary of the Berwick Union.

MACKENZIE, R. J., F.R.C.S.E., Junior Ordinary Surgeon to the Royal Infirmary, Edinburgh.

MILLER, JAMES, F.R.S.E., Professor of Surgery in the University of Edinburgh.

MYRTLE, JOHN YOUNG, M.D., F.R.C.P.E.

REDFERN, P., M.D., London, Lecturer on Anatomy and Physiology, and on Histology, at the University and King's College, Aberdeen.

ROBERTSON, WILLIAM, M.D., F.R.C.P.E., Physician to the Royal Infirmary, Edinburgh.

SIMPSON, JAMES Y., M.D., F.R.S.E., President of the Royal College of Physicians of Edinburgh, and Professor of Midwifery in the University of Edinburgh.

SMELLIE, WILLIAM, M.D., Wick, Caithness-shire, late of the H.B. Co.'s Service.

SMITH, Messrs T. & H., Chemists, Edinburgh.

STRUTHERS, JAMES, M.D., Leith.

STRUTHERS, JOHN, F.R.C.S., and Lecturer on Anatomy, Edinburgh.

STUART, CHARLES, M.D., Chirnside, Berwickshire.

SYME, JAMES, F.R.S.E., President of the Royal College of Surgeons of Edinburgh, and Professor of Clinical Surgery in the University of Edinburgh.

TRAILL, JOHN, Surgeon to the Arbroath Infirmary.

TRAILL, WILLIAM, Surgeon, Arbroath.

WOOD, ALEXANDER, M.D., Fellow and Member of Council of the Royal College of Physicians, &c.

THE

MONTHLY JOURNAL

OF

MEDICAL SCIENCE.

BOSTON MEDICAL LIBRARY ASS'N.
OCT 25 1886

S7

Part First.

ORIGINAL COMMUNICATIONS.

ARTICLE I.—*Case of Communication between the Stomach and External Surface of the Abdomen.* By WILLIAM ROBERTSON, M.D., F.R.C.P., Edinburgh, Physician to the Royal Infirmary.

ISABELLA DAVIDSON, an unmarried woman, thirty-six years of age, was admitted to Ward 13 of the Royal Infirmary, on the 11th November 1850. She gave the following history of her case:—She has resided all her life in the parish of Roxburgh, near Kelso, and has been accustomed to labour in the fields. Up to the age of fourteen, her health was remarkably good; about this period, however, she became subject to occasional darting pains in the left side of the epigastrium, and under the cartilages of the lower left ribs. These pains were not increased by pressure,—they continued to recur at intervals for seven years, and were accompanied with no signs of dyspepsia, except costiveness, eructations, and flatulent distension of the abdomen. Her food usually consisted of different preparations of oatmeal, of potatoes, milk, and occasionally of soup and boiled vegetables. Since she attained the age of fifteen, she has menstruated regularly. At the age of twenty-two, she gave birth to a child, after an easy labour. When twenty-five years of age, she recollects to have observed a soft elastic tumour (probably flatulent) between Poupart's ligament and the short ribs of the left side; it frequently shifted its place, and finally disappeared, when she was twenty-six years old, after an attack of diarrhœa, which lasted for six months. Up to this period she was always stout, and able for work; the diarrhœa did not prevent her from following her usual occupations, and her appetite continued good.

In 1842 she first observed a tumour in the left side of the epigas-

trium, not exceeding a pea in size. It was hard, painful, tender, but not discoloured; it increased steadily in size for two years, when it had attained the size of a very large orange. It then began to subside, and in about three months had completely disappeared. Pain and tenderness were still felt in its former situation for about another month (September 1844), when a dark spot, of the size of a half-crown, made its appearance in the epigastrium. At this period, the patient's general health first began to suffer; although able to shear at the harvest of 1844, and to engage in hard field labour, her strength and flesh became rapidly reduced. Her appetite still continued unimpaired.

On the night of October 22, 1844, while she was washing some clothes, she felt the black spot give way, and fluid trickle over the abdomen. She immediately stripped, found that a little blood and *matter* (?) had been discharged, and that the contents of her stomach were escaping by a rent in the black spot formerly noticed. She swallowed successively draughts of water and of milk, which gushed out in full stream from this orifice. She was seen next day by Dr Francis Douglas, then of Kelso, now of the H.E.I.C.'s service. His interference was limited to simple dressing of the sore, and the application of a bandage, to prevent the escape of the food. The patient states that when the dressings were removed, a sort of valve (mucous membrane?) could be seen closing the orifice, at a short distance from the surface of the abdomen. By the end of January 1845, the aperture had completely cicatrised. It continued closed for about a year, during which time the patient was able to move about, and engage in ordinary domestic occupations. She, however, continued to feel pain about the seat of the cicatrix, and was unable to work in the fields. Early in 1846 the cicatrix became red, small blebs made their appearance on its surface, and were succeeded by ulceration and re-establishment of the communication with the stomach. About the end of March 1846, the fistula again cicatrised; but in June 1847 ulceration occurred for the third time, and the communication with the stomach has existed ever since. For the last three years she has lain almost constantly in bed, and has worn a piece of linen, secured with a pad and bandage, over the external orifice of the fistula. She has been seen at intervals by Mr Stewart and Dr Hamilton, of Kelso, but has used no medicine except purgative pills, which she takes frequently, and laudanum, which has been given now and then, in small dose, to allay painful sensations. Her usual diet has of late consisted of coffee, bread, steak,—of many plain and nutritious articles of food, eaten frequently, and in small quantity, without much salt or other seasoning. She prefers her food warm, and drinks very sparingly. She says she is apt to shiver when anything cold is swallowed; and distension of the stomach, more especially with fluids, causes irritation about the external opening. She is of healthy family, and quite unaware of hereditary predisposition to any disease.

These particulars, gathered by interrogating the patient herself, were confirmed by the statement of her sister, who accompanied her to the hospital.

Her condition, on admission, was as follows:—She is dark haired, and of a ruddy, healthy complexion. There is no appearance of hectic, cachexia, or even of discomfort. She is rather thin, but has lost little of her muscular strength. Temperature of the surface of the body natural, but easily lowered by exposure to cold. Pulse 70, natural. Respiration slow, and unembarrassed. Tongue clean, with a few slight transverse fissures. Appetite good. Food taken in small quantity at a time. No thirst complained of. During the process of digestion, there is always more or less pain in the region of the stomach, and around the external orifice of the perforation. The bowels are habitually confined, and seldom act without the use of laxatives. The urine is secreted in small quantity; its density exceeds 1030; it is not coagulated by heat, nor on the addition of nitric acid. Menstruation continues regular. The patient occupies herself chiefly with reading and sewing. She is of cheerful disposition, and, for her station, remarkably intelligent. She sleeps well.

The external perforation is situated about two and a-half inches from the median line, and three and a-half from the umbilicus, close to the cartilage of the eighth left rib. It is of an oval form, and will admit an ordinary sized thumb. Its margin is rounded, depressed, hard, and red, presenting here and there minute points of ulceration. The surrounding integuments are, for the space of two inches around, red and indurated,—in a few points, ulcerated or excoriated. This appearance has been caused by the irritation of the parts during the patient's recent journey from Kelso. On passing the little finger deeply into the orifice, it meets with resistance when turned downwards and to the left side; in the direction of the pylorus it passes freely. The mucous membrane of the posterior wall of the stomach can be seen when the patient is placed in a favourable light, and when she takes a mouthful of milk it can be observed to flow over the surface opposite the external orifice.

The great similarity between the condition of my patient and that of Alexis St Martin, the subject of Dr Beaumont's interesting observations, suggested the propriety of repeating some of his experiments, and of instituting others which modern researches on the physiology of digestion render desirable. I accordingly brought the case under the notice of the Edinburgh Medico-Chirurgical Society, and with the assistance of a committee, appointed at their November meeting,[1] hope to have the opportunity of making such a series of observations as may seem compatible with the welfare of the patient,

[1] The committee consists of Mr Syme, Mr Goodsir, Dr Christison, Dr Bennett, Dr Douglas Maclagan, and Dr W. Robertson.

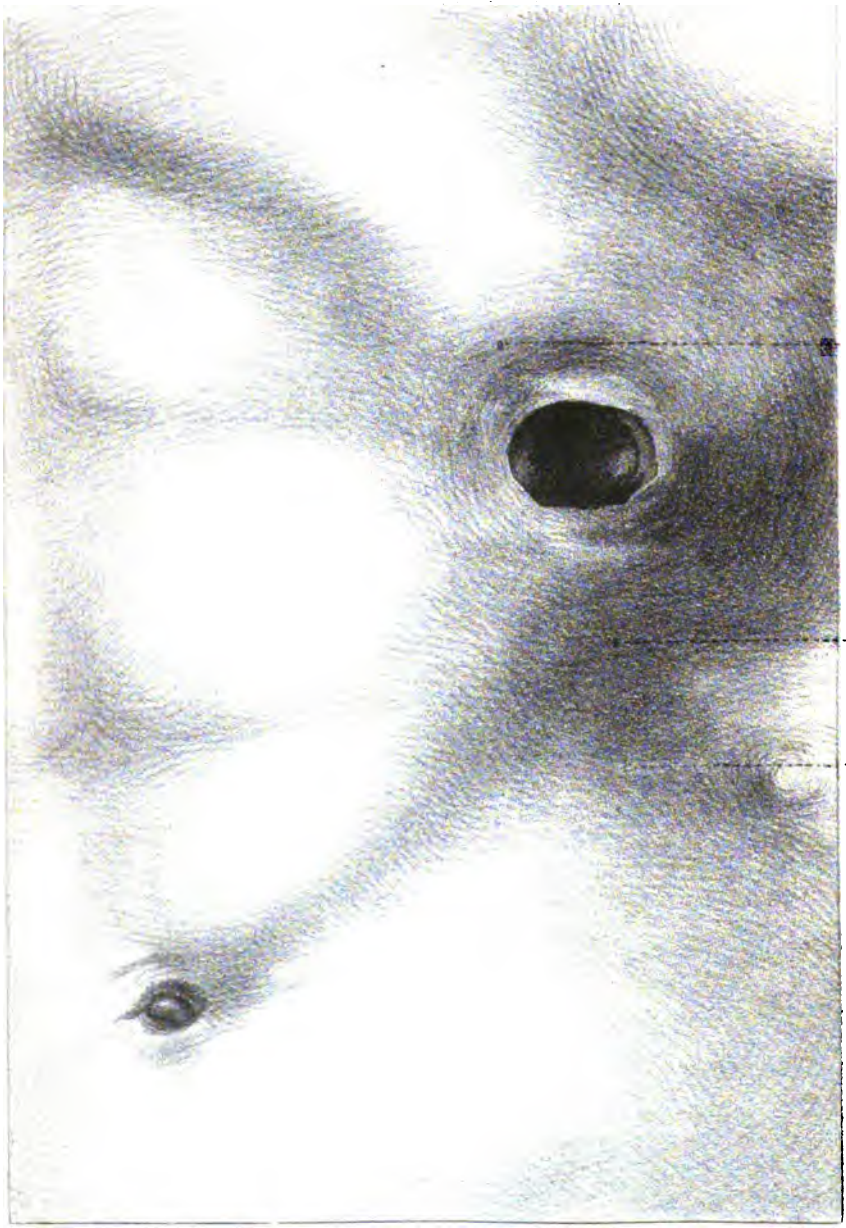
and which may at some future period be communicated to the profession. But as experiments on digestion can only be satisfactory if performed on a healthy subject, I have hitherto contented myself with regulating the action of the bowels by enemata and the use of proper diet, and with applying such mechanical contrivances as seem best adapted to promote healthy cicatrisation, and to reduce the irritation of the integuments by restraining the escape of the contents of the stomach from the preternatural orifice. In this I have partially succeeded, by laying a piece of vulcanised caoutchouc over the hole, and securing over all a pad and broad bandage. The dressing requires to be occasionally shifted, to permit the escape of flatus, which often accumulates to a troublesome extent. The contents of the stomach have been frequently roughly examined about mid-day: they are always highly acid, and contain particles of flour, coagulated albumen, milk, and other articles, derived from the patient's usual breakfast of coffee, milk, bread, and egg, taken about nine A.M.

On the 4th December I probed the cavity with an elastic catheter, and from the examination then made I am induced to believe that the perforation communicates with the stomach through its anterior wall, not far from the great curvature, and probably about four inches from the cardia. Although introduced with great caution, the bougie caused (as in Dr Beaumont's case) some little uneasiness or faintness.

About the same time my friend, Dr J. Smith, kindly made a coloured drawing of the external appearances, which are copied from it with sufficient accuracy in the accompanying lithograph. It will be seen that the external orifice has somewhat diminished in size since my first examination of the case,—that its upper margin includes the cartilage of the seventh rib,—and that the cartilages of the ninth and tenth ribs (indicated by figures 9 and 10) are situated beneath and external to it.

The precise nature of the lesion which induced the successive perforation of the stomach and abdominal parietes is rather obscure. The most probable conjecture which I can offer is, that a chronic ulcer of the stomach occasioned, whether by perforation or otherwise, extension of inflammation to a limited portion of its external surface, and consequent adhesion to the abdominal parietes,—that thereafter an abscess formed external to the stomach, and discharged its contents into the viscus,—that the contents of the stomach, acting upon the walls of the abscess, ultimately caused the ulceration of the integuments.

As to the prospect of cure, I fear the case does not admit of remedy,—certainly not of surgical interference at present. The edges of the aperture could not be thoroughly approximated without exposing or cutting away a portion of the cartilages of the seventh and eighth ribs, and although a flap of skin might easily be transplanted, so as to cover it, ulceration would, I believe, almost inevitably occur,



J. Bellaby Lithog Edinr

from distension from within, and from the action of the gastric juice and ingesta upon its internal surface. There would, moreover, be a risk of exciting fatal inflammation, to which I should not feel justified in subjecting the patient. Palliative measures, such as are at present adopted, may succeed in further reducing the size of the opening, and, at all events, render the patient's existence more comfortable.

ARTICLE II.—*Case of Difficult Labour, in consequence of Twins joined by the Breast.* By CHARLES STUART, M.D., Chirnside, Berwickshire.

THE following case is an additional instance of unforeseen difficulty occurring in the course of a labour, in which no certain rules of practice can be laid down, and shows what an amount of obstruction may be overcome, when sufficient time has elapsed for complete dilatation of the maternal passages.

I was called, on the morning of Thursday the 10th of October last, at seven o'clock, to attend Mrs E., æt. 22, residing in the village of Chirnside, who was in her first labour. On my arrival I found the pains regular, and of good strength. On examination, the os uteri was felt dilated to about the size of a shilling, soft and cushiony, the head presenting in the first cranial position, the vagina well lubricated; and, in short, everything was going on quite favourably. The pains continued steadily to increase, and after a space of four hours the os uteri was well expanded, and the pains began to have a bearing-down character, till about two o'clock P.M., when they recurred only at long intervals. Shortly after three o'clock the pains returned with considerable violence and frequency; but little or no change had taken place in the position of the head, and there seemed to be great difficulty in its passage through the brim into the cavity of the pelvis. I found that by allowing my patient to lie down the strength of the pains was most materially decreased, so I determined to keep her moving about as long as was prudent. I never recollect noticing this to such an extent in any other case, when the pains were so violent. The cranium advanced with extreme slowness, considering the strong nature of the pains; and it was only after the most severe straining that the head began to press on the perineum, and after a very tedious passage was born about seven o'clock P.M. The pains previous to this were of the most frightful description, and they were now, if it was possible, increased. Some apparently insurmountable obstacle seeming at this stage to oppose the further exit of the infant, I tried by every means in my power to discover the cause of delay, but from the extreme tightness of the parts it was impossible to ascertain its nature. I dreaded, from the enormous straining, that the uterus would speedily rupture, unless

delivery was immediately effected, so accordingly I applied at first gentle traction; but, when I found that unavailing, I was forced to increase it to what previously I would have considered a most unwarrantable degree, and succeeded in delivering the shoulders, when for the first time I discovered something unusual. I continued, however, my traction as my only hope of getting the woman delivered; and, after using the greatest force, I was in no small degree astonished when another head came down, with the face considerably flattened. This second head lay twisted round upon the back of the first delivered infant. After farther perseverance, I succeeded in extracting two males, still-born, and intimately joined from the sternum to the umbilicus, into which an umbilical cord, common to both, was inserted. The placenta speedily followed, and was not larger than is usually seen in cases of twins. The umbilical cord was rather thicker than usual. About an hour elapsed from the time the head was born till the delivery was completed. The pains during that period were of the most agonising and alarming character, and made me regret exceedingly having no chloroform. After a careful examination of the external parts, I was very glad to find no perineal laceration, which I feared very much from the passage of such a mass. The twins were at the full time, and fifteen inches long. The band of connection extended from the upper part of the sternum to the umbilicus, and was seven inches broad and three long; and the diameter of the twins, when laid together, was six and a-half inches. They were perfectly and fully formed in other respects, but the head that presented first was the larger of the two. I failed in obtaining permission to make any more particular examination.

When we consider the breadth of the connecting band between the two children in the above case, we see more clearly how the head of the second child could assume the position that it did, and to what an extent the connecting band must have been stretched to have allowed of its being placed at the back of the shoulders of the other child when delivered.

My patient is a woman of slender figure, but well formed, and of good constitution. During her pregnancy she enjoyed excellent health, which in some measure strengthened her for the extreme trial she had to undergo, and which she endured with the greatest fortitude.

She has made a most excellent recovery, and is now quite strong.

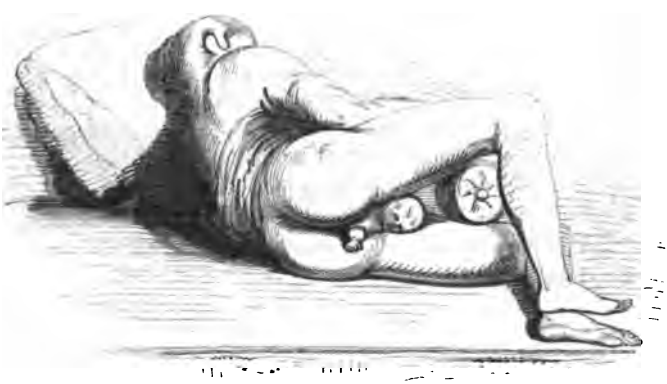
No doubt the long delay before the head was born, in a great degree saved her from the danger of perineal laceration, as there was ample time for complete dilatation, which was so essential for the safe passage of such a mass as had to follow.

ALLANTON, 18th November 1850.

ARTICLE III.—*Rare Case of Complicated Parturition.* By JAMES GRAY, M.D., Perth.

THE following instance of the protrusion of the hand of the child through the walls of the vagina and rectum (without injury to the perineum), in a case of common head presentation, is so rare, if not unique, in its occurrence, as to deserve being placed upon record:—

On the evening of the 26th February 1850, I was summoned to attend Mrs Y. in her first confinement. She resided at a distance of twelve miles from my residence in Perth. I found her in bed suffering from a severe expulsive pain. The cry which escaped her led me at once to make an examination. In doing so I discovered, as I thought, the head of the child partly born, and covered, as is not unfrequently the case, by the membranes. After the pain subsided, I had time to make a more careful examination. The os externum was found to be thrust forwards and upwards to the front of the pelvis; the perineum greatly distended; the orifice of the rectum dilated considerably,—exposing its mucous surface to the extent of an inch. The head presented in the second position; the membranes were entire. On introducing the index finger of the right hand into the rectum, I found that one of the child's hands lay upon the right cheek. Another pain soon followed, more fully stretching the perineum and opening the anus; without, at the same time, affecting the outlet of the vagina. To prevent the perineum from being injured, I placed the palm of my right hand over the anus and perineum, carrying my fingers into the vagina, so as to prevent the too sudden expulsion of the child. While thus engaged, I felt that something had given way; a gush of fluid followed, and a pressure of something solid against the palm of my hand. This I found to be one of the hands of the child, which had protruded itself through the walls of the vagina and rectum a little above the verge of the anus. The pain returning, I expected every moment, from the state of the parts, and from the powerful efforts made, that the perineum would give way. This was prevented by still carefully supporting the parts, retarding by counter-pressure the progress of the child—and thus giving time for the external parts to relax, which, in the course of half an hour, took place. The head, consequently, was gradually expelled, and as it emerged under the arch of the pelvis, the hand also descended through the opening in the walls of the vagina and rectum, as de-



scribed alove, until it hung over the left nates of the mother, as is seen in the accompanying sketch. With the shoulders another difficulty now presented itself; this I managed by pulling the arm in the direction of the spine of the mother, and guiding it through the aperture with my left hand; while, at the same time, with my right I supported the head, and carried it forwards and upwards during the continuance of a pain. The shoulders were soon expelled, and the child was safely delivered. The cord was tied, and in ten minutes the placenta came away. A bandage, with a pad attached to it, was applied round the abdomen. On the injured part being examined, the rent appeared to commence a little above the verge of the anus, and to extend upwards for about half an inch; but the muscular fibres of the rectum had contracted, and reduced it to a mere fissure. It was carefully cleaned, and a piece of lint introduced into the vagina, to absorb the discharge and prevent it from passing between the lips of the wound. At the end of an hour I left her, with strict injunctions to keep herself quiet, and not to move about in the bed. Next day I learned that she had passed a comfortable night, and was as well as could be expected. She went on gradually improving; the fæces were expelled *via naturali*, not in the least tinged with blood. In the course of three weeks she was able to be up a little. Since then I have seen her frequently, and she expresses herself as being quite well.

The circumstances most worthy of remark in this case are,—1st. The unfavourable position of the hand of the child. This, in a female who has had several children, would not have been attended with anything peculiarly dangerous; but with a woman who is in her first confinement, the case is otherwise. There is then a great retardation of labour, and the danger of rupture to the perineum. 2d. The unusual position of the posterior commissure of the os externum. This commissure was thrust forwards by the head of the child to the inferior extremity of the symphysis of the pubis, so that the vulva was thereby very much diminished. I have observed that, in many first cases, the external parts are carried forwards; but in no other case that has come under my notice have I observed so great an alteration from the natural position. The state of the anus is also deserving of notice. It was dilated to a size considerably above that of a crown piece,—the mucous surface of the rectum being exposed and carried downwards, so as to be in a line with the perineum.

Lastly, there is the protrusion of the hand through the walls of the vagina and rectum; the subsequent closing of the lips of the wound; and the healing process advancing without leaving any fistulous opening between the two canals. This last particular is rarely the result without the aid of a surgical operation.

The treatment adopted consisted in the application of a lotion of the sulphate of zinc, attention to the bowels, and strict rest in the horizontal position.

PERTH, 16th October 1850.

ARTICLE IV.—*Case of Hydrophobia.* By JAMES STRUTHERS, M.D., Leith.

(*Read before the Medico-Chirurgical Society of Edinburgh, 20th Nov.* 1850.)

JOHN WESTON, æt. 8½, was bitten, on the 13th October 1850, in the right eyebrow by a middle-sized dog. The animal was supposed to be mad, and was immediately killed. I saw the boy within an hour and a-half after the accident. There were two wounds, one above the eyebrow, an inch in length, and lacerated; the other immediately below the eyebrow, half an inch in length, and with a clean margin. Hemorrhage had ceased for some time, and the parts had been carefully washed.

There being reason to believe that the dog was mad at the time of the accident, I resolved to destroy the bitten parts as speedily as possible. The patient being much frightened and very unruly, was put under the influence of chloroform, without which it would have been impossible to have cauterised the parts satisfactorily, and without risk of injuring the eye. The two wounds were laid into one, the lacerated parts excised, and the whole of the raw surface carefully cauterised, first with the nitrate of silver, and then with the pernitrate of mercury; while the latter was being applied, one of the branches of the supra-orbital artery bled freely, and required a ligature.

The wound went on satisfactorily, and presented, at the end of the third week, a healthy cicatrix, with scarcely any deformity.

Nothing unusual had been observed in the behaviour of the dog previous to the day of the accident. In the morning he was noticed to be dull and restless, but took some food, and was observed once or twice in the course of the day lapping water in the usual way. In the afternoon he was observed rolling on the grass as if in agony, uttering at the same time a kind of suppressed bark or howl. It was at this time that the boy approached to caress him, when he sprang up and bit him in the eyebrow. Shortly before this the owner of the animal had given him some water containing nitre, which seemed to choke him, and caused vomiting.

The dog having been killed immediately after the accident, I was prevented from making any observations on his condition. There was nothing unusual, however, in the appearance of the body. The mouth contained some blood, but was free from froth, or fluid of any kind. On the following day I made a *post-mortem* examination of the body of the dog. The mouth, tongue, and pharynx presented nothing abnormal. The trachea and larynx were nearly filled by a rather firm clot of dark blood, which, as it did not extend into the bronchi, was supposed to have been extravasated at the time the animal was killed. The mucous membrane of the trachea and bronchi

was much redder than natural; and there was some frothy fluid in the bronchi. The lungs contained much dark blood, and presented several spots of ecchymosis on their surface. The heart was normal, and the large veins near it were distended with dark blood. The œsophagus was natural. The stomach was the size of an orange, contained some air, and about three ounces of a dirty greenish coloured fluid, in which were two blades of grass, and some dirty stringy mucus. The other abdominal viscera were normal; the bladder strongly contracted, and quite empty.

On the 15th November, thirty-two days after the accident, I was called at two in the morning to see the boy, who, I was informed, had been complaining for about two days. I found him sitting in bed in his father's arms. He was restless and fidgety, never keeping the same posture for a minute at a time. His look was quite peculiar, and expressive of profound dread. His eyes were full and clear, and much more intelligent looking than usual. On being asked what was the matter, he said he felt pain in the wound, in the eye, and all over the same side of the head. The cicatrix was pale and natural in appearance. He answered questions rationally, and without any appearance of fear; put out his tongue when desired to do so; and gave me his hand to feel the pulse, which was 112, rather weak, and remarkably irregular. While holding his hand I blew suddenly on his face, when he sprang, convulsed, to the other end of the bed; covered himself with the clothes; shook, and cried very much; and beseeched me not to breathe upon him any more. This established the diagnosis.

I then learned the following facts—namely, that he had continued quite well up to the 13th November, his friends taking care not to irritate him in any way, or to make any allusion to the dog. On the evening of the 13th, exactly a month after he had been bitten, he and some companions were out amusing themselves, when they were frightened by some one playing the ghost, and ran home. He refused to go out again, saying he was frightened. Immediately after this, on the ghost being mentioned, he began screaming and crying, complained of pain in the wound, in the eye, head, back, legs, and arms. He maintained also that his little fingers were bent and stiff, although his parents could not observe anything unusual. When any one happened to breathe upon him, he told them he did not like it, and to keep away from him. The same evening he took his supper, consisting of porridge and milk, in his usual manner, and without making any complaint. He slept but little during the night, was restless, and tossed about a good deal. Next morning he had some coffee for breakfast, but took only one mouthful, saying it would not go down. He tried water and milk, but with the same result. In the afternoon he ate a little biscuit, and took some milk, which he swallowed without difficulty. In the evening he asked for a potato, snatched it from his sister, took a bite of it hurriedly, and then pushed it away. He was restless during this, the second, even-

ing, and became more so towards morning, when I saw him for the first time since the seizure.

His state then was as formerly described. When asked if he would have some water or milk, he refused at first, but afterwards offered to take either. He put a cupful of milk to his mouth, but immediately pushed it away in an agitated manner, and buried himself in the bed-clothes. He was then offered some jelly, of which he readily took two or three spoonfuls, but in an agitated manner, and as if in a great hurry. He then had a bit of soft biscuit, which he ate calmly; when swallowing it he was ordered to hold up his head, when the act of deglutition was observed to be performed deliberately, and without any spasm or hurry. When asked if he had pain in the throat, he said "Yes," and pointed to the larynx. The tongue was clean, steady, and moist; there was no unusual amount of fluid in the mouth, and nothing like slaver on the lips. The breathing was calm, regular, and without noise. He was desired to get out of bed and to walk to the fire, which he did barefoot, on the cold floor, and without any agitation, and commenced warming himself. He was then offered a cup of cold water, of which he readily took a mouthful, but instantly drove away the cup, and ran into a corner, seemingly suffering from pain and spasm in the throat. The water was then warmed to blood-heat, when, after some pressing, he took another mouthful, which affected him in a similar manner, but not so severely. The temperature of the surface was natural, and there was no sensible perspiration. He could bear the legs, arms, and trunk to be handled and breathed upon without showing any uneasiness, but not so the face. He could also bear the head and spine to be tapped strongly, complaining only of slight tenderness from the head down to the middle of the back. A bright light presented suddenly caused slight agitation, but was soon borne with ease. The pupils were moderately dilated, and acted readily and equally. On being asked if he was afraid of anything, he hesitated, and would not answer me, but whispered to his mother that he was afraid of the ghost; and on being again asked, he told his father that he was afraid of the dog. He was now ordered 3ii. tincture of cannabis, with 3iij. of laudanum, of which he was to have half a teaspoonful every half hour.

At 8 A.M. his condition was much the same; he ate some biscuit and drank some milk during visit.

At 3 P.M. he was much worse. He could not remain quiet for a single instant. There was great general agitation, with constant spasmodic movement and jerking of the limbs; and every two or three minutes there occurred a fit of general convulsions, commencing about the throat, during which the countenance was distorted, and had a wild and savage expression. These fits occurred sometimes without any apparent cause, but were most frequently induced by his attempting to take anything in his hands, or attempting to swallow, or when he was breathed upon. There was now a profuse flow

of frothy fluid from the mouth, accompanied by constant hawking, and spasmodic working of the pharynx, with occasional retching and frequent belching of wind. The fluid was partly spat out and partly ran over the lips, whence it was constantly wiped away by the patient. He frequently asked for water, milk, and meat, and did not seem to have any dread of fluids except when he was pressed to take them, or when he attempted to swallow them. When offered any fluid in a cup, he would not take it, but would ask for a spoon, dip it into the cup, hold it for some time in front of him, make repeated and convulsive efforts to bring it to the mouth; then, with a look of great resolution, dash the spoon into the mouth, and throw it away. This was invariably followed by frightful convulsions of the whole body, which generally terminated by the belching of large quantities of wind. Once or twice he had spasmodic rigidity of the arms and hands, more especially of the right, and complained much of pain in the little fingers, experiencing great relief when they were squeezed. He was perfectly sensible, knew and named those around him, and answered questions rationally, but generally in monosyllables, and spasmodically, owing to the severity of the convulsions. He never showed the least inclination to bite or to hurt those around him. There seemed to be constant change of purpose and of thought. The senses of hearing, sight, and smell were preternaturally acute, though he once or twice said he could not see. It was impossible to deceive him in any way.

At half-past 6 P.M. Dr Christison saw him with me. He was then weaker, and presented the same symptoms as in the afternoon, but in a less marked degree. The pulse was scarcely perceptible. He ate a piece of biscuit and took a mouthful of tea, which was followed by severe convulsive efforts to swallow, and by convulsions of the whole body. He was ordered to have a suppository, containing half a grain of the muriate of morphia, to be followed in half an hour by another containing a whole grain.

At half-past 8 P.M. he was much weaker; had become a little quieter after the second suppository; and was said to have vomited some blood. He was seen at this time by Drs Coldstream, Keiller, W. T. Gairdner, and J. A. Sidey. The pulse was all but gone at the wrist; he had still fits of convulsions, but much less severe than during the day. He was evidently sinking fast. He continued much in the same state till half-past 9 P.M., when he expired calmly, having been nearly free from convulsions for the last quarter of an hour, and apparently quite sensible. Death took place thirty-three days after the accident, and fifty hours after the first symptoms had shown themselves.

Sectio-Cadaveris.

The body was examined 36 hours after death, by Dr W. T. Gairdner and Mr John Struthers, in presence of Dr Keiller and myself.

External Appearances.—There was slight lividity of the general surface; and the posterior part of the body presented the usual post-

mortem discoloration. There was a copious escape of whitish foam from the mouth and nose, that from the latter was slightly reddened. The *rigor mortis* was almost absent, except in the fingers and legs, where it was slight. The cicatrix on the right eyebrow was healthy in appearance. The eyes were clear and lustrous, the pupils moderately dilated, the left one oval and larger than the right.

Head. — Integuments rather exsanguine; superficial vessels of brain moderately congested. About half a drachm of clear fluid in each lateral ventricle, and very little at the base of the brain; the membranes healthy. The cerebral substance, both white and gray, very slightly and uniformly softer than usual. The vertebral and basilar arteries had a dark greenish appearance. The superficial vessels of the cerebellum and medulla oblongata were generally and pretty fully congested; the gray matter of the cerebellum rather deeper in colour than natural.

The cranial nerves were carefully examined, more especially the pneumogastric, both where it leaves the medulla oblongata, and at its place of origin in the floor of the fourth ventricle, but without detecting anything unusual.

Spinal Cord.—The dura mater normal. The pia mater in the dorsal and lumbar regions more congested than in the cervical, but not more so than usual. The same slight general softness which was observed in the brain existed in the cord, but to a less extent, more especially at its upper part. The medulla oblongata was normal, and firmer than any part of the cord.

Chest.—A considerable quantity of blood had escaped from the body when the head was examined, and before opening the thorax. There was no fluid in the pleuræ. Both lungs presented at the posterior part a few spots of sub-pleural ecchymosis, and at the lower and anterior borders a few ridges of interlobular emphysema. Numerous lobules were observed of a darker colour than the others, well defined, and depressed slightly below the general surface. About two drachms of clear fluid were found in the pericardium. The visceral pericardium presented several small spots of ecchymosis. The right ventricle and auricle contained two or three half decolorised clots, and a considerable quantity of frothy fluid blood. The left ventricle and auricle were well contracted, contained a little blood, but not the slightest froth.

The *Tongue*, *Pharynx*, and *Œsophagus* were perfectly natural, the mucous membrane presenting its usual pale colour. The arytenoepiglottidean folds, and the mucous membrane for half an inch below them, posteriorly, were of a faint rose-red colour, but not deeper than natural.

The *Sublingual Glands* were natural; the other salivary glands were not examined.

The *Trachea* was coated throughout with a sanguinolent froth. The mucous membrane of the epiglottis and larynx was natural; that of the trachea was of a rosy hue, and minutely injected, the red-

ness, commencing about half an inch below the cricoid cartilage, and becoming deeper towards the bronchi. The bronchi contained much frothy mucus; their mucous membrane was of a deeper red than that of the trachea. The pneumogastric nerve was carefully examined on both sides of the neck, and was found perfectly natural.

Abdomen.—The stomach contained about half a pint of dirty greenish coloured fluid. The mucous membrane in the posterior surface presented a considerable amount of finely arborescent injection, with some ecchymosis. The other abdominal viscera presented nothing unusual.

Remarks.—This is the third case of hydrophobia that has occurred in this neighbourhood within the last twelve months. The first of these occurred at Dalkeith, an account of which will be found in the report of the proceedings of the Medico-Chirurgical Society for last year; the second in Edinburgh, and was published by Dr Sidey in the last number of this Journal. The present case I consider worthy of being recorded, its history being complete, and a careful post-mortem examination having been made.

The state of the dog at the time of the accident, as described by those who saw him, was certainly such as to lead to the belief that he was mad. He was apparently suffering severely; and his bark was of that character which is considered pathognomonic of true rabies in the Dog. The animal also had been previously familiar with the boy, and bit him without any provocation.

The bite was in an unfavourable situation, the part being uncovered, and very vascular. The chances of inoculation taking place were thus rendered greater than if the bite had been given on a part covered with clothes, by which the teeth would have been wiped in passing. The most unfavourable view of the case having been taken at the time, the whole of the surface through which absorption might take place was immediately and thoroughly destroyed; and although an hour and a-half had elapsed, and the wound had ceased for some time to bleed, I entertained good hopes that the parts had been destroyed in time to prevent absorption of the virus. I gave, however, a guarded prognosis to the parents, and cautioned them against irritating the boy, or referring to the dog in his presence.

The examination of the body of the dog did not tend to remove the suspicion as to his having been mad. The appearances met with were those most frequently found in cases of hydrophobia, but they might have been caused by other diseases. The result of the case, however, puts it beyond all doubt that the dog must have been mad at the time of the accident; the symptoms of the disease were too well-marked to leave the slightest doubt as to the case being one of true hydrophobia, or canine rabies.

A fact incontestably established by this case is, that absorption may take place within an hour and a-half; and I think it not improbable that, in similar cases, unless the parts be thoroughly destroyed

before bleeding has ceased, we can have no security that absorption has not already taken place to an extent sufficient to induce the disease. The period of incubation was here thirty days, which is rather under the average. It is doubtful whether the fright the boy received had any influence in accelerating the appearance of the symptoms; no doubt the affection would have shown itself independent of this; but in a disease where the susceptibility of the nervous system is so intense, any shock must have a hurtful effect, and may give the first impulse to the development of the symptoms. The convulsions in this case were very severe during the last ten hours; they were strongest in the muscles of respiration, and in those of deglutition, which at first were the only ones affected. They resembled those of epilepsy, the efforts at expiration being strong, frequent, and ineffectual, while the face and neck were much congested.

The post-mortem appearances were such as could only be considered as secondary consequences of the disease. Those met with in the lungs, bronchi, and trachea,—viz., congestion, ecchymosis, and emphysema, are those most constantly found both in man and animals that have died of the disease, and correspond in frequency to one of the most constant of all the symptoms,—viz., convulsions of the muscles of respiration, to which they may fairly be attributed. The existence of frothy mucus in the trachea and bronchi after death, and the circumstance that the flow of frothy fluid from the mouth, commenced only after the convulsions had become severe and constant, would seem to point to the lungs and air passages as the source of that fluid—the *bave*—which collects in the pharynx and back of the mouth, and is hawked up by the patient; and in which, and not in the saliva, the virus has been found most active. There was no cough throughout the whole course of the disease, the only approach to it being short and continued hawking. Inflammation or congestion of the pharynx and œsophagus, which has been noticed in some cases, was entirely absent, the mucous membrane being quite natural and free from any unusual moisture. The most uncommon appearance met with was the frothy state of the blood in the right cavities of the heart. This has been noticed by two observers only—Morgagni and Trolliet, the latter of whom found it present in three out of six cases, and in some of these it existed in the left as well as in the right side of the heart.

The nervous centres and roots of the nerves have in some cases been found much congested or inflamed; but here there was absolutely nothing which could indicate that the disease was other than a purely functional affection of the nervous system. The eighth pair of nerves, which some have supposed to be specially concerned in this disease, were carefully examined in the neck and at their origin, along with all the parts of the medulla oblongata; and all of us present at the post-mortem examination, being familiar with the appearance of these parts, were unanimously of opinion that we had never seen them with less appearance of disease.

ARTICLE V.—*Case of Popliteal Aneurism.* By JAMES SYME, Esq., Professor of Clinical Surgery in the University of Edinburgh.

T. M., aged 35, a seaman, sent from Banff by Dr Milne, to be under my care, on account of an aneurism in the left popliteal artery, was admitted into the hospital on the 18th of November. The tumour filled the popliteal space, and pulsated strongly. It was first noticed about six months before, by the patient, after lying one night with his leg hanging over the edge of his hammock. He had been either at sea, or actively engaged in harbour duties, ever since, and always remarked that exertion was followed by an increase of pain. With exception of occasional rheumatic pains, he had enjoyed good health.

Having come so far, nearly 200 miles, the patient was kept quiet in bed, with restricted diet and gentle aperients, to prepare him for the operation. In the course of a day or two, the aneurismal pulsation became much less distinct, and could not be felt at all on the 23d, when the tumour was also observed to be greatly reduced in size, and no longer the source of any uneasiness. On the 9th of December, the patient being completely relieved from the disease, proceeded homewards.

A case precisely similar to the one just related, occurred under my care some years ago. The patient having come over from Kirkcaldy in Fife, where he had pursued his occupation as a weaver until the day he was admitted into the hospital, when the perfect rest there afforded was immediately followed by coagulation and the other steps of a spontaneous cure. These facts seem to suggest a suspicion that the one or two very rare instances of pressure being quickly followed by recovery, may really have owed the beneficial change to assistance of the *vis medicatrix*, from rest in the horizontal posture, and not to the effect of compression. Certain it is, that if the two cases just mentioned had been subjected to pressure, they would have appeared triumphant examples of its successful employment. The Dublin writers allege that no confidence can be placed in trials of pressure on this side of the Irish Channel, accounting for all its failures and bad consequences by attributing them, with characteristic frankness, to prejudice and ignorance on the part of the operators. Well aware of this peculiarity in the estimation of evidence, I have calculated the average length of time requisite for the duration of pressure, from the facts supplied by the advocates of this system. In twenty-three cases of aneurism, reported by Dr Bellingham, from the practice of seventeen surgeons, as successfully treated by pressure, I find that the average duration, not of the treatment, but of the actual compression, excluding the intervals of its discontinuance, amounted to thirty-eight days. Thirty-eight days and nights of misery, to escape a few minutes of trivial uneasiness!

If the cases treated in the Dublin hospitals since the publica-

tion of Dr Bellingham's work, have been less protracted than those he has recorded, it would be well to acquaint the profession with them. But if this be done, I trust that the results of *all* the cases will be given, together with a full statement of the means required to render the patients able to endure the prolonged torment of compression.

ARTICLE VI.—*On Leucocythemia, or Blood containing an unusual Number of Colourless Corpuscles.* By JOHN HUGHES BENNETT, M.D., F.R.S.E., Professor of the Theory of Physic and of Clinical Medicine in the University of Edinburgh.

ON the 19th of March 1845, I examined the body of a man, who died under the care of Professor Christison, in the Royal Infirmary, labouring under hypertrophy of the spleen and liver, and whose blood was crowded with corpuscles which exactly resembled those of pus. The case was published in the "Edinburgh Medical and Surgical Journal," for October 1845, under the title of "Case of Hypertrophy of the Spleen and Liver, in which Death took place from Suppuration of the Blood." Dr Craigie, who was present at the dissection, recognised its similarity to one he had had under his care four years previously, the blood of which had been examined microscopically by Dr John Reid, who found "that it contained globules of purulent matter and lymph." It was from the January number of Schmidt's "Jahrbücher," for 1848, I first learned that Virchow had dissected a body in the Charité Hospital of Berlin, on the 1st of August 1845, in which the blood examined microscopically presented the same increase of colourless corpuscles, also associated with enlargement of the spleen. This case was originally published in the 780th Number of Froriep's "Notizen," under the name of white blood, or "Leukhemia." On the 31st of December 1845, a man was received into St George's Hospital, London, in whom Dr Fuller detected, both before and after death, the increased number of colourless corpuscles in the blood. This man, like the other individuals, had great hypertrophy of the spleen. A notice of the case is inserted in the "Lancet," for July 1846. Since this, several similar cases have been published, in which this morbid condition of the blood has been determined to exist, by an accurate examination with the microscope; and a reference to the records of medicine has shown the previous occurrence of like cases. In these, it is true, the blood was not physically proved to contain an unusual number of colourless corpuscles, although now, on looking back upon the facts which are mentioned in regard to them, we can have little doubt that such was the case.

The term "Leukhemia," or white blood, given to this disease by

Virchow, is objectionable, because, in the first place, as is correctly stated by Dr Parkes, the blood is not white, but presents its usual red tinge when drawn from the arm. The colourless clots occasionally observed, will certainly not warrant the application of this term to the blood generally, as they are frequently present without the morbid condition under consideration. Besides, the same name (white blood) has been given with more propriety to the fatty blood, examined by Drs Traill, Christison, and others, which presents a milky, opalescent appearance. What is required to be expressed is, that the blood abounds in colourless corpuscles, and this is, I think, done by the term Leucocythemia—from λευκὸς, white; κυτος, cell; and αἷμα, the blood—literally, white cell blood, which expresses the simple fact, or a pathological state, and involves no theory.

I propose, in the first part of this paper, to bring together all the cases I have been able to collect of this newly discovered disease; and, in the second part, to give a systematic account of what is known of its symptoms, pathology, and treatment. The cases may be arranged in two divisions,—1st, Undoubted cases, in which the Leucocythemia was proved to exist by a microscopic examination; 2d, Probable cases, in which, though the blood was not examined physically, it still presented such appearances as to warrant our deriving from them considerable information, in drawing up a systematic account of the disease. To these I shall add, thirdly, a few cases for the purpose of showing that the spleen may be greatly enlarged, without necessarily occasioning the peculiar condition of the blood under consideration.

PART I.

I.—CASES OF LEUCOCYTHEMIA, IN WHICH THE DISEASE WAS POSITIVELY PROVED TO EXIST BY MICROSCOPIC EXAMINATION.

The three following cases are those which have been under my own observation. In the first one, the blood disease was only discovered after death; in the other two, it was proved to exist by a physical examination of the blood during life.

CASE I.[1]—*Leucocythemia discovered after Death; Hypertrophy of the Spleen, Liver, and Lymphatic Glands.*

John Menteith, aged 28, a slater, admitted into the clinical ward of the Royal Infirmary, February 27, 1845, under the care of Dr Christison. He is of dark complexion, usually healthy and temperate; states that twenty months ago he was affected with great listlessness on exertion, which has continued to this time. In June last he noticed a tumour in the left side of the abdomen, which has gradually increased in size till four months since, when it became stationary. It was never painful till last week, after the application of three blisters to it; since then, several other small tumours have appeared in his neck, axillæ, and groins, at first attended with a sharp pain, which has now, how-

[1] Originally published in the Edinburgh Medical and Surgical Journal. October 1845.

ever, disappeared from all of them. Before he noticed the tumour, he had frequently vomiting in the morning. The bowels are usually constipated, appetite good, is not subject to indigestion, has had no vomiting since he noticed the tumour; he has used chiefly purgative medicines, especially croton oil; employed friction with a liniment, and had the tumour blistered. On admission, there is a large tumour, extending from the ribs to the groin, and from the spinal column to the umbilicus, lying on the left side. It is painful on pressure near its upper part only. Percussion is dull over the tumour; pulse 90; states that for three months past he has not lost in strength. There is slight œdema of the legs. *To have two pills of iodide of iron morning and evening.*

March 1.—Urine of yesterday somewhat turbid when just passed, natural in colour, acid to litmus; sp. gr. 1013. Sediment presents cubic crystals under the microscope, disappears almost entirely on the addition of aqua potassæ, but is unaffected by nitric acid. The filtered urine is not affected by aqua potassæ, and yields only a slight white haze when boiled.

March 9.—Œdema of legs increased. They have been bandaged with flannel rollers. ℞. *Potassæ Carbonatis*, ʒi.; *Spiritus Ætheris Nitrici*, ʒiv.; *Aquæ Menthæ*, ʒiij.; *Aquæ fontis*, ʒij. M. *Sumat unciam ter in dies.*

March 10.—Tormina and considerable diarrhœa; urine not increased. *Habt. Haustum ex Olei Ricini*, ʒss. *statim; et exactis quatuor horis, Opii*, gr. ii.

March 13.—Attacked this morning with heat of skin; thirst; pulse 110, full, very compressible. The diarrhœa, which had been checked, returned yesterday; none this morning, after taking an opium pill. Urine, 100 ounces. *Omittantur medicamenta. Sumat statim Pulveris Ipecacuanhæ et Opii*, gr. x., *et repetatur dosis singulis semihoris ad tertiam vicem.*

March 14.—No sweating from the powders; diarrhœa still rather troublesome; pulse, 100, softer; tongue, dry and brown; febrile expression of countenance, resembling that of typhus. ℞. *Aquæ Acetatis Ammoniæ*, ʒvi.; *Solutionis Morphiæ*, ʒi.; *Aquæ fontis*, ʒiij.; *Syrupi*, ʒj. *Sumat unciam quartâ quâque horâ. Habt. Decoctum Hordei pro potu.*

March 15.—Died suddenly in the morning.

Sectio Cadaveris.—March 19 (*four days after death*).—Externally, the body presented a considerable prominence of the ensiform cartilage and false ribs on both sides. The abdomen was contracted; considerable dulness on percussion on left side, which had previously been marked out by a line formed with nitrate of silver. No ascites nor œdema of the limbs.

Blood.—The blood throughout the body was much changed. In the right cavities of the heart, pulmonary artery, venæ cavæ, vena azygos, external and internal iliac veins, and many of the smaller veins leading into them, it was firmly coagulated, and formed a mould of their size and form internally. In the cavities of the heart and venæ cavæ, the blood, when removed, was seen to have separated into a red or inferior, and a yellow or superior, portion. The red portion was of a brick-red colour; it did not present the dark purple smooth and glossy appearance of a healthy coagulum, but was dull and somewhat granular on section, and when squeezed readily broke down into a grumous pulp. The yellow portion was of a light yellow colour, opaque and dull, in no way resembling the gelatinous appearance of a healthy decolorised clot. When squeezed out of the veins, as was sometimes accidentally done where they were divided, it resembled thick creamy pus. In some portions of the veins, the clot was wholly formed of red coagulum. In others it was divided into red and yellow. In a few places the yellow formed only a streak or superficial layer upon the red, or covered the latter with spots of various sizes. Whether this coagulum existed in all the veins could only have been ascertained by a complete dissection of the body. It was seen, however, that the femoral veins, after passing under Poupart's ligament, were empty and perfectly healthy as far down as the Sartorius muscle. The external and internal iliac

veins, as well as the pelvic veins, were full and distended. The azygos, both axillary and jugular veins, were full, also the longitudinal, the lateral, and other sinuses at the base of the cranium, and veins ramifying on the surface of the brain. In this last situation some of the veins appeared as if full of pus, whilst others were gorged with a dark coagulum—(see the coloured plate, which will be published in the next Number of the Journal). In the aorta and external arteries were a few small clots, resembling those found in the veins. These vessels, however, were comparatively empty. The basilar artery at the base of the brain was distended with a yellow clot.

Vessels.—The arteries and veins themselves were perfectly healthy. Although carefully looked for, in no place could thickening or increased vascularity be observed. Nowhere was the clot adherent to the vessels, but, on the contrary, it readily slipt out when an accidental puncture was made in them.

Head.—On removing the dura mater, the veins which empty themselves into the longitudinal sinus were considerably engorged, especially posteriorly. Some were filled with the red and others with the yellow clot previously described. Others, again, were half filled with red and half with yellow coagulum, the passage of the one into the other being clearly perceived. Both hemispheres, with the longitudinal sinus and *falx in situ*, were removed by a section across the brain, as low down as the division of the cranium would permit. The brain was then discovered to be very soft uniformly,—a circumstance accounted for by the time which had elapsed since death. The part removed was put aside, in order to be preserved and hardened in spirit. The lateral ventricles were found healthy, contained no serum, and the choroid plexus was perfectly normal. At the base of the brain the basilar artery was seen distended with the yellow coagulum, as were also a few of the arteries, but to a very slight extent. The substance of the brain itself was throughout healthy. All the sinuses at the base of the cranium gorged with the red coagulum.

Chest.—A few chronic adhesions united the pleuræ on both sides, which were easily torn through. Both lungs were slightly engorged posteriorly and inferiorly. The anterior margin of the left lung emphysematous, but to no great extent. On section, the yellow coagulum of the blood was observed to occupy all the ramifications of the pulmonary artery. In some places it was so consistent as to be drawn out, exhibiting an arborescent form ; in others, it was more soft, and exuded from the cut surface like thick pus. Heart somewhat enlarged ; weighed, when freed from coagulum, eleven and a-half ounces. Its texture was healthy ; the valves normal. The right auricle much distended, and gorged with a firm coagulum, the upper third of which was found composed of the yellow, and the two inferior thirds of the red clot formerly described. The right ventricle and pulmonary artery were similarly distended ; portions of the clot closely embraced the columnæ carneæ, but were in no place adherent. The coronary arteries and veins were normal.

Abdomen.—On the inferior surface of the diaphragm there existed a firm, almost cartilaginous, deposit about a line in thickness, of a white colour, oval form, two inches long by one and a-half broad, with irregular margins, which were composed of several rounded tubercular bodies, the size of a small pea, and of a similar structure. The liver enormously enlarged from simple hypertrophy. Its structure throughout healthy. Gall-bladder enlarged, and distended with a clear pale yellow bile. The whole weighed ten pounds twelve ounces. The spleen also enormously enlarged from simple hypertrophy. It was of a spindle shape, largest in the centre, tapering towards the extremities. It weighed seven pounds twelve ounces. It measured in length fourteen inches ; in breadth, at its widest part, seven inches ; and in thickness, four and a-half inches. Towards its anterior surface was a yellow firm exudation, about an inch deep, and three inches long. The peritoneum, also covering a portion of its anterior surface, was thickened, opaque, and dense over a space about the size of the hand. Both kidneys healthy. The stomach and intestines healthy through-

out. About four inches from the anus the superior hemorrhoidal veins were distended on both sides external to the rectum. They formed two chains of tumours about three inches long, consisting of, on the one side, three swellings as large as a walnut; on the other, of one swelling somewhat larger. They were filled with a red coagulum, broken down into a grumous mass. The lymphatic glands were everywhere much enlarged. In the groin they formed a large cluster, some being nearly the size of a small hen's-egg, and several being that of a walnut. The axillary glands were similarly affected. The bronchial glands were not only enlarged, but of a dark purple colour, and in some places black from pigmentary deposit. The mesenteric glands were of a whitish colour, some as large as an almond nut. A cluster of these surrounded and pressed upon the ductus communis choledochus. The lumbar glands were of a greenish yellow colour, also enlarged, forming a chain on each side, and in front of the abdominal aorta, more especially at its bifurcation into the iliacs.

No collection of pus could be found in any of the tissues.

Microscopic Examination.—The yellow coagulum of the blood was composed of coagulated fibrin in filaments, intermixed with numerous colourless corpuscles, which could be readily squeezed out from it when pressed between glasses. Where the yellow coagulum was unusually soft, the corpuscles were more numerous, and the fibrin was broken down into a diffluent mass, partly molecular and granular, partly composed of the debris of the filaments broken into pieces of various lengths. The corpuscles varied in size from the $\frac{1}{80}$th to the $\frac{1}{120}$th of a mill. in diameter; they were round, their cell-wall granular, and presented all the appearance of pus corpuscles. (Fig. 1.) Water caused them to swell and lose their granular appearance, and acetic acid dissolved the cell-wall and caused a distinct nucleus to appear. This nucleus was composed sometimes of one large granule about the $\frac{1}{200}$th of a mill. in diameter, at others of two or three smaller granules, as is seen in corpuscles of laudable purulent matter. (Fig. 3.) The red portion of the coagulum contained a smaller number of these colourless corpuscles, mixed with a multitude of nor-

Fig. 1. Fig. 5.

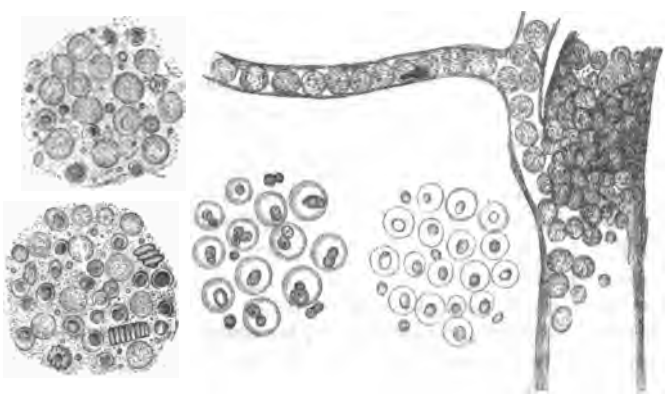


Fig. 2 Fig. 3 Fig. 4.

Fig. 1.—Colourless corpuscles, mingled with a few coloured ones, from the white clot of the blood in Case I.

Fig. 2.—The same bodies, mingled with a larger number of yellow blood corpuscles, in the red clot.

Fig. 3.—Change produced in the colourless corpuscles on the addition of acetic acid, the yellow corpuscles being dissolved.

Fig. 4.—Cells in the fluid squeezed from the lymphatic glands, after the addition of acetic acid.

Fig. 5.—Blood-vessel giving off a capillary from the pia mater; the latter is seen filled with colourless corpuscles; the former partly with colourless, mingled with coloured, corpuscles. 250 diameters.

mal yellow corpuscles. (Fig. 2.) The colourless corpuscles now described were found in the blood throughout the system. They were seen in the veins and arteries ramifying on the brain, in the coronary veins, hemorrhoidal tumours, and wherever the blood was examined. On stripping off a portion of the pia mater, and examining the capillary vessels of that membrane, all that were not too minute to contain them were found crowded with the same corpuscles. (Fig. 5.) This fact was confirmed by Dr Allen Thomson, to whom I sent a portion of the brain for that purpose.

The cartilaginous deposit on the inferior surface of the diaphragm was composed of dense fibrous tissue, in which numerous granules and molecules were observed. The exudation in the spleen was composed of amorphous fibrin mixed with numerous molecules, granular and imperfect cells. These were intermingled with bundles of filamentous tissue. The enlarged lumbar glands, on being pressed, exuded a fluid that was crowded with corpuscles; some resembling the colourless corpuscles already alluded to; others oval and round, containing a distinct nucleus. (Fig. 4.)

The ultimate textures of the muscles, brain, nerves, &c., were carefully examined, and found normal.

Remarks.—The enormous size which the liver and spleen had attained in this case, caused apparently no great inconvenience. There was only slight œdema of the legs without ascites. Diarrhœa appeared on the 10th, and was present during the subsequent progress of the case. The most important symptom, however, seemed to be the fever which occurred on the 13th, and continued until his death, on the 15th. At the same time, it cannot be said to have been very severe, and it was unattended by prostration of strength. The post-mortem examination was conducted with the greatest care, for the appearances at the time were quite new to me. Nowhere could the slightest traces of phlebitis be discovered; for, notwithstanding the blood was firmly coagulated in the venous system, so far from being adherent to the vascular walls, on making an opening it slipped out from them with the greatest facility. Neither could abscesses, or purulent infiltration of any tissue, be discovered. No observations having been made on the blood during the life of the individual, it was impossible to say how long that fluid had contained the excess of colourless corpuscles which were found in it after death; but I believed at the time that they were formed towards the close of life, and probably when fever became manifest. A similar conclusion was arrived at by Dr Craigie, from his observation of this case, as well as of the one afterwards to be detailed. (Case IV.) That such conclusion was erroneous, however, will be made evident from several other examples of the disease, but especially from the next to be recorded.

CASE II.—*Leucocythemia detected during Life; Hypertrophy of the Liver; Ascites; Phthisis Pulmonalis.*

Barney Tinlay, æt. 17, farm-servant, admitted into the clinical ward of the Royal Infirmary, January 25, 1850. With the exception of an attack of scarlet fever, which he experienced about three years ago, he enjoyed perfect health until twelve months since, when he first noticed a swelling in the abdomen, accompanied by some pain. The tumour since this period has gradually increased in size, and

latterly he has been unable to walk fast on account of dyspnœa. For the last two or three years he has been employed in farm-service, and during three months last summer he resided in the fenny district of Lincolnshire, but never had intermittent fever. On admission, his complexion is pale, the conjunctivæ are unusually blanched, and his whole appearance is very cachectic. On examining the abdomen a hard tumour can be felt occupying the whole left side. Superiorly, it can be felt emerging from the false ribs about two inches to the left of the ensiform cartilage. It then passes downwards an inch and a-half to the right of the umbilicus, and curves round inferiorly to a point about an inch and a-half above the symphysis pubis, from which it may be traced directly backwards to within three inches of the spinous processes of the lumbar vertebræ. Its anterior margin presents a semicircular convexity, which is smooth, with a distinct notch in its upper third, and is apparently about half an inch thick, as with the fingers a fold of integument may be pressed somewhat below it. The tumour is completely dull on percussion throughout, and is in several parts painful on pressure. It measures about ten inches in the long diameter, and thirteen and a-half transversely. The rest of the abdomen has the usual tympanitic percussion, and there is no fluctuation. Liver, on percussion, found to be of the natural size. Tongue clean; appetite good. There is profuse diarrhœa, the bowels being open eight or ten times a-day; this symptom has existed for the last three or four weeks. Pulse 80, weak. Complains of giddiness on assuming the erect posture. Heart sounds natural. He has occasional epistaxis and hemorrhage from the gums. Respiratory and urinary systems healthy.

The appearance of the blood drawn from the extremity of the finger, when magnified 250 diameters linear, is represented Fig. 6. The coloured corpuscles for the most part have collected together in rolls, the numerous colourless corpuscles filling up the intervening space. Acetic acid dissolved the coloured bodies, and rendered the cell-wall of the colourless ones very transparent—bringing into view the nucleus, consisting of a single round or oval body in some, but in the majority presenting two, three, or even four granules, each having a depression in its centre. Here and there the nucleus was crescentic, or in the form of a horse-shoe. (Fig. 7.)

Fig. 6. Fig. 7. Fig. 8.

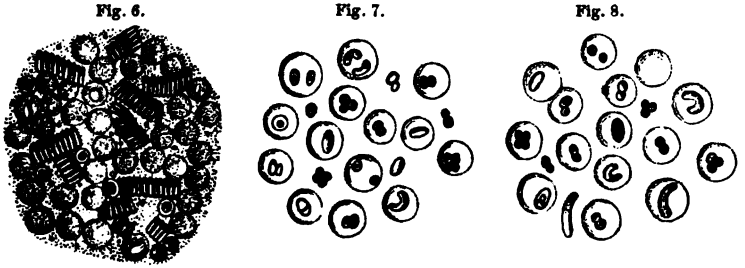


Fig. 6.—Appearance of a drop of blood, magnified 250 diameters, taken from Tinlay's finger.
Fig. 7.—The same, after the addition of acetic acid.
Fig. 8.—A drop of blood, treated with acetic acid 24 hours after being taken from the arm by venesection.

January 27th.—He has had eight leeches applied to the epigastrium, and has taken the lead and opium pills—one three times a-day. The diarrhœa is much diminished, and there is less pain. ℞. *Ferri Citratis*, ʒi.; *Tinct. Card. Comp.*, ʒi.; *Infusi Calumbæ*, ʒvii. *M. fiat Mistura cujus Sumat*, ʒi. *ter in die.* ℞. *Sulph. Quinæ*, gr. iij.; *Pulv. Catechu extr.*, gr. iv.; *Ol. Carui* m. i. *M. fiant pil.*, ij. *Mittantur tales*, xij., *et Sig.*, *Sumat* ij., *Mane et Vespere.*

January 30th.—Diarrhœa now entirely ceased. ℞. *Pulv. Quinæ Sulph.*, gr. iij.; *Ferri Carb. Sacch.*, gr. vij.; *M. fiant pil.*, ij.; *Mittantur tales*, vi.; *quarum Sumat* j., *ter in die. Intermittantur alia.*

Yesterday three ounces of blood were taken from the arm, which Dr Robertson was so good as to analyse. The results are as follows:—

Sp. Grav. of Blood, ...	1041·5
Sp. Grav. of Serum, ...	1026·5
Composition of 1000 *parts*—	
Fibrin, ...	6
Serous Solids, ...	72
Globules, ...	67·5
Total Solids, ...	145·5
Water, ...	854·5
	1000·0

The analysis was conducted on nearly the same plan as that recommended by Dr Christison, and subsequently adopted by Andral and Gavarret; but it is believed that the fibrin is more exactly estimated than by the process of the latter authorities. The sp. grav. of the blood and serum were very accurately taken, and the errors of manipulation cannot exceed ·5 per 1000 in each constituent.

The blood allowed to remain in a vessel for twenty-four hours presented a large and firm clot. Examined microscopically, it exhibited the same appearance as in Fig. 6—the rolls of coloured bodies not being so large. On adding acetic acid the same kind of nuclei were observed; but they were now tinted of a deep yellow colour, having apparently imbibed colouring matter dissolved in the serum. Some of the crescentic nuclei had become nearly straight. (Fig. 8.)

February 2d.—Urine is observed to be loaded with lithates, and diarrhœa has returned. *Recipiat Pil. Plumbi opiat.* j. *ter in die, et Syrupi Iodidi Ferri guttas*, xv., *ter in die ex aqua. Illinatur Tinct. Iodini parti dolenti. Intermittantur alia.*

February 11th.—Tumour increased in size, and dyspnœa aggravated. Diarrhœa has again disappeared.

February 26th.—Marked improvement in the expression of his countenance. No increase in size of tumour, which slightly shifts its position when patient changes from one side to the other in bed. Less pains and dyspnœa than formerly. There is still slight diarrhœa, but no epistaxis, and the bleeding from the gums has diminished.

March 1st.—The urine is scanty, and there is some ascites. Sweats occasionally at night. ℞. *Sp. Æth. Nitr.*, ʒvj.; *Aquæ font.*, ℥vj. *M. fiat Mistura, cujus sumat*, ℥ss. *ter in die.*

March 24th.—During the last few weeks the hemorrhage from the nose and gums has continued to recur, and the ascites is not abated. He was ordered an astringent lotion for his gums. Since the 11th, Spongio-piline, with Tr. of Digitalis, has been applied to the abdomen, which apparently, in consequence, is less tense, while the pain has undoubtedly diminished.

April 2d.—Cough increased. Complains of severe pain and tenesmus, for which he was ordered an opium suppository every evening.

April 8th.—Diarrhœa has recurred. Stools very fluid. He suffers also from cough, and there is harshness of respiratory murmurs, and prolonged expiration to be heard at the apices of both lungs. No dulness on percussion. *Recipiat Pil. Plumbi opiat.*, j. *ter in die.*

April 13th.—Pain in abdomen, and diarrhœa nearly gone. Sweats profusely at night. ℞. *Quinæ Sulph.*, gr. xij; *Acidi Sulph. dil.*, ℥j.; *Syrupi Aurant.*, ℥j.; *Aquæ font.*, ℥v. *M. ft. Mistura, cujus sumat*, ℥ss. *ter in die. Repetantur Pil. Plumbi opiat.*

April 15th.—Sweating diminished; diarrhœa ceased; cough most severe at night; no dulness on percussion at the apices of the lungs, but the respiratory murmurs are harsh, especially in the right side, with loud moist râles on inspiration. Expectorates muco-purulent matter.

April 23d.—The diarrhœa has ceased for the last eight days, but to-day has returned with considerable pain. *Intermittatur Mist. Quinæ Sulph.; Applicetur Emplastrum Opii 4 × 4 Abdomini; Recipiat Pil. Plumb. opiat.*, ij., *ter in die*; *Utatur Enem. Amyli cum Sol. Morph. statim, et Suppositorio Opii quotidie h. Somni.*

April 30th.—Diarrhœa much abated, but pain in abdomen continues. Girth of abdomen at this period was thirty-seven inches.

May 5th.—Has complained a good deal lately of nausea and vomiting, for which he has been ordered a draught every evening, with naphtha. He has also taken the squill and digitalis pill three times a-day, and had the tumour fomented with infusion of digitalis. The distension of abdomen has increased. ℞. *Sp. Æth. Nitrici*, ʒiss.; *Aquæ Potassæ; Sol. Mur. Morph. a. a.*, ʒij. *M. Sig. Sumat*, ʒj. *ex aqua ter in die.*

May 7th.—Distension of abdomen from accumulation of fluid still increases, and pain continues. The urine presents an acid reaction, and is loaded with a copious sediment of lithate of ammonia, with a few colourless rhomboidal crystals of lithic acid. ℞. *Pulv. Scillæ*, ℈j.; *Pulv. Digitalis*, gr. x.; *Extr. Hyoscy.*, ʒss.; *Cons. Rosarum, q. s., ut fiant pilulæ*, xx.; *Sig. Sumat*, j. *ter in die.*

May 13th.—Since last report diarrhœa has been very profuse, the bowels having been acted upon sometimes twenty times in the course of a night. Stools very loose but feculent. Spongio-piline with digitalis to be discontinued, on account of its pressure causing uneasiness. He has taken the lead and opium pills four times a-day, as well as the starch injection, with Sol. Mur. Morph. at night. *Omittantur pil. Plumbi opiat.* ℞ *Tanini*, gr. xv.; *Pulv. Opii*, gr. vj.; *Cons. Rosarum, q. s., ut fiant pil.* vj.; *quarum Sumat*, j. *sexta q. q. hora.* ℞ *Acidi Nitrici dil.; Syrupi, a. a.*, ʒss.; *Aquæ fontanæ*, ℥j. *M. et Sig. Sumat*, ʒj. *ter in die ex aqua.*

June 1st.—Since last report the diarrhœa has continued, but is now much abated. Fluctuation in abdomen evidently diminished. There has been occasional slight epistaxis. Still sweats at night. Girth of abdomen at broadest part diminished to thirty-five and a-half inches. *Intermit. Mist. Quinæ.*

June 12th.—Has progressed favourably to this date; the tumour and ascites continue to diminish; and the diarrhœa and other symptoms having abated, the abdomen is now flaccid, and the skin is cracked similar to what is observed in a woman after pregnancy. To-day the diarrhœa has returned with some violence, with abdominal pain. *Habeant Pil. Tanini ut antea.*

June 18th.—Diarrhœa has continued up to this date, but has again abated. Dulness under right clavicle, with increase of the vocal resonance, harsh inspiratory murmur, and prolonged expiration. Cough diminished, but still mucous expectoration. His appearance is emaciated, and he complains of weakness. *Habeat. Ol. Morhuæ*, ʒij., *ter in die.*

June 26th.—Diarrhœa still more diminished. There is a good deal of cough, with some expectoration, and harsh respiration is heard under clavicles, with increased vocal resonance. His general strength is now greatly improved. He sits up the greater part of the day, and even walks about on the green. His amendment is so great that he is very anxious to return to his parents, who reside in Hull.

August 7th.—Since last report the diarrhœa has returned at intervals, and still continues to be troublesome. On the whole, however, his health has improved; his appetite and strength have increased, and all ascites nearly disappeared. The cough and expectoration have ceased. The tumour measures transversely thirteen and a-half inches, and longitudinally fifteen inches. From the lower border of ribs to inferior margin of tumour, ten inches. The circumference of the abdomen at the widest part (a little above the umbilicus), is thirty-four inches.

He was now dismissed, having for some time expressed great impatience to return to his friends in Hull.

Remarks.—This boy was in the Infirmary upwards of six months, and the symptoms and entire progress of the case were watched with the greatest care. Unlike the former case, the spleen was the only organ enlarged, the liver presenting its normal dulness on percussion. The abdomen, however, was also the seat of ascitic distension. The smallest drop of blood taken from the boy's finger exhibited, during the whole of his residence in the Infirmary, the excess of colourless corpuscles, and the number of these underwent no perceptible increase or diminution, notwithstanding the varieties of treatment to which he was subjected. Owing to the theories which have been from time to time advanced regarding the functions of the two kinds of corpuscles found in the blood, and of the nature of its colouring matter, iron was the drug which seemed indicated. This boy had also lived in the fenny districts of Lincolnshire; and, although he denied ever having been affected with intermittent fever, it seemed very probable that the enlargement of the spleen was owing to this cause. I commenced the treatment, therefore, with the exhibition of iron and quinine. Other symptoms, however, became so urgent as to demand special attention, and the suspension of these remedies. I allude to the diarrhœa and dyspnœa, the former of which constituted the leading symptom of the disorder during the entire period he remained in the house. All kinds of astringents were given, with occasional temporary, but never with permanent, advantage. At one period he was so exhausted that I expected his death daily for a period of some weeks. He, however, again gained strength; and his bodily powers, except towards the termination of his residence in the house, were subject to considerable variations, evidently dependent on the amount of diarrhœa.

In April, pulmonary symptoms were added to his other complaints; and from the character these presented, as well as from the physical signs, there is every reason to believe that he laboured under phthisis pulmonalis. Under a tonic treatment, with cod-liver oil, assisted by the advance of summer, these symptoms diminished, and his general strength was so improved that, as is stated in the report, he insisted on going home. At the time of his discharge he was remarkably ill, greatly emaciated, and cachectic-looking, with an enormous abdomen,—so that it was only by comparison with what he had been that he could be said to enjoy tolerable strength. It seems, however, that he reached Hull, by the steam-vessel, in safety.

Dr Robertson was kind enough to analyse the blood for me in this case, and from the results he obtained it appears, that the fibrin was increased to about double its amount in healthy blood. The albumen and salts existed in their normal proportion. The globules were diminished to about one-half their proper amount, which deficiency was counterbalanced by an increase in the amount of water.

This combination of increase in the amount of fibrin and diminution in the amount of corpuscles, indicates a condition of the blood which, so far as I am aware, is not peculiar to any other morbid state of the economy. The diminution of the corpuscles, however, seems to be the chief alteration; for in the following case, whilst this was well-marked, the normal amount of fibrin was preserved. The interesting case now to be given entered the wards of Dr Robertson, to whom I am indebted for an account of the symptoms which presented themselves whilst under his care. The account of the post-mortem examination is drawn up partly from my own observation, and partly from the record of the pathologist of the Infirmary, Dr Gairdner:—

CASE III.—*Leucocythemia detected during Life and after Death; Hypertrophy of Spleen, Liver, and Mesenteric Glands; Purpura Hemorrhagica; Bronchitis.*

Janet Mackay, aged 53, for some years past a washerwoman in Edinburgh, was admitted on the 18th July 1850, into the Royal Infirmary, and placed in ward 13, under the charge of Dr Robertson. She stated, that her health had been uniformly good till the end of March 1850, when she became subject to fits of vomiting, which usually soon succeeded a meal; that about the end of May she observed some swelling of her feet and ankles; that she continued to follow her usual occupation till the 11th of July, when she expectorated a large quantity of blood. The hæmoptysis had since that time frequently recurred, and she had been for some days aware of the existence of bloody spots upon the skin of both trunk and extremities. Her habits had been regular and temperate, and her food (which always included some portion of animal food in the course of each day) seems to have been deficient neither in quantity nor in quality.

State on Admission.—The patient has a sallow and slightly jaundiced complexion; is considerably emaciated; and has lost much of her muscular force. The skin of the body and extremities is marked with spots of *purpura hemorrhagica*, of various sizes,—the largest not exceeding three lines in diameter. The respiration is hurried, but performed without much effort; there is a good deal of cough, and tough bloody sputa are frequently expectorated. The pulse is 120, feeble. The tongue is brown and moist; the gums are spongy, and bleed on pressure. The patient is thirsty, and has little desire for food.

The bowels are habitually constipated, and evacuations are seldom procured without the use of purgatives. The urine is secreted abundantly; its density is 1016; its reaction acid—not coagulable by heat, or on the addition of nitric acid. When this acid is added in successive drops, a deep green colour, rapidly changing to a violet, is produced.

The patient is intelligent, says she suffers little, and is inclined to sleep. No medical treatment has been applied before admission. The purgatives above mentioned were not prescribed by a medical man.

Physical Examination.—Some dulness on percussion and crepitant râle at dependent portions of both sides of the chest. Coarser râles in vicinity of the large bronchial tubes. Heart's sounds normal. Great enlargement of both liver and spleen, indicated by palpation and percussion.

A drop of blood, examined microscopically, was at once seen to contain a most unusual quantity of white corpuscles. This observation was verified by Dr Bennett, and other experienced observers. The corpuscles exactly resembled those seen in the case of Tinlay.

A small quantity of blood—about ʒiij.—taken from the arm, presented the following characters:—a small, easily lacerable coagulum, and a large propor-

tion of slightly milky serum; no buffy coat. It was analysed by Dr Robertson, with the following results :—

Specific gravity of the blood,				1036
Ditto ditto serum,				1023
1000 Parts contained :—				
Fibrin,	2.3			
Serous solids,	67·0	Organic,		55·8
		Inorganic,		11·2
Globules,	49·7			
Total solids,	119·0			
Water,	881·0			
	1000·0			

The abstraction of the blood gave some relief, and the patient soon fell asleep. Symptoms of extreme debility, and recurrence of the hemorrhage from the mouth, were treated by the use of stimulants, of tannin, and of turpentine, given in small doses by the mouth, and more liberally in the form of enema. The patient sank, and died on the 22d July.

Sectio Cadaveris.—July 25.—The body was generally emaciated, and there were purpura spots scattered over the whole surface.

The Blood and Blood-Vessels presented nothing unusual to the naked sight. If anything, the blood was unusually fluid in all the large venous trunks, and the clots in the heart were small, soft, and easily broken down under the fingers. I examined the portal system of veins with great care, and found them perfectly healthy, containing only a moderate amount of fluid dark blood.

The Head was not examined.

Thorax.—There were three ounces of clear serum in the sac of the pericardium, and both serous surfaces were covered with ecchymosed spots. The heart was small and pale. The valves healthy. There was a slight atheromatous deposit in the arch of the aorta. The lungs were firmly adherent, especially on the left side; their substance œdematous, otherwise healthy. Bronchial glands enlarged.

Abdomen.—Liver and spleen adherent. Spots of purpura irregularly scattered over the peritoneum, especially over the linings of the abdominal walls and pelvic viscera. The liver weighed 12 lb. 5 oz. On section, it presented an extremely variegated appearance, being mottled throughout with irregular purplish-brown patches, and resembling an exaggerated form of the nutmeg liver, except in the greater diffusion and extent of the brown patches. The tissue of the organ was unusually soft. The spleen was much enlarged, and weighed 2 lb. 5 oz. The surface, on section, had a mottled appearance; its general colour being somewhat paler than usual, while here and there small patches of a dull white colour were observed. These corresponded to whitish masses, the largest of which was not double the size of a millet-seed. They were irregularly scattered throughout the parenchyma of the organ. The mesenteric glands were much enlarged and very soft,—some of them even fluctuating. On section, they were seen to be mottled with irregular masses of extravasation. All the other lymphatic glands were in a similar condition.

Microscopic Examination.—The blood everywhere contained the same amount of colourless corpuscles, and presented the same appearance as has been already described in Cases I. and II. The cells of the liver varied considerably in size, and many of them contained fatty granules scattered through their interior, or clustered around the nuclei. Others contained a yellow-brownish granular matter, evidently bile. The spleen presented its normal structure, abounding in splenic cells, with a large number of free nuclei. The fluid squeezed from the mesenteric glands contained corpuscles exactly similar to those observed in Case I., and delineated, Fig. 4.

Remarks.—This case, while almost identical with the first, in regard to the morbid condition of the blood, spleen, and liver, and the existence of febrile action, differs from it in several remarkable particulars. There was no tendency to diarrhœa. It is expressly stated that she was generally costive. On the other hand, there was an hemorrhagic tendency,—the skin and some serous surfaces being thickly sprinkled with spots of purpura, in addition to hæmoptysis and spongy bleeding gums. In connection with these symptoms, we should remark, that the blood was observed to be unusually fluid after death, and that its analysis by Dr Robertson during life showed that, while the corpuscles were greatly diminished in amount, and the water correspondingly increased, the fibrin had undergone no change in quantity, or, if anything, was slightly under the average. The excess of colourless corpuscles, therefore, is not necessarily connected with an increase in the amount of fibrin, although here, as in Case II., a remarkable diminution in the aggregate quantity of globules, both white and coloured, existed.

In giving the other examples of the disease which have been recorded, I do not consider it necessary to repeat the appearances presented by the microscopic examination, and, with a view to brevity, shall detail in as condensed a manner as possible the facts of each.

CASE IV.[1]—*Leucocythemia detected after Death; Hypertrophy of Spleen and Liver.*

Peter Campbell, æt. 30, a weaver, was admitted into the Royal Infirmary, February 24, 1841, under Dr Craigie, with symptoms of feebleness, impaired health, and diffused swelling of the belly. He first felt hardness and swelling in the left epigastric and hypochondriac regions four months previous to admission, which continued to increase gradually until the last three weeks, during which time it has remained stationary. On entering the hospital, the belly was uniformly swollen and prominent, soft and elastic on the right side; but, on the left, hard, resisting, and solid. In this position there was felt a tumour, with a distinct circumscribed edge, having its upper part as high as the epigastrium, where the edge is hollow, and does not cross the linea alba. Below it crosses the linea alba, and forms a prominent border, extending about an inch on its right side. Its lower edge is rather irregular, being prominent below the umbilicus, but receding towards the left hypochondriac region. He has never had vomiting, shivering or chills, but has perspired freely during the night. Pulse from 92 to 96 in the minute. Respiration impeded, and laborious on exertion. Bowels stated to have been confined, but latterly regular. Purgatives, and subsequently a course of the ioduret of iron, were ordered. The purgatives produced numerous stools. On the 23d, it is stated that the bowels were moved three times daily. Diaphoresis continues. On the 25th he had headach, followed by marked febrile symptoms, for which he was bled to fourteen ounces. Delirium and insensibility supervened, for which blisters were applied afterwards to the head, and saline mixtures and purgatives given. He died April 1st.

Sectio Cadaveris—April 1st.—The spleen when removed weighed seven pounds three ounces and a-half. The greatest length was eleven and a-half inches; its circumference at the broadest part fifteen inches. On section, its substance was firm, and the cut surface presented many whitish bodies, seemingly form-

[1] Edinburgh Medical and Surgical Journal, vol. lxiv., p. 402.

ing numerous septa between the red matter. The liver weighed six pounds three ounces; its structure was normal. Kidneys were healthy. Many of the isolated intestinal follicles were distended so as to present prominences the size of millet seeds. The pulmonary organs were healthy, with the exception of a small hepatised portion of the lower lobe of the left lung. The vessels on the surface and at the base of the brain contained whitish coloured bodies, like pieces of lymph, which could be moved backwards and forwards by compressing the veins. In the cavities of the heart and the large veins, the blood presented exactly the same appearances as were presented in Case I.; and Dr Reid, at that time pathologist to the Infirmary, on examining the blood with the microscope, found it to contain globules of purulent matter and lymph.

Remarks.—I have placed this case among those in which the blood was physically examined, notwithstanding the imperfect account given of the appearances observed under the microscope, because there can be no doubt that the individual laboured under leucocythemia. No mention is made of the condition of the mesenteric and lymphatic glands in this case. The other appearances were identical with those noticed in Case I. The account says nothing of epistaxis or hemorrhage from the mucous membranes. The bowels were freely moved during most of the period he was in the house; but as purgatives were liberally administered, whether this symptom resulted only from their action, or depended on the disease, becomes difficult to determine. Fever latterly was well marked, with head symptoms, including delirium and insensibility.

The three following cases have been published by Virchow:—

CASE V.[1]—*Leucocythemia discovered after Death; Hypertrophy of Spleen; Diffuse Suppuration of the Hands.*

On the 1st March 1845, the patient (a female cook, æt. 50) was admitted into the Charité Hospital, Berlin, who had become affected a year before with swelling of the inferior extremities and abdomen, with severe cough, expectoration, and abdominal pain. In summer 1844, the cough disappeared, but came on again in autumn, along with severe diarrhœa. This again ceased, but subsequently reappeared, with bloody evacuations. There was slight œdema of the inferior extremities; the abdomen was swollen and fluctuating; the spleen was greatly enlarged, and somewhat painful; the cough frequent; expectoration copious. Rattling murmurs were heard in the chest; appetite good; urine scanty; great emaciation; pulse, 98. The diarrhœa was checked by treatment. Afterwards repeated epistaxis came on. In May, pain began to be felt in the region of the spleen, with costiveness. In the night of the 8th June the patient had a severe epistaxis, after which a furunculous eruption appeared on the nose, and between the thumb and index fingers of the right hand. On the 6th of July copious hemorrhage from the nose returned. On the 15th, a red and painful swelling of the skin was observed over the palmar surface of both hands, on which blisters, with purulent contents, formed, and opened themselves, assisted by warm fomentations. Subsequently an incision through the skin became necessary, from which a quantity of discoloured pus escaped. On the 29th July, diarrhœa suddenly supervened, the patient rapidly sank, and died on the morning of the 31st.

Sectio Cadaveris.—August 1.—Diffuse suppuration in the neighbourhood of the incisions on the hands; lymph and blood-vessels of the arm normal, but

[1] Schmidts' Jahrbücher. Januar 1848.

the superficial veins contained a dirty yellowish, badly coagulated clot. Almost all the vessels contained pus-like masses. The somewhat hypertrophied heart contained large, loosely-lying, greenish-yellow white clots, of the consistence of pus. Similar coagula were found in all the large vessels, arterial and venous. In veins which had thin coats, such as those of the brain, the contents presented the appearance of solid whitish strings, which, however, were always free, and nowhere attached to the vascular walls. Liver normal; spleen enormously hypertrophied, a foot long, very heavy, and of a dusky brown-red colour. It was extremely resistant, brittle in texture, displaying little blood on section, homogeneous, slightly lustrous, resembling the condition observed in fever. Kidneys normal, excepting uric acid concretions in the pelvis and ureters.

Remarks.—This case bears a close resemblance to the first related in most of the post-mortem appearances, especially the aspect of the blood in the blood-vessels generally, and on the surface of the brain. The state of the lymphatic glands, however, is not mentioned. The principal symptoms were diarrhœa, hemorrhages from the mucous membranes, and local suppuration over the hands. It is not distinctly stated that a febrile state was present, although the inflammatory phenomenon noticed renders this probable. I have copied the account from Schmidt's "Jahrbücher," the journal in which the case was originally published not being accessible to me.

Case VI.[1]—*Leucocythemia detected after Death; Hypertrophy of the Lymphatic Glands generally and of the Liver; Hydrothorax, Pneumonia, Bronchitis.*

Aug. Schulz, locksmith, from Potsdam, a very strongly built man, stated that he has frequently before suffered from inflammation of his lungs. For nearly two years he has observed a painful and gradually increasing swelling of the glands on both sides of his neck, as far as the angle of the lower jaw, and downwards to the shoulder. At the same time, frequently-returning and very violent constrictions of the chest came on, producing the greatest difficulty of breathing, and at night especially he coughed often and expectorated with difficulty. Gradually the body became swollen, the appetite impaired, whilst the thirst was increased. It was difficult to obtain fæculent discharges, although masses of mucus were passed continually. The patient felt very depressed and weary, active febrile symptoms appeared, and his sleep became disturbed. When, on the 26th June 1849, he was received as an out-patient of the Charité Hospital, Berlin, under the care of Herr Grimm, large swellings existed in the neck, axillæ, and groins, which were uneven, nodulated, not painful, very soft, but not fluctuating. In the cavity of the abdomen it was thought that enlarged glands could be felt, although a co-existent effusion of fluid made the examination difficult. Respiration was hurried; cough frequent, with mucous expectoration. At the inferior right lobe of the lung there was bronchial respiration, of considerable extent. On the left side mucous râles spread unequally over the middle portion of the lung. Externally, frictions of oil of turpentine on the chest; internally, Senega c. Amm. Mur., and Liq. Am. anis. This produced slight amelioration, but very soon fresh attacks of dyspnœa came on. Tr. Opii croc. c. Syr. Althææ. The dyspnœa increased so rapidly, that at two o'clock in the morning of the 2d August, death took place with symptoms of suffocation.

Sectio Cadaveris, eighteen hours after death.—Body little emaciated. The whole region of the neck, from the clavicle to the ear, the axilla, and groin, on both sides,

[1] Archiv für Pathologische Anatomie und Physiologie. Band. I. P. 567.

presented large, irregular, nodulated tumours, which had the soft and elastic feeling of lipoma. This was found to be owing to enlargement of the lymphatic glands, which, in the groin, had swollen to the size of large plums. More precise examination showed that this was simple hypertrophy. On making a section, a reddish-white, somewhat lax parenchyma was seen, from which a slightly muddy watery fluid exuded on pressure. In the lymphatic glands of the groin especially, not only was there enlargement of the peculiar external gland substance, but also of the connecting tissue in the interior, which gives them a peculiar kidney-like appearance. Microscopic examination showed the usual constituents,—namely, round, tolerably large, very granular nucleus corpuscles, the nucleus unaffected by acetic acid. More rarely finely granular cells having similar nuclei with several molecules between them. These enlargements extended throughout the whole system of lymphatics as far as was examined, but existed mostly in the interior of the body. The lumbar mesenteric and epigastric glands formed similar large nodulated tumours. In the anterior mediastinum and the pelvis masses of glands of extraordinary size were found. The pelvis was laterally quilted with gland substance on both sides, which seemed owing to the glands placed along the iliac vessels. The thoracic duct was packed in the substance of the gland, from its entry into the thorax to its termination, in such a manner, that it was difficult to recognise any division of the whole into individual tumours. Anatomical and microscopical examination nowhere showed any alteration of the structure of the glands. The lymph vessels were larger than normal, but not remarkably so, and contained a fluid almost as clear as water.

The spleen showed no alteration, except it were that the pulp was a little more compact than usual. The liver was much enlarged; and in its dusky-brown parenchyma, especially at the inferior portion of the anterior margin of the right lobe, a few white points were observed, of about the size of the normal liver lobules, and so embedded in the tissue, that they appeared as if substituted for lobules. In one place a larger grain was found, as large as a pea, of a pale white colour, neither projecting above nor depressed below the surface. On cutting through them a white fluid escaped from all these bodies, which showed, under the microscope, the nuclear elements characteristic of lymphatic and blood glands. They could be taken whole, without tearing, from the parenchyma with tolerable facility. If broken under the microscope, and then compressed, they presented the appearance of the Malpighian corpuscles of the spleen, a scarcely fibrous, almost structureless, membrane, and a dense mass of gland-nuclei. In the rest of the abdominal viscera nothing else unusual or of importance was found. There was slight serous effusion in the abdominal cavity.

Thyroid gland was normal. The bronchial glands hypertrophied. In the right pleura a considerable amount of clear watery effusion. Inferior lobe of right lung compressed and collapsed. Inferior lobe of left lung solid, collapsed, granular, and pale-red on section. In both lungs marked bronchial catarrh, with abundant mucous secretion.

There was some watery fluid in the pericardium. Heart normal, except that the right side was distended very considerably with blood. On opening the cavity of the heart, a thick, creamy, white, or slightly yellow mass was immediately expelled, so uncommonly like pus, that one of the bystanders believed that I had opened a large abscess in the heart. More careful examination showed that this was the blood, which had coagulated in the right side of the heart in such a manner, that it presented a red and a white clot, one under the other. The proportion of the one to the other could not be exactly determined; we may, however, consider two to three as an approximation. Microscopic examination showed that the white clot, besides a spare admixture of fibrin, consisted only of colourless nuclear and cellular structures, which only differed from the elements found in the lymphatic glands, in that the cells were proportionally more numerous. Besides these, numerous very small molecules occurred. The red clot, which presented to the naked eye a slight tinge of

grey, contained, besides normal red blood corpuscles, very numerous coloured structures. The rest of the vascular system contained sometimes more red clots, sometimes more white,—upon the whole, having the greatest number of white corpuscles.

Remarks.—This must certainly be regarded as a very remarkable case, presenting all the phenomena observed in the condition of the blood, recorded in Cases I. and IV., without enlargement of spleen, but instead, great hypertrophy of the lymphatic glands distributed throughout the body. Virchow also speaks of certain free nuclei in the blood, the nature of which, without a figure, it is very difficult to understand. We have previously seen, however, that in Case I. the lymphatic glands were enlarged, and the appearance of their cell-elements is represented Fig. 5. I presume that these with their nuclei free are the bodies to which Virchow alludes. The case proves that leucocythemia may be produced independent of enlargement of the spleen, although hypertrophy of other glands, abounding in similar cell-elements, seems necessary. The shut sacs, resembling the Malpighian bodies of the spleen, found in the liver, constitute altogether a new production, unlike anything observed before or since. No tendency to hemorrhage from the mucous membrane is noticed in this case, the whole of the symptoms being pulmonary, evidently associated with the lesions which existed in the lungs.

Case VII.[1]—*Leucocythemia detected after Death; Hypertrophy of Spleen and Lymphatic Glands; Tuberculosis.*

Heinrich Hoensch, journeyman joiner, ætat. 38, received into the Charité Hospital, Berlin, February 28th, 1848, under Professor Wolff. Seventeen years ago he suffered from an intermittent fever of ten weeks' duration. Four years ago, in consequence, as he says, of cold, he felt a stitch in the left side of his abdomen, together with diarrhœa and abdominal pain. These symptoms disappeared and returned at intervals. Nine months after they were more constant, and two months since the large swelling was first observed, which occupied the whole left side of the abdomen. Its margins were easily determined, and pressure caused no great pain. Diarrhœa continued; the appetite was normal; he sleeps well, and is somewhat debilitated. In the middle of March headach and epistaxis appeared and frequently returned. On the 26th he lost a quart of blood by epistaxis, which was arrested by plugging, but produced great weakness. During April there was no recurrence of bleeding. The pulse rose gradually, and the strength improved; the tumour also diminished. He had an anemic appearance, and was very fretful, and left the hospital on the 13th of June. He returned in a fortnight. Slight fever came on, which increased, and after the 21st July there was an evening exacerbation. The tumour again enlarged, and slight epistaxis returned. Accurate examination showed that the tumour was from half an inch to an inch larger in diameter in the morning than in the evening. Pulse in the morning was 104, in the evening 120; urine

[1] Archiv für Pathologische Anatomie und Physiologie. 2te Band. P. 587.

contained a considerable sediment of urate of ammonia and phosphates. The fever became aggravated, with occasional epistaxis, till October, when headach, dizziness, dyspnœa, cough, with deep inspiration, came on. There was diminished respiratory murmur in the left breast inferiorly, and in one part crepitation. 10th October, he was cupped on the affected part. Pulse frequent, especially in the evening; there was cough, with deep inspirations; the sputa were tenacious and rusty coloured. The symptoms went on increasing till, on the 26th October, copious bilious diarrhœa supervened, the fever continued, and he died on the 2d of November.

Sectio Cadaveris.—Body emaciated—fat disappeared. The internal table of the cranial bones was thickened by osteophytic deposits. In the longitudinal sinus the blood presented somewhat of a greyish coagulum. Considerable effusion of serum in the enlarged ventricles, the walls of which were softened by imbibition. The capillaries presented no apparent accumulation of colourless blood-corpuscles. Thyroid gland tolerably large and pale. A few tubercles in the right lung. Recent tuberculosis of left lung and pleura, with fibrinous exudation. In the heart, which was normal, were a great many fragmentary coagula, which presented an inferior mass, moderately thick and red, and a superior one, like coagulated pus, of a white colour. These clots were continued into the arteries. In the veins was fluid grayish-red blood, along with similar yellowish-white clots. Examined microscopically, the red blood-corpuscles were mixed everywhere with numerous colourless ones, which in the white clots occurred almost alone, or were associated with fatty molecules. The colourless corpuscles were of the usual size, slightly granular, displaying, with acetic acid, single nuclei or several small ones, with all intermediate gradations. Very often the transition forms were horse-shoe shaped or crescentic. The spleen occupied the whole side of the abdomen, and the diaphragm was adherent to its upper surface. Externally it was very firm, bluish-red, and at its outer margin were two yellowish-white tubercles. On section, it was homogeneous, pale grayish-red, almost flesh colour. By careful examination it displayed small white corpuscles. Pulp very resistent and dense. When examined microscopically, there was found, among normal cells and nuclei, a slightly granular mass of exudation. Liver, with exception of small tubercles, normal. Kidneys flabby, with miliary tubercles. Epigastric lymphatic glands enlarged, flabby, somewhat œdematous. Internal mucous membrane presented here and there tubercular ulcerations.

Remarks.—In this case, associated with the leucocythemia, there was enlargement of the spleen, without hypertrophy of the liver. The lymphatic glands are said to be enlarged and œdematous. Whether they were, or were not, similar to the hypertrophied glands formerly noticed, is not stated. Tubercular exudation existed also in various tissues, and especially tubercular pleurisy. Here, again, we observe epistaxis to be a leading symptom, followed by fever and copious diarrhœa.

CASE VIII.[1]—*Leucocythemia discovered during Life; Hypertrophy of Spleen.*

Johanna Sheen, aged 69, a fruit-seller in the streets, was admitted into the North London Hospital under the care of Professor Parkes. Has never had any serious illness or malarious disease. Towards the end of 1848 had swelled feet, and shortly afterwards pain in the left side of abdomen. She then first perceived a considerable swelling below the false ribs on the left side. She had no sickness,

[1] Medical Times, June 8, 1850.

shivering, or fever. She entered the hospital in January 1849, and remained under treatment there (blisters, iodide of potassium, and morphia) until the end of February, when she was discharged. On several occasions during this period she had morning sickness, and vomited a little blood. In November, when in the street, she was knocked down by a cart, and suffered some pain in the abdomen for four weeks in consequence. On re-admission, December 13, 1849, "we found her thin and shrivelled, without œdema or enlargement of veins; there was a peculiar dusky, yellowish-brown colour of the skin, most evident on the trunk, and less marked on the face and extremities; the conjunctivæ were clear; there were no head symptoms; and no pulmonary symptoms, with the exception of a little dry friction low down on the left side; there was a feebly acting heart, in its right position, and without bruit; the pulse was 72, regular; the radials not visible. A large tumour, evidently an enlarged spleen, filled the left side of the abdomen, descending from the lower border of the seventh rib, nearly to the ilium, bulging into the posterior left lumbar region when she lay on her back, and reaching nearly to the umbilicus on the right; falling over considerably to the right of the umbilicus when she lay on the right side; with a prominent, rigid, smooth, lower border; very hard throughout, and tender; the extreme length of the dull percussion-note was 8 inches; no splenic murmur was ever audible. The height of the hepatic dulness (vertical line from nipple) was 4½ inches; the lower edge was 1½ inch below the false ribs; to the left of the middle line the hepatic dulness confounded itself with the splenic, that is to say, with the dulness of the tumour. There was no fluid in the peritoneum, no nausea; the appetite was good, the tongue clean; there were no intestinal or uterine symptoms. She had had no epistaxis at any time; no hæmoptysis; a little hæmatemesis, apparently, when she was in the hospital previously; no melæna, hæmaturia, or mennorrhagia.

"During the patient's stay in hospital, viz., till the 17th of March, she remained nearly in the same condition. Occasionally she suffered from severe frontal headaches; she also had, on several occasions, sharp stabbing pains in the abdomen, both over the spleen and liver, for which leeches were applied, the bleeding from which was profuse and not very easily arrested. On four or five occasions, also, she had moderate shivering, followed by heat and sweating; these attacks were not very regular, seemed to observe no certain times, and were separated from each other by long intervals. On the 27th of February, according to her own account, she passed a pint of blood with a stool; but this, and another stool passed subsequently, were thrown away accidentally, and were not seen by any one; the next stools were free from blood. With this exception, she had no bleeding from any part of the body, and none into the substance of the skin. On the 17th of March the patient left the hospital, nearly in the same state as on admission."

Blood drawn from the finger evidently presented, from the excellent description given by Dr Parkes, the same appearances represented in Figs. 6 and 7.

The venous blood was analysed on two occasions, by a process closely resembling that adopted by Dr Robertson of Edinburgh; first, on the 20th December, the blood being taken three hours after food, when the composition of the blood in 1000 parts was as follows:—

Fibrin (with probably adherent white corpuscles), ...	7·08
Red particles, with a number of white corpuscles which could not be separated, ...	101·63
Coagulable organic matters of serum, ...	63·03
Incoagulable, ...	3·08
Soluble salts of the serum, ...	8·63
Insoluble salts of the serum, ...	·48
Water, ...	816·07
	1000·00

"The composition of the serum was as follows. The reaction was strongly alkaline:—

Coagulable organic matters, ...	70·71
Incoagulable, ...	3·46
Soluble salts, ...	9·68
Insoluble (obtained by incinerating the dried albumen), ...	·55
Water, ...	915·6
	1000·0

"A small portion of serum was examined for uric acid in the ingenious method devised by Dr Garrod, viz., by the addition of a little acetic acid to a portion of serum in a watch-glass, at the bottom of which lies a fine hair. No crystals of uric acid could, however, be perceived.

"On the 15th of February the patient was bled again three hours after food; the fibrin was estimated by washing the clot. The composition was as follows:—

Fibrin (with probably adherent white corpuscles), ...	4·75
Red particles (with undetermined white corpuscles), ...	97·93
Organic solids of serum, ...	69·27
Inorganic solids of serum, ...	8·25
Water, ...	819·8
	1000·00"

The urine was subjected in this case to a series of careful observations, but nothing unusual was observed.

Remarks.—In this case, which was under careful observation for three months and a-half, there were observed occasional slight accessions of fever, but none of the epistaxis, or violent hemorrhages and diarrhœa, we have seen to constitute such leading symptoms in previous cases. At one period, indeed, she is said to have had hematemesis, and at another a bloody discharge from the bowels; but the latter is doubtful. The further progress of this woman's case is a matter of some interest. As regards the analyses of the blood, they show increase of the fibrin, and very slight diminution in the amount of the corpuscles,—in the latter respect exhibiting a marked difference from the results obtained by Dr Robertson in the Edinburgh cases.

The two following cases are recorded by Dr Fuller:—

CASE IX.[1]—*Leucocythemia detected during Life and after Death; Hypertrophy of Spleen and Liver.*

On the 31st December 1845, a man was admitted into St George's Hospital, who had been seized eight months before with dyspeptic symptoms. Six weeks later, a small, firm, painless tumour appeared in his left hypochondrium, which in three months rapidly increased. The dyspnœa continued, he lost appetite, occasional vomiting came on, and his bowels were costive. There was constantly recurring bleeding at the nose, and since the middle of December diarrhœa has continued. He died on the 8th of January 1846.

Sectio Cadaveris.—Spleen and liver very much enlarged. All the blood-vessels enlarged, and the blood grumous, of a remarkable gray colour.

The blood which was examined before death showed, besides its normal corpuscles, abnormal spherical bodies, of a finely granular appearance, colourless,

[1] Lancet, July 1848.

without visible membrane or nucleus; some of the usual size of corpuscles, others much larger. They were so numerous that they formed about a quarter of the whole sum of the corpuscular elements of the blood. The splenic artery and vein, like all the portal veins, were much enlarged, and filled with half-coagulated blood, of the consistence of soft spleen pulp, but of a peculiar gray-red colour.

CASE X.[1]—*Leucocythemia; Encephaloid Tumour of the Abdomen.*

An encephaloid tumour of the abdomen was removed from a girl, nine years of age, who became an out-patient, under Dr Fuller, at St George's Hospital, in February 1850. She had enjoyed tolerable health till July. In a few weeks after this a tumour was perceived projecting from under the ribs on the left side downwards towards the pelvis. It increased very rapidly, and pressed upon the rectum and neck of the bladder,—thus producing much distress. When brought to the hospital, the abdomen was enormously enlarged, the right side tympanitic, the left dull on percussion. Protruding from under the ribs on the left side, and causing them to bulge considerably, was a firm, solid tumour, which could be traced down the abdomen on the left side, dipping into the pelvis, its anterior margin being irregular. It was regarded to be an enlarged spleen. She was greatly emaciated, and her legs œdematous. She died on the 30th of March.

The encephaloid tumour was seen occupying the whole of the left side of the abdomen. Superiorly it pressed against the diaphragm, and encroached considerably upon the thorax. Inferiorly it dipped into the pelvis between the rectum and the bladder. It was irregularly oblong, knobby on its surface, and larger at its superior than inferior extremity. It weighed four pounds nine ounces. Its cut surface was in most parts of a cream colour, firm and elastic to the touch, but in parts of a dark mottled appearance, and somewhat softer. The bladder was greatly distended, and was above the brim of the pelvis. The ureters were considerably dilated, also the pelvis of either kidney. The structure of the kidneys was much diminished by pressure. The other viscera healthy.

The tumour itself presented nothing of particular interest, but showed that a condition of the blood, which had been supposed peculiar to certain forms of disease of the spleen, may be seen in other disorders. The peculiarity consists in the presence of large numbers of colourless, granular, spheroidal globules, varying in size from that of a common blood corpuscle to twice or three times that size, as shown by actual admeasurement. Whether these globules be merely the colourless corpuscles of the blood in an altered condition, or altogether of abnormal production, there can be no doubt that their presence is indicative of an unhealthy state of the blood. These globules Dr Fuller had observed in every case of enlarged spleen, unconnected with ague, in which the blood was examined; and satisfied by repeated examination that they do not exist in the blood of persons having enlargement of the spleen as the result of ague, he had supposed them peculiar to that form of enlargement of the spleen which occurs independently of malarious influence. But as the same condition of blood was found in this case, it is obviously not peculiar to, or diagnostic of, the disease alluded to; and it becomes a question of considerable interest as to the conditions under which these globules make their appearance.

Remarks.—These notices of cases leave much to be desired. Still the facts mentioned are important. With regard to the latter case,

[1] Report of the Proceedings of the Pathological Society of London. Fourth Series. Pp. 224, 225.

I was anxious to ascertain the nature of the tumour and a few other particulars, which Dr Fuller was so good as to give me in the following note:—

"I have much pleasure in replying to your queries respecting the case of encephaloid disease reported in the last vol. of the 'Transactions of the Pathological Society.' The tumour was not examined microscopically; for I performed the post-mortem examination only a short time before the meeting of the society, and from some mismanagement, I was unable to regain possession of the preparation when I sent for it on the following day. My other answers however, are more satisfactory. The poor child had frequent hemorrhage from the bowels, the nates, and the mucous membrane lining the trachea and bronchi; her gums, too, were spongy and bled occasionally. There was *no* enlargement of the mesenteric glands, or of the lymphatic glands in any part of the body; but she had great irregularity of bowels, which were occasionally relaxed but more generally costive. I have now discovered this condition of the blood in six cases,—four of which were cases of enlargement of the spleen, unconnected with ague or malarious influence. One was the case of encephaloid disease reported in the 'Pathological Transactions,' and one a case which I have now lost sight of,—of a tumour, still apparently solid and immoveable, situated on the left side of the epigastric region, which may possibly have been an enlarged spleen. This case, however, when I was watching it, about three years since, was in its early stage. The patient, a married woman, was apparently in good health; her digestive functions had not materially suffered, nor had her gums become spongy, nor had she had hemorrhage from any of the mucous membranes. Much difference of opinion existed among those who saw her as to the nature of the tumour; and this it was which induced me to examine the condition of the blood. The colourless corpuscles were not at that time numerous, but they were of very large size."

CASE XI.[1]—*Leucocythemia detected after Death.—Hypertrophy of the Spleen.*

The spleen measured about fourteen inches in length, and four or five in depth. Its structure was extremely dense, exhibiting, on section, a beautiful mottled appearance; but under the microscope presented no obvious deviation from the normal character. It was taken from a strumous subject. The heart was quite healthy. A peculiarity of the case was the fluidity of the blood after death, and the presence in this fluid of a large number of granular, irregular, spheroidal bodies, twice or three times the size of the blood corpuscles. This was the second case that had come under the notice of Dr Chambers, in which the post-mortem phenomena were analogous."

The eleven cases now given are all those, so far as I can discover, which have yet been published. I have heard, however, of others, and entertain some expectations that my professional friends will kindly allow me to publish some of them in continuation of the present series.

(*To be continued.*)

---

[1] Proceedings of the Pathological Society of London. First Session, 1846-7.

ARTICLE VII.—*Notices of Ancient Roman Medicine-Stamps, &c., found in Great Britain.* By J. Y. SIMPSON, M.D., Professor of Midwifery in the University of Edinburgh.

MANY years ago Schmidt, in his work on the Antiquities of Nimiguen,[1] published an account of two small, thin, greenish-coloured, square-shaped stones, both of them engraved with intagliate inscriptions on their four sides or edges; but he failed in making out the nature of the inscriptions, or interpreting the object and uses of the stones.

Subsequently, Spon,[2] Chishull,[3] Caylus,[4] Saxe,[5] and Walche,[6] published accounts of other stones exactly similar in their character to the two found at Nimiguen; and it came to be generally admitted, that the nature of the inscriptions upon them, the incuse and retrograde form of these inscriptions, and the localities in which they were found, all proved the stones to be medicine-stamps, employed for the purpose of marking their drugs, by the Roman doctors, who (some fifteen or sixteen centuries ago) practised at the various stations throughout Europe, that were in these olden times occupied by the colonists and soldiers of Rome. Latterly, various additional examples of these Roman medicine-stamps have been discovered, at different old Roman towns and stations in France, Germany, &c., and described by Tochon,[7] Sichel,[8] Duchalais,[9] and others. These medicine-stamps all agree in their general characters. They usually consist of small quadrilateral or oblong pieces of a greenish schist or steatite, engraved on one or more of their edges or borders. The inscriptions are in small capital Roman letters, cut intagliate (like the letters on modern seals and stamps), and consequently reading on the stone itself from right to left, but making an impression (when stamped upon wax or any other similar plastic material), which reads from left to right. The inscriptions themselves generally contain, and that engraved on each side, first, the name of the medical practitioner to whom the stamp pertained; then the name of some special medicine, or medical formula; and lastly, the disease or diseases for which that

---

[1] Antiquitates Neomagenses, p. 98.

[2] Miscellanea Eruditæ Antiquitatis, p. 236.

[3] Hayms Tesoro-Brittanico (1720), vol. ii. Letter in Preface.

[4] Recueil d'Antiquités, tom. i., p. 224, and tom. vii., p. 261.

[5] Christophori Saxii Epistola de Veteris Medici Ocularii Gemma Sphragide, prope Trajectum ad Mosam erutæ. 1774.

[6] Antiquitates Medicæ Selectæ. Jena, 1772.

[7] Dissertation sur l'Inscription Grecque ΙΑΩΝΟΣ ΑΥΚΙΟΝ. Paris, 1826.

[8] Cinq Cachets Inedits de Médécins-Oculistes Romains. Paris, 1845. To my friend, M. Sichel, one of the most learned of living physicians, I am much indebted for various valuable suggestions in collecting the materials for the present essay.

[9] Observations sur les Cachets des Médécins-Oculistes Anciens, à-propos de Cinq Pierres Sigillaires inedites. Paris, 1846.

medicine was prescribed. But sometimes the first and last of these items are omitted, and the stamps present merely the appellation of the medicine alone; and occasionally the modes and frequency of using the medicine are added. To this brief description one more curious fact remains to be added,—namely, that in almost all, if not in all, the Roman medicine-stamps hitherto discovered, the medicines inscribed upon them are drugs for affections of the eye and its appendages; and the diseases, when specified upon them, are always ophthalmic diseases. Hence it may, with great probability, be concluded, that either these stamps were used by oculists alone, or they were used by the general medical practitioner in marking his eye medicines only. Some authors, on this account, have described them under the designation of Roman oculist-stamps.

The number of the stamps that have already been discovered amply proves that ophthalmic diseases must have been extremely frequent in the sites of the old Roman colonies spread throughout western Europe; and although only one or two such oculist-stamps are as yet described as having been found within the bounds of Italy itself,[1] yet the frequent references to individual oculists at Rome[2] by Celsus, Galen, and others, and the elaborate descriptions of eye diseases left us by the various Greek and Roman medical authors, who practised in the Eternal City during the time of the empire, sufficiently testify to the fact, that these diseases were also sufficiently common in the Roman capital, and that many of the fellow-citizens of Horace could probably personally apply the well-known description which the poet gives of himself,—

> Hic oculis ego nigra meis Collyria lippus
> Illinere.

Galen, Celsus, Aetius, &c., all describe the different diseases of the eye with care and minuteness; and the Roman practitioners had evidently studied these affections, and their specific distinctions, with great attention. In modern times medical literature has been enriched with more complete and elaborate monographs upon the diseases of the eye, than upon the diseases of any other single organ of the body. But, perhaps, few of these monographs describe a larger

[1] The two found in Italy have been discovered, the one at Sienna, the other at Verona. See Muratoris' *Thesaurus Inscriptionum*, D. viii. 4; and Maffei's *Museum Veronense*, p. 135.

[2] Among the ancient Egyptians the medical practice seems, according to Herodotus, to have been subdivided, some three thousand years, into more specialities than we even have at the present day. "The art of medicine is (says he) thus divided amongst them: Each physician applies himself to one disease only, and not more. All places abound in physicians,—some physicians are for the eyes, others for the head, others for the teeth, others for the parts about the belly, and others for internal disorders."—*Cary's Herodotus*, Enterpeii., § 84, p. 125. Most of the old Roman medical authors mention "medici oculariі," "medici ophthalmici," in such terms as to show that the department of eye diseases was cultivated by some medical men as a speciality of practice in their time.

number of ophthalmic diseases than was professed to be known and discriminated in the times of Galen. This author, in the 16th chapter of his book, entitled "Introductio seu Medicus," enumerates not less than one hundred and nineteen diseases to which the eye and its appendages are liable.

In the management of these diseases of the eyes, the Roman practitioners used, as their writings show, bleeding, scarification, and other appropriate constitutional and local treatment. But the practical part of their treatises, referring to ophthalmic affections, is specially loaded with collyria—professedly of use in almost every stage of every disease of the eye. Galen speaks of Asclepiades, describing in his works a perfect forest of collyria (*collyriorum silvam*). In his book "De Compositione Medicamentorum secundum Locos," Galen has himself left us formulæ for upwards of two hundred of the ancient collyria. Aetius gives as great, if not a greater, number. The "Opus de Compositione Medicamentorum" of Myrepsus, contains recipes for eighty-seven ophthalmic collyria; and the works of Scribonius Largus, Celsus, Actuarius, Alexander Trallianus, Marcellus, Paulus Aegineta, &c., present us with abundance of formulæ for the same class of preparations.

These collyria were composed of very various,[1] and in some instances, of very numerous ingredients. But most of them which had attained any great degree of reputation, seem (like the compound formulæ, or prescriptions in our modern pharmacopœias), to have each passed under a short specific name, by which they were, no doubt, readily and generally recognised by the profession, and perhaps also by the public, in these ancient times. The specific appellations of the individual collyria were derived from different sources.

Some of them were known under the names of the oculists who invented or employed them. Thus Galen gives recipes for the collyria of Asclepiades, of Philoxenis, of Capiton, of Zoilus, of Antonius Musa, the collyrium of Sergius, the Babylonian oculist; and many others.[2] Occasionally the appellation under which the collyria were known was derived from some of their more marked physical properties, as the "collyrium *Chloron* appellatum," from the green

---

[1] In the following passage Galen tersely enumerates the very varied general ingredients, and general therapeutic effects, of the numerous collyria used by the Roman practitioners of his day:—"Nam et liquores, et succi, et semina, et fructus, et plantarum particulæ, ocularibus compositionibus induntur, veluti etiam non pauca ex iis, quæ metallica appellantur; aliqua quidem extreme austera, et acerba, atque acria; aliqua vero his moderatiora et tamen fortia; quemadmodum item aliqua omnis mordacitatis expertia, ac lenissima per lotionem addita."—*De Compositione Medicam: Secundum Locos*, Cap. I.

[2] Celsus, in the same way, enumerates and describes the collyria of Philon, of Dionysius, of Cleon, of Theodotius, of Euelipedes (qui ætate nostra maximus fuit ocularius medicus), of Nileus, of Hermon, &c.

colour of the preparation; the *Cirrhon*, from its yellowish tint; *Euchron*, from its agreeable hue (a colore bono dictum); the collyrium *Cygnus*, from its white or swan-like hue.[1] One was termed *Aromaticum*, from its odour, and so forth. One or other of the principal ingredients entering into its composition seems to have given the name under which other collyria were known, as the *Nardinum*, from its containing spikenard; the collyrium *Diasmyrnes* (δια with, and σμυρνα, myrrh), from its containing myrrh; the *Diarrhodon*, from its containing roses, &c. Occasionally the collyrium seems to have derived its name and fame from some great person whom it had been fortunate enough to benefit or to cure. Thus, for example, Galen gives a recipe for the collyrium which Phlorus used in the case of Antonia, the mother of Drusus; the "*collyrium harmatium*," which king Ptolemy used, &c. One was termed *Achariston*, from its cheapness: and this collyrium repeatedly occurs on the oculist-seals. Another was termed *Atimeton*, from its supposed great value. But perhaps the most common mode of appellation was the use of some recommendatory name, advertising the supposed high qualities of the drug. Thus the old Greek and Roman authors give various species of the collyrium *Monohemeron*,—so named from its being alleged to effect a cure in a single day; others are designated the *Miraculum*, the *Mysterium*, the nectar collyrium (*Nectarium*); the royal Indian collyrium (*collyrium Indicum regale*); the golden (*Isochryson*); the divine (*Isotheon*) &c., &c. And lastly, a collyrium was often known under some high sounding but unmeaning name, such as the collyrium *Olympus*, *Proteus*, *Phœnix*, *Phynon*, the "collyrium nominatum *Sol*," &c.

Under such designations the principal collyria of the Roman oculists were known and used (like the one invented and boasted of by Galen) "per omnes gentes quibus imperant Romani;" and it is under such special appellations that we find these different collyria mentioned in the inscriptions engraved upon the old oculist-stamps, and scattered among the ruins of their ancient colonial stations.

Above sixty Roman oculist-stamps have been now discovered in different parts of western Europe, but particularly in Germany, France, and Holland. Some time ago one was found about ten miles east of Edinburgh; and it is principally with the view of describing this and the other specimens that have been detected in Britain that I have drawn up the present imperfect notes. I have been the more induced to do so because this Scottish stamp is remarkable, both as being found on almost the very frontier of the ancient Roman empire, and as being one of the most perfect yet discovered. Besides, I entertain a strong hope that a notice such as the present, in a Medical Journal, may perhaps be fortunate enough to lead,

[1] Appellantur talia a medicis collyria *libiana* et *cygni*, ob colorem quidem album.—*Galen. de Compos. Med: Secundum Locos*, Cap. I.

through the zeal of some members of the profession, to the detection in England and Scotland of additional examples of these curious remains of our Roman medical predecessors in these islands.

Description of Individual Roman Medicine-Stamps found in Scotland and England.

No. I.—The Scottish specimen of medicine-stamp to which I have above adverted was discovered some years ago at Tranent in East Lothian, not far distant from the old, and doubtlessly, in former times, extensive Roman town or settlement at Inveresk. The stamp now belongs to the Museum of the Society of Scottish Antiquaries.

It was presented to the Museum by the late Mr Drummond Hay, formerly one of the Secretaries of the Society. From Mr Hay's notes it appears that it was found amid a quantity of broken tiles, brick, and other debris of an old (and probably Roman) house, near the church of Tranent. For many years after being deposited in the Antiquarian Museum its character remained undiscovered, till the present excellent Secretary to the Society, my esteemed friend Mr Daniel Wilson, was led, in reading the descriptions of other similar stamps, to ascertain its true character.

The stamp itself is, as usual with these seals, cut upon a greenish coloured steatite. The stone is of the figure of a parallelogram, with inscriptions cut upon two of its sides. There is a roundish projection at either extremity of the stone, as seen in the accompanying lithograph (Plate I., No. I., Figs. 1, 2, 3), where the stone and letters of the two inscriptions are, in every respect, faithfully copied from the original as to form and size. The letters are, as in all other similar medicine-stamps, cut incuse and reversed, so as to read from left to right, when the inscription was stamped upon any impressible material. Fig. 2 shows one of the inscriptions as it appears cut intagliate upon the stone. Fig. 3 presents an accurate copy of this inscription as it is seen when stamped upon wax. Fig. 1 is an equally faithful copy of the second inscription placed on the opposite side of the stone. It will be observed that, as in the original, the size of the lettering varies on the two sides.

The inscriptions on the sides (1 and 2) read as follows, when we separate the individual words composing them from each other.

1. L. VALLATINI EVODES AD CICATRICES ET ASPRITUDIN

2. L. VALLATINI APALOCROCODES AD DIATHESIS

Let us endeavour to interpret each of these inscriptions in detail, supplying the elisions and contractions which exist in almost all Roman inscriptions, but which are less in this seal than in most others.

1. *Lucii* VALLATINI EVODES AD CICATRICES ET ASPERITUDINES. —*The Evodes of Lucius Vallatinus for cicatrices and granulations.*

Several of the collyria derived, as I have already observed, their designation from some special physical character. The present instance is an example in point, the appellation *Evodes* (εὐώδες) being derived from the pleasant odour (εὖ, well, and ὄζω, I smell) of the composition. Marcellus, in his work "De Medicamentis," specially praises the collyrium known under the name of *Evodes;* and that too in the class of eye diseases mentioned on the Tranent seal. For, in his collection of remedies for removing ulcers, cicatrices, &c., of the eyes and eyelids, he recommends (to use his own words) "præcipue hoc quod quidam Diasmyrnon, nonnulli *Evodes*, quia boni odoris est, nominant." And he directs the *Evodes* to be dissolved and diluted in water, and introduced into the eyes with a probe, or after inverting the eyelid, when it was used with the view of extenuating recent cicatrices of the eyes, and removing granulations of the eyelids,—"ex aqua autem ad cicatrices recentes extenuendas, et palpebrarum asperitudinem tollendam teri debet, et subjecto specillo aut inversa palpebra, oculis inseri."[1]

Scribonius Largus had previously described, in nearly the same words, the collyrium,—"quod quidam εὐώδες vocant," and its uses in recent cicatrices and granulations, &c. Both these authors give the same recipe for the composition of the *Evodes*,—viz., pompholyx, burnt copper, saffron, myrrh, opium, and other ingredients, rubbed down in Chian wine. Its agreeable odour was probably owing to a considerable quantity of spikenard being used in its composition.[2] Galen gives two other collyria, of a different composition, and for other affections, as known at his time under the same name of *Evodes*,—the one termed the "*Evodes* of Zosimus," the other the "diasmyrnon *Evodes* of Syneros."[3]

2. L. VALLATINI APALOCROCODES AD DIATHESIS. — *The mild Crocodes of L. Vallatinus, for affections of the eyes.*

The term diathesis in this inscription is used in a different sense from that in which we now employ the same word in modern medicine. At the present day, we apply the term diathesis to designate the tendency or predisposition to some special disease, or class of diseases. In the times of the Roman physicians, it was often used as synonymous with disease itself; and in the Latin translations of the Greek texts of Galen, Aetius, &c., it is hence rendered usually by the general word "affectus," "affectio," &c. The first sentence in Paulus Æginetus's chapter on Ophthalmic Diseases, affords an instance in point: "Quum dolores vehementiores in oculis fiunt, con-

[1] Medicæ Artis Principes, p. 273.

[2] Medicæ Artis Principes: Scribonii Largi de Compositione Medicamentorum Liber. Comp. xxvi., p. 198.

[3] Galeni Opera Omnia. (Kuehn's Edit.) Vol. xii., pp. 753 and 774.

ROMAN MEDICINE-STAMPS Plate I.

N°I

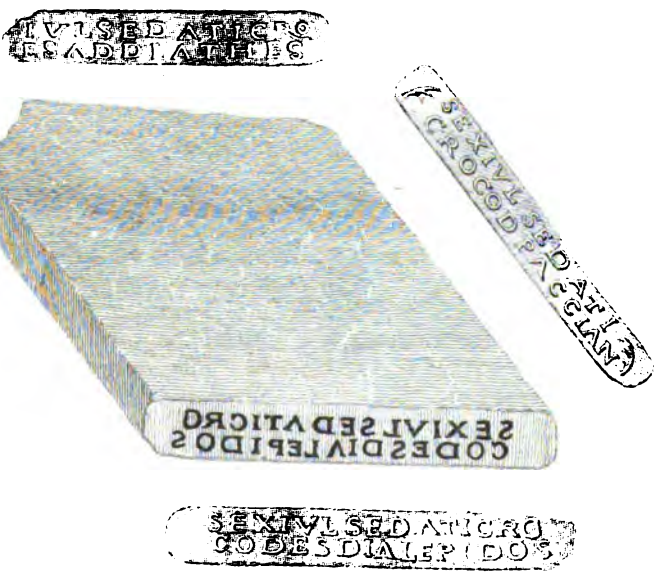
1

2

3

N°III

N°II



Month's Medical Journal. January 1851.

qualifying term to the *Crocodes*. Several of the collyria have the Latin adjective "lene," and "leve," placed before them, in order to certify their mild nature. Scribonius Largus gives a whole division of collyria, headed "Collyria composita levia." Aetius has a chapter, "De Lenibus Collyriis." The expression *apalo*, as a part and prefix to *Crocodes*, would seem to indicate the same quality in the crocodes sold by Vallatinus, the term being in all likelihood derived from the Greek adjective ἁπαλος, or the corresponding Latin adjective *apalus* (mild, soft). Homer frequently uses the word as signifying soft, delicate, and especially as applied to different parts of the body (See Iliad, book iii. 371; xvii. 123, &c.); and, indeed, Aetius employs the Greek adjective therapeutically in the sense of mild, and as applied to collyria. In the treatment of acute inflammatory ulcers of the eye, after inculcating the usual antiphlogistic treatment, he adds, "collyria vero tenera (ἁπαλα) ulcerato oculo infundantur."[1]

I am indebted to the kindness of Mr Birch for the impressions of two unpublished oculist-stamps, found in England, and contained in the British Museum. Their forms and inscriptions are represented in Plate I., Nos. II. and III.; and I shall describe them under these numbers.

No. II. This large stamp consists (Plate I., No. II.[2]) of a flat quadrilateral stone, about an inch and a-half broad, and engraved upon three of its sides. A portion of one corner of the stone is broken off; but it is easy to supply the deficiency which is thus produced in one of the inscriptions. The three inscriptions read as follows:—

1. SEX: JUL: SEDATI CROCOD PACCIAN.
2. SEX: JUL: SEDATI CROCODES DIALEPIDOS.
3. (*Sex*): JUL: SEDATI CRO(*cod*)ES AD DIATHES.

The name of the oculist—Sextus Julius Sedatus—is imperfect on the 3d or broken side, the prænomen "Sex" being wanting on that side in the first line, and the middle syllable "cod" of the word Crocodes being also wanting, from the same cause, in the second line.

The restored reading of this 3d side—viz., SEXTI JULII SEDATI CROCODES AD DIATHES*es*—need not be dwelt upon, as it is so very similar to that on one side of the Tranent stone. The other two sides contain the names of two new varieties of crocodes.

[1] Cornarius' Latin edition of Aetius, 1549, p. 371; and Venice Greek edit., p. 126.

[2] The central figure shows the size of the stone, and the intagliate inscription on one side. The other figures show its three inscriptions as they read from left to right when stamped on wax.

One of these varieties—the CROCODES PACCIANUM—received its name from Paccius the oculist, who either invented this special collyrium, or brought it into repute. In the list of his ophthalmic medicines, Galen gives formulæ for various collyria, invented by Paccius, such as the *Sphragis Paccii*,[1] *Asclepiadeum Paccii*,[2] *Collyrium* ex terra Samia *Paccii Ophthalmici* ad affectus intensos (επιτεταμενας διαβασεις).[3] Galen does not give any receipt for the *Crocodes* of Paccius; but it was evidently a collyrium duly esteemed at the time at which he wrote; for, in his chapter on ulcers of the eyes, he specially names the "*Crocodes Paccianum*,"[4] and recommends its use in cases in which the accompanying inflammation has already ceased, and at the stage when a stimulating application becomes necessary.

The other variety of crocodes used by Sedatus is the CROCODES DIALEPIDOS. A formula for *Dialepidos* is given by Marcellus,[5] with the crocus as the first ingredient mentioned in its composition. The *Dialepidos* derived its name from its containing the scales (λεπιδος) of burnt copper, or the black peroxide of that metal,—a preparation which Dioscorides (lib. v., cap. 89) describes as useful in eye diseases; and which Galen declares to be a "medicamentum multo utilissimum," xii. p. 223.

No. III. The second undescribed Roman medicine-stamp, contained in the British Museum (see Plate I., No. III.) is small and broken. It is only engraved on one side, and the inscription does not contain, as usual, the name of the oculist who possessed and employed it. The inscription reads as follows:—

COLLYR. P. CLOC.

If extended, the inscription would probably run thus,—COLLYR*ium* P*ost* CA*Liginem* OC*ulorum*. Several of the prescriptions found in these medicine-stamps are collyria, professing to be useful against and after (*ad* and *post*) *caliginem*.

No. IV. The first Roman medicine-stamp discovered in Great Britain was described about 130 years ago by Mr Chishull in the learned "Dissertatio De Nummo CKωIII," which he addressed to Haym, and which this last-mentioned author has published in the preface to his second volume of the "Tesoro Brittanico."

The stamp had been found some years previously at Colchester, a well known and extensive Roman colonial station. Mr Chishull

---

[1] Kuehn's Galen. Tom. xii., p. 751.
[2] Ibid. P. 772. [3] Ibid. P. 760.
[4] Ibid. Tom. xi., p. 715.
[5] Medicæ Artis Principes: De Medicamentis Liber. P. 280.

belonged to some old Roman inscriptions [illegible]

The following is a copy of the inscription [illegible] as given by Chishull:—

1. Q. IVLI MVRRANI MELI
NVMAIDCLARITATEM.

2. Q. IVLI MVRRANI STAGIV
MOPOBALSAMATADCAP.

interpreted those inscriptions thus:—Quinti Julii Murrani Melinum, sive ex malis cotoneis oleum, ad claritatem quemcunque, Quinti Julii Murrani stagium opobalsamo permixtum, ad cap[illegible] vehementem utile." In this interpretation Mr [illegible] fallen into more than one important error, as I shall show by considering the two inscriptions

1. MELINUM AD CLARITATEM.—*The Melinum* [illegible] *for clearness of vision.*

[illegible] the collyrium *Melinum* are given by [illegible] collyria he gives formulæ for the [illegible] *Ophthalmicum* (*i. e.* against the [illegible] powerful medicament, fitted for those who could [illegible]

[illegible] mentioned in relation to the origin [illegible] Welch,[2] like Chishull, de[illegible] apples supposing it to be [illegible] And certainly Pliny and [illegible] *melinum*,[3] being made [illegible] flower of the plant is [illegible] use of the eyes. But no [illegible] of the three *Melina*

[illegible] seem to have come [illegible] it was conse[illegible] granulations [illegible] (p. 29) [illegible] *Melinum* of [illegible] several oculist

---

[illegible] gives a formula [illegible] Principes [illegible]

stamps. But again, the same objection holds,—namely, that in none of the collyria *Melina* of Galen was alum a component ingredient.

In his observations, however, upon the different forms of emplastra (and many of which were named Melina), Galen gives a sufficient explanation of the origin of this term as it was applied to plasters; and the same holds, no doubt, also in reference to its application to collyria. According to his own explanation, it was a term significant merely of the colour of the resulting medicament, like the green, &c., plasters and collyria, named *chloron*, *cirrhon*, &c. &c. Gesner, Cooper, and other philologists, lay down *Melinum* as an adjective, meaning yellow; and perhaps the term was originally derived from mel (μελι), honey, honey-coloured. The yellowish tint of the emplastra melina was, as Galen tells us, generally, but not always, derived from their containing verdigris, altered by a moderate boiling with the other component ingredients.[1] The collyria *Melina* of Galen contain ceruse and calamine in their composition.

The *Melinum* is professed, in Murranus' stamp, to be efficacious for the clearing of the eyesight (ad claritatem). The *Melina* collyria of Galen are all alleged by him to have effects conducive to this object, viz., the removing of cicatrices and calli, and every weakness of vision (omnem hebetudinem visus),

2. Q. JULII MURRANI STAGIUM (STACTUM) OPOBALSAMAT*um* AD CAP (CAL*igines*).—*The Opobalsamic Stactum, or Opobalsamic Eye-drops, of Q. Julius Murranus, for dimness or blindness.*

Mr Chishull read Stagium instead of Stactum, the CT of the latter word having been mistaken by him for GI. Mr Forster showed to the London Antiquarian Society,[2] in 1767, a plaster-cast of what was doubtlessly this same Colchester stamp, and gave the reading correctly in the second inscription as Stactum.

The Latin designation Stactum, analogous to the Greek terms Stacton, Enstacton, and derived from the verb σταζω (I drop), denoted any liquid collyrium, applied by drops into the eye—"collyria stactica, hoc est, instillatitia, appellata."[3]

A collyrium, with the appellation *Stactum* or Staticon, is described by Marcellus,[4] Myrepsus,[5] Paulus Aegineta[6] &c.; and Aetius[7] gives

[1] Quemadmodum viridium emplastrorum plurima propter æruginem præpollentem talia fiunt, presertione quæ sunt ex ipsis coloratiora; ita quoque Melina. Sed viridia æruginem incoctam habent, Melina vero coctam quidem, sed mediocriter; nam si amplius coquas, bicolora emplastra quibusdam appellata, quibusdam gilva, efficies. Solent Medici simpliciter viridia, Melina, et rufa, nominare, &c.—*Galen de Compositione Medicamentorum per Genera*, Cap. VI.—Kuehn's Edit., Vol. XIII., p. 503.

[2] See Archæologia, Vol IX., p. 228.

[3] Ætius' Tetrabilias, Cornarius' Edit., p. 435.

[4] De Medicam. Liberi; Principes Artis. Part III., p. 284.

[5] De Compos. Med. in Ib. Part III. p. 660.

[6] Paulus Aegineta's Work. Dr Adams's Translation, Vol. III. p. 551.

[7] Cornarius' Translation, p. 435.

a chapter of collyria under this designation. In this chapter Aetius describes five collyria *Stactica;* and, of these, four contain the Opobalsam[1] as an ingredient, showing the origin and propriety of the term *Opobalsamatum* in the inscription on the seal.

Chishull read the last three letters of the inscription CAP, and thought that the oil was serviceable for head diseases. But if the inscription is not really CAL, the P has been substituted by an error of the engraver for L (CAL), an abbreviation for *Caligines*. The same inscription occurs at greater length on an oculist stamp found at Daspich in France; and in it the *Stactum Opobalsamatum* is professed to remove *Caligines*.[2] And, no doubt, Murranus, of Colchester, vended, of old, his Opobalsamic Eye-drops for the same alleged purpose. This quality of "visum acuens" is attributed to two out of the four forms of Opobalsamic Eye-drops mentioned by Aetius. The Stactum is (avers Myrepsus) "ad acumen visus mirabile admodum."—P. 660.

BOSTON MEDICAL LIBRARY DEC 23 1886

In a second and concluding Part, I shall describe other Roman medicine-stamps found in these islands; speak of the uses, &c., to which they were applied; and point out some other relics referring to the former existence of Roman medicine and Roman medical practitioners in Great Britain.

Part Second.

REVIEWS.

The Pharmacopœia of the King and Queen's College of Physicians in Ireland. 1850. Dublin: Hodges and Smith.

"The New Pharmacopœia of the London College is prepared, but is not yet published."—*Pharmaceutical Journal and Transactions*, for December 1850.

A QUARTER of a century is a long time for science to be allowed to advance in these onward days, without its progress being acknowledged in a National Pharmacopœia. The second edition of the Dublin Pharmacopœia appeared in 1826; and twenty-four years have elapsed before its re-appearance in a fresher form,—an interval so long, that it must have ceased in a great measure to be the guide alike of the physician and of the druggist. But the College can scarcely be blamed for this. In Dublin, as everywhere else, an amiable attachment to classical prejudices and ancestral custom, clothed the pharmacopœia in the dress of a dead language, which

[1] Opobalsam, the "succus a plaga" of the Syrian balsam tree. *See Pliny.* Lib. xii., c. 25.—Dioscorides, in describing its origin, effects, &c., specially recommends it as a detergent application in dimness of sight (quæ pupillis tenebras offundunt, exterget.) Lib. i., cap. xviii., p. 18.

[2] The inscription on the Daspich stone is "Q. Valleri Sexti Stactum ad Caligines Opobalsamatum."

had long ceased to be the language of every other branch of medical science and art throughout Europe. But the medical public, practitioners no less than druggists, chose to have the book in the vernacular; and, accordingly, unauthorised English translations, supplanting the Latin original, doomed it to become a permanent load on the shelves of the publisher. Thus it has happened, that the character of the Colleges, the progress of pharmacy, and the convenience of the medical profession, have necessarily been all sacrificed to the more direct interests of a bibliopole.

From these trammels the College of Edinburgh, setting an example to its neighbours, freed itself in 1839; and the consequence was, that a reprint of the first English Pharmacopœia was called for in the short space of two years. The Dublin College has now followed the same course, having apparently no fear that its scholarship will be called in question, because it declines any longer to publish its ordinances in what is at best but a semi-barbarous Latin. The London College in 1836 observed a middle course. The custom of two centuries and a-half was not to be abruptly departed from there. But the chemist, whom it could not find within its own body to get up its pharmacopœia, was encouraged, or allowed, to bring out almost simultaneously an English and commented edition. No little curiosity must be naturally felt as to the intention of the College in this respect, with the "New Pharmacopœia, which is prepared, but is not yet published." The less austere influence, which may be expected to move its greatly changed constituency in 1851, ought to enable it to throw aside the remains of a monkish prejudice. And if it has not already done so, what may not be looked for from the frank example now set by its sister in Dublin?

The adoption of an English version is not the only radical change in the Dublin Pharmacopœia of 1850. The College has also boldly ventured on another, not less difficult to adopt, and much more so to carry into execution.

It is well known that when the legislature mercilessly overthrew in 1826 the innumerable incongruous weights and measures used in various trades throughout these realms, it spared the goldsmith and the apothecary, and left them to their own devices. The latter has taken ample advantage of the license. Following substantially his ancient usage, he dispenses his drugs with a scruple of 20 grains, a drachm of 60, an ounce of 480, and a pound of 12 such ounces; while the grocer, butcher, baker, and all other tradesmen, except the goldsmith, use a drachm of $27\frac{1}{3}$ grains, an ounce of $437\frac{1}{2}$, and a pound which, containing sixteen such ounces, is about a fifth heavier than that of the apothecary. But this is by no means all. If the apothecary's weights could have been rigorously followed in the practice of pharmacy, and all others prohibited, as the London College in the exercise of its stringent prerogatives at one time attempted to do, no very great inconvenience perhaps

might have been felt, except by the scientific chemist, who would necessarily be always impeded in his course by a double set of weights, whenever he had occasion to enter within the confines of medicine. The apothecary, however, could not be restricted to the use of his own weights. He must buy from the wholesale dealer, manufacturer, or merchant, by the imperial weights. He must therefore possess imperial weights to check his purchases; and thus he introduces into his establishment a palpable source of error and abuse. One singular abuse has consequently arisen. If a physician send his patient to a druggist for an ounce of Epsom salt or cinchona bark, without a prescription, the druggist supplies him with an imperial ounce of 437½ grains. But if he carry with him a prescription, say for a solution of Epsom salt with a few drops of sulphuric acid, the druggist in that case uses his apothecary's ounce of 480 grains. As for errors arising from two systems of weights being kept in the same establishment, the conscience of the apothecary's or druggist's apprentice can alone tell their amount; because, fortunately, the difference between the two systems of weights, as to the value of the denominations chiefly used, is not such as to entail a very appreciable difference of effect upon the patient.

Impressed by these irregularities, and encouraged by the opinion of some of the most experienced retail druggists in Edinburgh, the College of that city in 1839 declared itself as having "never been able to see the force of those objections which prevented the introduction of the imperial system of weights into the practice of medicine and pharmacy, on the occasion of the late reform in the national weights and measures" (Edin Pharm., 1839, Preface, p. 12). But the College resisted the temptation to innovate on so radical a point, being withheld by unwillingness to introduce a new element of contention into the negotiations, which, there was some reason to hope, might be renewed between all the colleges for agreeing together in adopting an imperial pharmacopœia. The Dublin College has had no such check on its proceedings; for every hope of beholding a conjunct pharmacopœia, for at least a long time to come, must have ceased in every quarter. Accordingly, the new Dublin Pharmacopœia, discarding the ancient weights of the druggist, adopts those of other trades,—with the single difference, that, instead of the imperial drachm of 27⅓ grains, a new drachm is invented of twice that amount, or 54 $\frac{7}{10}$ grains, so as to correspond more nearly with the old apothecary's drachm of 60 grains.

The College has already been assailed for this innovation. But the only objection hitherto made is not likely to breed much repentance on its part. A London druggist of repute complains, that in England there is "no means of correctly representing the Irish drachm and scruple in dispensing, without having new weights made; which would be objectionable on account of the additional complication it would introduce into our system

of weights" (Pharm. Journ. and Trans., December 1850, p. 273). The practical difficulty here is not to be denied. But on the most dispassionate consideration of it, a doubt will arise whether the Dublin College of Physicians, in suiting its pharmacopœia to the wants and wishes of Ireland, is bound to consult the convenience of London druggists. The more superficial objection, that inveterate habit may lead practitioners or dispensers to commit the error of using the older and heavier drachms and scruples, is answered by the College in its preface. It is futile to deny that it is of no consequence to medical prescriptions, "if those practitioners, who do not take the trouble of consulting this work, should, in ordering a drachm or a scruple of some active drug, be administering a little less (or more) of it than they intended,"—the whole possible difference being only an eleventh part. Neither is it a very serious difficulty that there already exists in the national imperial weights a drachm of only half the value of the new Dublin apothecaries' drachm; because the imperial drachm of 27½ grains is so little in use, that few practical men in any trade are aware of its existence. There remains a fourth difficulty to dispose of,—namely, that the new scruple, drachm, and ounce are not multiples by entire numbers of the Troy grain. This is a difficulty which the College cannot get over, as it has endeavoured to do, by the simple assertion, that "it cannot consider it a matter of any practical importance" (Preface, p. 23). Is it not an important practical difficulty, that, when a physician has arranged in his mind a dose of some composite medicine, which is to be frequently repeated, he must be subjected to the inconvenience and risk of calculating without the aid of entire multiples, how many scruples, drachms, or ounces must go into his prescription, for the necessary repetition of what is represented in grains for a single dose? Or if he impose the calculation on his druggist by his cabalistic *Mitt. tal. vi.*, or whatever the number may be, what confidence can he put in the dispenser habitually doing that right, which he is himself afraid of doing wrong? It will be answered, however, that in such circumstances the physician and the druggist may adhere to grains in the calculation, discarding higher denominations of weight altogether. But then arises irresistibly the question,—Whether these higher denominations are essentially necessary for any purpose? In point of fact, nothing but inveterate custom can be pleaded for the retention of any below the value of an ounce, except the grain. And it may be confidently predicted, that should the Colleges of London and Edinburgh follow the example set them in Dublin, and substitute imperial for apothecary's weights, the medical profession, whatever pains may be taken to devise ingenious drachms and scruples, will disown these inventions, and be content with grains for actual service. Meanwhile, every pharmacologist will feel deep interest in the fate of the experiment now fairly begun in Ireland.

The Dublin Pharmacopœia of 1826 was prepared too soon to

take advantage of the reformed measures of the Act of Parliament of the same year. But the new edition follows the example first given by the London College. All the pharmacopœias, therefore, now adopt the imperial measure, according to which the gallon consists of eight pints, each of twenty ounces avoirdupois; and the inferior denominations are the arbitrary but convenient subdivisions first proposed in the pharmacopœia of London. All that need be said under this head is, that the Colleges are probably right in following the system fixed on by the Legislature; but that the Legisture is clearly wrong in one material article of it. An imaginary advantage on the part of the number 8, as a divisor, led to the division of the gallon into pints of twenty ounces each,—an exorbitant measure, which few can cheerfully swallow even after twenty-four years' trying. Had the very convenient sub-multiple 10 been chosen, the country would have had a pint but little different from those previously in use everywhere, corresponding exactly with the imperial pound, and such as pharmacy would have adopted without a murmur.

The nomenclature of pharmacy has been felt no less a stumbling-block by the College of Physicians of Dublin than by the other colleges. It has contrived to steer a middle course between the parti-coloured pharmaceutic names of the Edinburgh pharmacopœia and the sternly scientific ones of that of London. Thus, it follows the former, not only in the terms *Alumen*, *Lithargyrum*, *Calomelas*, and *Sublimatum corrosivum*, but likewise in the less ancient yet unchemical designations of *Liquor arsenicalis*, *Ferrum tartarizatum*, *Antimonium tartarizatum*, *Acidum muriaticum*, *Ammoniæ murias*, *Morphiæ murias*, *Quinæ sulphas*; and even adds to these its own *Hepar sulphuris*, *Cupri subacetas*, and *Bismuthi subnitras*. But, with a singular and incomprehensible impartiality, it imitates the London College in now, for the first time, adopting such phrases of the newest chemistry as *Barii chloridum*, *Ferri sesquichloridum*, *Acidum arseniosum*, and even those barbarous London inventions *Ammonio-chloridum*, *Ammonio-citras*, *Ammonio-sulphas*. This is a theme, however, on which criticism is dumb. Happy is he who can be satisfied with any pharmaceutic nomenclature now practicable. If there be any single topic which calls more loudly than another for a pharmaceutic convention of the three Colleges, it is the nomenclature of chemical drugs. Let the convention be based on only a single condition,—that the names chosen shall be permanent,—and great good cannot fail to result. One inevitable consequence, indeed, would be the abandonment, sooner or later, of the strict language of chemical science, which the London College perseveres in following as its guide. But surely that learned body must now acknowledge this surrender to be a crying necessity. The language of scientific chemistry is tottering in its most essential components. The universally received doctrine of the hydracids has already for some time involved a more philosophical nomenclature

for them; and the terms *Hydrochloric*, *Hydrocyanic* and *Hydrosulphuric acids*, had barely been sanctioned by the London pharmacopœia when they became the *Chlorhydric*, *Cyanhydric* and *Sulphydric acids* of French scientific chemistry. The change which has arisen in the views taken by the majority of chemists, relative to the combining equivalents of some metals, overturns the received names of many of their compounds. The *Sesquioxide of iron* must presently become the *Peroxide*, the *Sesquisulphuret of antimony* the *Tersulphuret*; and while London physicians prescribe calomel and corrosive-sublimate as *Chloride* and *Bichloride of mercury*, a fatal chemical necessity will soon compel the druggist to know the former also by the name of *Dichloride*, and the latter by that of *Chloride*. And if the recent doctrine, that hydrogen is the only acidifying element, gain converts in proportion to its philosophical attractiveness, who can tell how soon chemistry may cease to know even such household words as *Sulphuric*, *Nitric*, and *Phosphoric acids*? Already they are the *Sulphionide*, *Nitrationide*, and *Phosphionide* of *hydrogen* of some reformers. Why not speedily the *Sulphionhydric*, *Nitrationhydric*, and *Phosphionhydric* acids, if the French nomenclature of the recognised hydracids be sanctioned?

The arrangement of the new Dublin pharmacopœia has been censured, and certainly seems open to objection. Whether it be regarded as a work of reference for the practitioner, the druggist, or the student, it is inconvenient that the chemical compounds, instead of being arranged according to their active bases, for example, as compounds of iron, potash, lime, &c.,—should be scattered throughout the book according to the elements or compound bodies with which the bases are united,—that is, grouped as oxides, chlorides, sulphates, and so on. But this is a question of taste, as to which competent judges may differ without serious imputation upon their discernment. Not so in regard to the last topic, to which we must confine ourselves in the present criticism.

If there is any one object which more than another every college of physicians is bound to keep constantly before their eyes, it is the necessity of accommodating themselves to one another in their pharmacopœias. An Imperial pharmacopœia, which was aimed at about sixteen years ago, and we believe consistently and earnestly aimed at by at least two of the colleges, is one of those blessings which the present generation is doomed to behold only in its dreams. But in the meantime it must greatly contribute to the comfort of practitioners in the three divisions of this empire, and still more to that of chemists and druggists, if the several Colleges will endeavour to assimilate the pharmacopœias to one another. And it is our honest conviction that, unless by such progressive assimilation, the way never will be cleared for a conjunct pharmacopœia. The Dublin College has not lost sight of these considerations. In many details, the data of the other colleges have been adopted. So far as we can see, this might have

been done more frequently, with great convenience, and without any sacrifice of independence or dignity. But at the same time we must frankly acknowledge that a reasonable spirit of accommodation may be traced throughout the new Dublin pharmacopœia; and in return, we feel bound to confess that a new Edinburgh pharmacopœia might usefully profit, as it has done in former times, by adopting several improvements now introduced by its rival.

An English critic has intimated,—whether querulously or approbatively we cannot discover,—that the framers of the Dublin pharmacopœia, in endeavouring to assimilate it here and there to the others, have shown a greater leaning to that of Edinburgh than to that of London. There can be but one reason for this; since every natural predilection would operate towards an opposite result. The fact, therefore, ought not to be without its legitimate influence on the London College, whose pharmacopœia is now "prepared, but not yet published." After what occurred during the negotiations between the Colleges of London and Edinburgh, about sixteen years ago, for bringing out a *Pharmacopœia Britannica*, we cannot but feel that the lesson thus conveyed was needed. It luckily so happened that the negotiations were broken off, or, as was at the time believed, were interrupted, for reasons which reflected no discredit on either body. But without a more compromising spirit than was shown in London on that occasion, the proceedings could not have been carried forward much longer; and until a better spirit be cultivated there, a conjunct or Imperial Pharmacopœia is an impossibility. The London College, however, is now very differently constituted from what it was in 1834; other professional views, besides those which then reigned rampant in the bigoted old body, have now their able representatives among its fellows. It can boast, too, of more than one cultivator of the materia medica of undoubted authority. Its more liberal modern policy, evinced on sundry late occasions, can scarcely fail to penetrate into its pharmacopœia committee; and we therefore look forward with some confidence, yet not altogether without anxiety, to the promised result of their labours.

Having been able to view thus far, with almost unmixed approbation, the leading features of the Dublin pharmacopœia, it is with regret that we feel compelled to notice what appears to us a serious defect. With the recent example of both British Colleges before it, and the powerful aid of their prior investigations, the College of Dublin has nevertheless shrunk from the task of preparing a series of formulæ for detecting the adulterations of drugs, and has left the list of the Materia Medica, as before, a bare catalogue of references. It is impossible to doubt that great practical benefit has resulted to pharmacy from the course adopted in the London and Edinburgh pharmacopœias. If adulterations are still practised with an unsparing hand by foreign merchants, domestic manufacturers and wholesale dealers, practitioners and druggists have at all events

been put upon their guard. They have been supplied with the readiest means of protecting themselves from imposition and disappointment. Some important adulterations have entirely disappeared; and some important drugs, which previously could not be obtained in a pure state, may now be had of genuine quality without difficulty. This beneficial change has been owing in part to the better education of druggists, but chiefly, we apprehend, to the promulgation of the great frequency of the practice of adulterating drugs, and the means of detecting the offence. This branch of the pharmacopœias may indeed be regarded as one of the most practically useful objects to which the attention of the Colleges can be directed, now that the preparation of drugs has been taken almost entirely out of the hands of practitioners and retail druggists, and has fallen into those of wholesale dealers and manufacturing chemists, who are independent of the jurisdiction of the colleges, and work independently of the college rules. Something was done for this department of pharmacy in the London pharmacopœia of 1836, and much more in that of Edinburgh of 1839 and 1841. But ample room still remained for correction and improvement; which the Dublin College might have undertaken, to its own credit, and to the advantage of practical pharmacy.

In a subsequent number we shall look a little into the execution of the details of the work. On a cursory survey, we fear that a closer inspection will find not a little room for censure.

Operative Surgery. By FREDERIC C. SKEY, F.R.S. 8vo. London: 1850.

THE author of this work does not give any information on the title page as to his opportunities of acquiring information, or his position in the profession. But upon turning to the "London Medical Guide," we find that he is Assistant-Surgeon of St Bartholomew's Hospital, and a Member of the Council of the College of Surgeons. We therefore conclude, that this handsome, well-got-up, volume of 722 pages may be regarded as an exposition of the present practice of surgery in London, proceeding from the most authentic source, and entitled to the fullest confidence. The pleasure and profit which we had derived from perusing the works of Pott, and Ramsden, and Abernethy, and that distinguished surgeon who still sheds the lustre of his talents on the wards and lecture-room of St Bartholomew's—Mr Lawrence—prepossessed us in favour of any practical information emanating from this hospital, and we proceeded to open Mr Skey's book with every disposition to be gratified and instructed. But, alas! it soon appeared that our anticipations were doomed

to a grievous disappointment, and that, instead of new ideas or clear descriptions, which might be recommended to the attention of our readers, we were to meet with nothing better than unmeaning trivialities, interspersed with old exploded notions, and imperfect views of modern improvements. Lest this sentence should seem hasty or unduly severe, we may give some specimens in illustration of the grounds upon which it was formed.

In entering upon his subject, the author begins at the beginning, and dilates upon the importance of the operator being provided with a good knife, of which he thus represents what he conceives to be the most perfect form. We are at a loss to imagine from what musty case of old instruments the original of this hideous portrait was extracted. The rough surface, and disproportionately large size of the handle to the blade, which every one conversant with the use of instruments knows to be inconsistent with any nice manipulation; and the uncouth form of the edge, which, Mr Skey says, should for the last inch be nearly parallel to the skin when the knife is employed in cutting, constitute a combination of objectionable qualities such as we have seldom known equalled, and certainly never surpassed. But, while wondering how any delicate dissection was to be executed by a knife that could not cut less than an inch at a time, we came upon the following paragraph on "silver knives," which we must place entire before the reader, lest he should regard any less authentic statement of it as a libel on the surgery of St Bartholomew's.

"It is not always desirable that the edge of the knife be perfectly keen. The fact that a wound formed by laceration pours out less blood than another caused by incision with a sharp knife, has been long known to our profession; and there is no reason why this principle in the economy should not be turned to advantage by the operating surgeon. Whether for the exposure of large arteries, or the removal of large tumours, or in hernia, the escape of blood is one of the chief obstacles to the progress of the operation; and when the quantity is large it ceases to be a mere inconvenience, and becomes a serious evil. Much bleeding from small vessels may be avoided by the use of a blunt knife, which lacerates coarsely instead of minutely. I was indebted to Sir Benjamin Brodie, in the early part of my own career as an operative surgeon, for this useful hint; and I have seen the benefit derived from the employment of a knife with a silver blade too frequently to hesitate in recommending it as a valuable appendage to the operating case."—Pp. 33, 34.

We lately felt it necessary to express our regret for the countenance which Sir B. Brodie had bestowed on the employment of pressure instead of the ligature for the treatment of popliteal aneurism; but now, when it appears that the question lies between exposing the femoral artery, by tearing through its connections with *silver* knives, and compressing it with Dr Bellingham's "clamps," we are not in the least surprised at the preference given to the latter.

It might be expected that wounds inflicted after a fashion so extraordinary, would exhibit some remarkable peculiarities in the mode of their dressing, and here the reader will not be disappointed. Mr Skey adopts the treatment by stitches and moist applications, without any acknowledgment of the source from which it was derived; but forgetting its great fundamental principle, that the edges of a wound should not be so approximated as to prevent the discharge of blood, which issues from them in the first instance, he, when most desirous of promoting complete union, introduces so many points of suture as must seal the edges more closely together than they could be maintained by the most adhesive of plasters. He says:—

"I employ, both in ordinary and in Taliacotian operations, the finest needle, slightly curved at the point, with silk thread to correspond; of these two or three may be introduced, or even more, should the length of the wound justify it. The puncture with the needle should be made at not more than a line's breadth from the edge of the wound, brought out in the wound, and again introduced, its point finally appearing at the same distance on the opposite side."—P. 45.

In illustration of this doctrine a woodcut is given, to show a wound of the face, about two inches in length, stitched *secundum artem*. By the assistance of a magnifying glass, we make out fifteen (!) sutures in this extent, for which three, or at the very most four, would have been quite sufficient; and our practical readers may determine how far it is likely that such an amount of local irritation as is here proposed could avoid causing inflammation, subversive of primary union, or that the small portion of skin included by the threads could escape ulceration.

In selecting particular subjects as specimens of this author's principles and practice, we might, with equal propriety, take any page of the book, and therefore beg that the following examples may not be regarded as remarkable for any special degree of absurdity.

The operation of lithotomy is elaborately discussed, with the usual allowance of startling principles:—

"We are directed not to divide the bulb. Why not divide the bulb? It is an unimportant structure, and may be cut with impunity. The bulb was always necessarily divided in the Marian operation of dilatation. Yet we hear nothing of hæmorrhage. We are told to beware of the artery of the bulb. Whence this caution? The artery of the bulb is scarcely larger than a digital artery, which we rarely tie when divided. In order to avoid these two objects, we are directed to introduce the finger into the wound, and press the bulb and rectum out of the line of division. Who ever saw the finger introduced into the wound for this purpose, or, indeed, who ever had a passing thought of either the bulb or its artery, from the moment of his first incision."—P. 547.

After this statement, the reader will not be surprised, when told that the best "line of operation" is to "cut straight backwards through the side of the bulb" (p. 548). Now, anything more dangerous, or rather more certainly destructive, than such a direction, it is difficult to imagine. The bulb may be divided with perfect

safety in the mesial plane; but if opened at the side, is almost certain to have its artery wounded, and this vessel, though not very large, is so situated as to pour out a great quantity of blood, unless restrained by pressure, which again exposes the patient to extravasation of urine, with all its attendant horrors.

Next to lithotomy comes "Lateral Curvature of the Spine;" for the *cure*—not the prevention—of which, the following ingenious procedure is recommended:—

"A very useful agent of treatment, which I very frequently employ, consists in a weight applied on the head, in the form of a ring or coronet, containing shot, varying in weight from four to eight or ten pounds. The weight may be sewn in a tube of strong linen or ticking, and tied under the chin. So long as the weight is worn on the head, the facility of transmitting it to the ground without effort, almost compels an upright position of the trunk, and it may be worn, whether during exercise or while sitting, for many hours during the day. It is this habit of carrying weights on the head that renders people accustomed to this practice so particularly upright, which is rendered so manifest in market-women and soldiers. In fact, it is the only position of ease, for the moment the body is inclined from the perpendicular, the muscles are brought into action in order to support it. If the patient, divested of her clothes down to the loins, be placed in the sitting position on a stool, with the weight applied on the head, and desired to carry the head laterally and circularly in all directions, the influence of these movements on the spinal muscles will be readily observed, and this, indeed, forms as good an exercise as I can suggest, particularly if the patient be required to bend the head somewhat forwards."—P. 566.

It thus appears that when the spine has not been able to support the head and arms, the author proposes to render the column straight by placing ten pounds weight of lead upon the patient's head!!

In regard to the cure of "Fistula in Ano," Mr Skey says:—

"This end may be attained by means of a thread passed through the fistula, when open at both extremities, and tied loosely in a knot. The thread may be allowed to remain for some days, or even longer, if the sinus do not appear conscious of its presence. When removed, the canal will heal rapidly, particularly under the administration of bark. When the fistula is complete, and the knife is to be rendered the agent of cure, a director is passed into the sinus, and forced upwards to the rectum, upon which a probe-pointed bistoury is conveyed; the index finger, sufficiently greased, receives the probe point within the gut, and the bistoury and finger are withdrawn together. This act is facilitated by the integuments of the buttock being drawn tightly outwards by an assistant, at the moment of withdrawing the knife, by the application of both his hands.

"When the inner orifice is incomplete, a flat and smooth piece of soft wood should be used, instead of the finger, and introduced into the rectum. A sharp-pointed bistoury is employed, instead of the probe-pointed instrument. The point of the bistoury is passed through the mucous membrane and fixed in the wood, and the two are then withdrawn."—P. 520.

Here we have a surgeon, writing in the year 1850, who obviously knows nothing of the facts established by M. Ribes, thirty years ago, as to the constant presence, and fixed position, of the internal opening,—the limitation of the incision to this extent,—and the inefficiency of all means of remedy that do not effect division of the septum thus far,—and who is, consequently, as profoundly un-

acquainted with the modern treatment of fistula as if he had been a bat, quietly sleeping for the last quarter of a century under the old eaves of St. Bartholomew's.

But enough, and more than enough, has now been said to show that this book, so far from aiding in the promotion of surgical science, is not even trustworthy as a guide for discharging the routine duties of practice. If the surgery taught here be a specimen of that which the College of Surgeons requires from candidates for its diploma, we should indeed be surprised were the general practitioners of England not to betray some uneasiness under the yoke of Lincoln's-inn-fields monopoly.

Der Aderlass in der Lungenentzündung, Klinisch und Physiologisch erörtet. Von Dr Joseph Dietl. 8vo. Pp. 128. Wien: 1849. —[*Bloodletting in Inflammation of the Lungs, clinically and physiologically considered, &c.* Vienna: 1849.]

In many former Numbers of this Journal, allusion has been made to changes upon the treatment of acute diseases, traceable to the influence of modern pathology. The comparative neglect into which the old-fashioned remedy of venesection has fallen in these days is due, as we think, to many causes,—to the indiscriminate activity with which it has too often been applied in combating all *phlogoses*,—to the unequivocal mischief which has resulted from such irrational practice,—to the difficulty which undoubtedly attends the discrimination of the precise cases to which it is specially adapted,—to mistakes in diagnosis (still very numerous, notwithstanding the introduction of the stethoscope and plessimetry as aids in our investigations),—to changes in the type of certain diseases, or of the epidemic influences of our seasons,—to premature attempts to substitute for the experience of ages the obscure suggestions of theory or of fashionable pathology,—to falsely-styled facts, embodied in rotten statistics,—and to erroneous inferences, drawn from what is real and significant. In no part of Europe is the horror of the lancet more conspicuous than in Vienna, where, if report speaks truly, a too exclusive devotion to the study of morbid anatomy, and to the perfection of physical diagnosis, has induced, even among the most eminent professors of the school, a species of medical scepticism, more favourable to the institution of unwarrantable clinical experiments, than to the discovery of a truly rational system of therapeutics. It is a fact not easily accounted for, that the German soil is favourable to the growth of all sorts of quackeries and visionary systems, such as homœopathy, hydropathy, and mesmerism. These, as if by some mystic charm, impose upon the speculative, under the guise of science, to the exclusion of the results of experience, and suggestions of sober reason; which, if not supported by evidence

almost mathematical, are simply disbelieved and denounced as mischievous fallacy.

These introductory observations will prepare our readers for a brief examination of what is practical in the work of Dr Dietl. In a clinical lecture upon the appropriate practice in cases of pneumonia ("Monthly Journal," New Series, vol. ii., p. 163), Dr Alison, while alluding to the alleged success of the expectant, or, what is quite the same, of the homœopathic system pursued at Vienna, makes the following remarks upon the only kind of statistical evidence which would satisfy a logical mind as to the comparative expediency of ordinary and expectant practice :—

> The only perfectly fair way of determining this question by statistics would be, to have a certain number of patients treated from the beginning, in inflammatory diseases, by practitioners accustomed to the use of blood-letting in such diseases, and with the symptoms which they have been accustomed to regard as indicating it, by the expectant practice only; and then an equal number treated by the same practitioners, likewise from the commencement, by the depleting remedies; and farther, we must have perfect confidence, if not in the judgment, at least in the good faith, of the practitioners using (and not abusing) these active remedies. To such a trial I shall only say, that I should not wish myself, nor advise any of my friends, to be a party; and the moral reasons which make me say so must, I think, for ever prevent any such statistical experiment being really tried by any trustworthy practitioner."

The moral scruples here alluded to seem to have had little weight with Dr Dietl, if indeed they ever suggested themselves to him. By education and prejudice favourably disposed towards the use of the lancet in pneumonia, he, during the first years of his practice, bled all the patients who presented serious symptoms of inflammation of the lungs, and sought his assistance. As he makes no mention of any auxiliary measures, of the amount of blood usually abstracted, nor of the indications which guided him in directing repetition of the operation, it is assumed that Dr Dietl, a disciple of Raiman, when first emancipated from the schools, practised phlebotomy with no sparing hand, as the *sole* and grand cure for all *severe* cases of pneumonia, without reference to age, sex, stage, or epidemic influence. This inference is strengthened by a perusal of various portions of the work before us; its correctness is, however, in no way essential to the validity of certain statistical objections, fatal, as we shall presently show, to the most important conclusions of the author. In 1831, 1832, and 1833, he so far departed from his usual practice as to commence the treatment of some *severe cases* on homœopathic principles; but courage failing him, as he saw the progress of the disease, the experiment was very properly abandoned, and the pupil of Raiman betook himself to his lancets to complete the bungled cure. The history of these homœopathic experiments must not be lost sight of. It seems to fill what would otherwise be a blank in the author's description of his early practice, and strengthens our belief that he was in the habit of resorting to bloodletting even in advanced cases. (See also "Aphorisms," 76 and 78.)

Dr D. next tried tartar emetic in contra-stimulant doses; and, after he had made the remark that this drug seemed to induce amendment, even when no physiological effect was manifested, came to the conclusion, that the remedy had no power whatever over pneumonia.

"A change came o'er the spirit of his dream,"—bleeding and antimony were alike discarded, and Dr D. coolly determined to go to the fountainhead for information,—to trust all his pneumonic patients in future to the operations of nature and low diet, with some simple ptisan, and to watch the sequel, which could not fail to be highly interesting and instructive. He now presents the profession with the results of a three years' perseverance in this hopeful plan, contrasting them in a variety of ways with the consequences of his earlier practice, and introducing a number of physiological and pathological observations of most startling and untenable novelty, which will hardly pass for current coin in this part of the world. As we should be sorry to do the author the injustice of concealing any part of his doctrine—as the work seems to have been received on the Continent as a valuable contribution to practical science, and as the nature of its contents has not yet been fully exposed in the medical journals of this country—we give the aphorisms, the cream of the whole, unabridged, and accurately translated, so far, at least, as our feeble perception of the author's meaning, when he waxes pathological, will allow:—

Aphorisms.

1. The first stage of pneumonia lasts longer in young strong individuals, attacked with the disease for the first time, than in old weak subjects, who have repeatedly suffered from similar attacks.

2. Venesection may shorten the first stage (literally, *premonitory stage*).

3. The hyperæmia of the lung which precedes inflammation has no essential connection with the dyspnœa of pneumonia.

4. The patient respires with more freedom when hepatisation has taken place than before it.

5. The dyspnœa of pneumonia is not caused by the obliteration of the air-cells by plastic exudation.

6. Extensive hepatisations certainly confine the respiratory movements, but do not induce a corresponding amount of sensation of breathlessness.

7. The mechanical obstacles to respiration are neither the sole nor the essential causes of the dyspnœa of pneumonia.

8. The interchange of gaseous elements which takes place in the lung-cells is the fundamental condition on which depend the normal activity of the nervous centres and the movements of respiration which these regulate.

9. The pneumonic process so affects and alters the forces, in virtue of which the interchange of gases is brought about, that it greatly interferes with the usual interchange of the carbonic acid of the blood and the oxygen of the atmosphere.

10. In pneumonia, there exists an increase in the proportion of the fibrin, and a diminution in the corpuscular element of the blood.

11. In consequence of the excessive production of fibrin—an oxide of protein—a considerable amount of oxygen is withdrawn from the blood; and as the blood corpuscles, whose special office it is to convey oxygen, are diminished, the blood is not supplied with oxygen in the same proportion as it is wasted.

12. In pneumonia, less oxygen is abstracted from the atmosphere than is necessary for the oxidation of the venous blood and animation of the nervous centres.

13. The dyspnœa must be greatest while the fibrin is being secreted in the lung-cells; for at this period the consumption of oxygen, and consequently the need of oxygen, is at its height.

14. Dyspnœa must exist even in the premonitory stage, for the blood-changes, at least the diminution of corpuscles, commence at this period.

15. The dyspnœa of pneumonia is more especially due to the altered chemical conditions of the blood.

16. Venesection induces the same conditions of the blood, which the pneumonic process effects; it must, consequently, in a chemical point of view, rather increase than diminish dyspnœa.

17. Venesection acts in relieving the dyspnœa of pneumonia by favouring the continuous flow of the circulation,—hence permitting more blood to traverse the capillaries of the lungs in a given time, whereby the process of oxidation is facilitated, and the nervous system animated.

18. No other remedy relieves pneumonic dyspnœa so remarkably as bloodletting.

19. In cases of pneumonia left to nature, the perspirations are never so profuse as when the lancet has been used.

20. Many cases, if left to nature, run their course, and get well without perspiration. In all cases, the cure is the surer and quicker the less the patient sweats.

21. The sweating is a result of weakness, or of venesection, and has no critical significance.

22. When the exudation is completed, the pulse returns suddenly to its normal frequency.

23. The acceleration of the movements of the heart in pneumonia is neither due to mechanical impediments nor to loss of blood.

24. The exudative process, or the particular "crasis" on which it depends, is the special cause of the accelerated pulse.

25. The frequency of the pulse is diminished after venesection, or even while the blood is flowing.

26. The effect of bleeding on the pulse is only transitory.

27. The diminished frequency of the pulse which follows blood-letting is, like the relief of the dyspnœa, caused by the promotion of the regular blood-current, by the animation of the nervous centres, and consequent regulation of the heart's action.

28. Venesection can lower the frequency of the pulse only in individuals who are plethoric enough.

29. Venesection should never be repeated when the pulse becomes quicker instead of slower after the first operation.

30. In the treatment of pneumonia by bloodletting, the frequency of the pulse does not immediately subside when the exudation is completed, but continues to be observed for some time afterwards.

31. Tumultuous action of the heart is not so common when a case of pneumonia is trusted to nature, as when it is treated by venesection.

32. Tumultuous action of the heart is partly caused by venesection.

33. The pulse of pneumonia is dicrotous, but not large, like that of typhoid fever.

34. The compressed pulse of the old pathologists is to be regarded as normal and auspicious; the large dicrotous pulse as abnormal and inauspicious.

35. After one or more bleedings the pulse becomes large and dicrotous; *it develops itself and becomes free*, as the phrase goes.

36. This change upon the pulse is a sure sign of an increase in the serosity of the blood, and of adynamia.

37. It is a bad sign when the pulse becomes free after the first bleeding.

38. In the treatment by bloodletting the large dicrotous pulse does not so quickly pass into the small normal pulse, as when dietetic treatment is alone trusted to.

39. There is more thirst observed in the course of the treatment by bloodletting.

40. Venesection relieves, along with the dyspnœa and rapid pulse, the sensation of exhaustion and the muscular weakness; this service is not rendered by the expectant treatment.

41. The yellow tint of the skin subsides remarkably in cases treated by the expectant measures, on the completion of the exudation, and in a very short time passes off completely.

42. The yellow tinge is comparatively rare in cases treated on the expectant plan.

43. The yellow tinge increases after every bleeding.

44. It is in many cases caused by bleeding.

45. In cases subjected to the expectant treatment, the physiognomy of the patient, when exudation is completed, indicates the most perfect comfort.

46. In cases treated by bleeding, the improvement of physiognomy is not so remarkable, for the weakness caused by venesection prevents the patient from enjoying his convalescence.

47. In expectant practice appetite soon follows the completion of the exudation, but seldom reaches the degree of ravenous hunger (Heisshunger).

48. In the treatment by bleeding, appetite does not so soon follow the completion of exudation, but it often reaches the degree of voracity.

49. The short dry cough of pneumonia is due to hyperæsthesia of the lung.

50. The most common cause of the cough is bronchitis.

51. There are cases of pneumonia without cough.

52. We have unequivocal evidence that a single bleeding often diminishes or altogether removes the cough of pneumonia.

53. This result is effected by diminishing the pulmonary hyperæsthesia and the bronchial secretion; hence the remedy acts as in diminishing the dyspnœa.

54. Expectant practice offers no means of alleviating the cough of pneumonia.

55. The most benign (besten) pneumonias are those in which there is but little expectoration.

56. When, in expectant practice, the completion of the exudation is at once followed by a cessation of the expectoration, it is a favourable sign, indicating the probability of rapid resorption.

57. The expectoration of an albuminous fluid, setting in from the eighth or ninth day, at first liquid, afterwards ropy, and intermixed here and there with puriform streaks, effected by short fits of coughing, and in the course of the expectant treatment of extensive pneumonia, is to be regarded as a normal and favourable variety of sputum.

58. In the treatment by bloodletting the sputa undergo several changes, among which the transition of the tough clear sputa into the so-called sputa cocta, is the most important.

59. Venesection favours the conversion of the pneumonic exudation into pus or into puriform cells.

60. Sputa cocta are very often produced by bleeding.

61. Sputa cocta are neither necessary nor critical evacuations.

62. The most benign pneumonias are those in which no urinary sediment is observed.

63. The less the sediment in the urine, the quicker will be the resorption of the pneumonic exudation.

64. Pneumonias treated on the expectant plan often run their course without urinary sediment; in those treated by bleeding, copious urinary deposits are common.

65. These deposits are not critical evacuations, but are products of organic decomposition.

66. They are induced artificially by bloodletting.

67. Patients treated by bleeding become much more emaciated than those treated dietetically.

68. In expectant practice, the instant that exudation is completed the sensation of weakness vanishes, and the patient very quickly recovers; when bleeding is practised the patient is certainly easier after exudation is complete, but he then becomes affected with a degree of weakness difficult to combat.

69. The length of convalescence is comparatively far shorter in expectant practice.

70. There is no such disease as chronic pneumonia.

71. Primary croupal pneumonia is a well-marked, distinct, and acute process, in its course bearing most resemblance to the acute exanthemata, especially to small-pox.

72. Hepatisation, whether extensive or limited, requires for its completion, in most cases five, in rarer instances, seven days.

73. Most cases of quickly-cured, or suppressed pneumonia, are instances of hepatisation of the lungs, hastened by venesection.

74. Clinical and physiological facts tend to prove, that venesection cannot prevent the development of the pneumonic exudation.

75. In cases treated by bleeding, the most extensive hepatisations, and largest fibrinous coagula in the cavities of the heart and great vessels, are met with.

76. Bleeding promotes the extension of hepatisation.

77. Many intense and extensive pneumonic cases originate and run their course in patients while under antiphlogistic treatment (unter der Lanzette).

78. Bleeding cannot stay the course of hepatisation once commenced.

79. Rapid resorption is observed after both forms of treatment, but the most rapid instances occur in those treated on the expectant plan.

80. Resorption seldom proceeds rapidly in patients considerably debilitated by blood-letting.

81. Resorption of pneumonic exudation commences when appetite is first restored: no appetite, no resorption.

82. Absence of the so-called critical evacuations is a sure sign of rapidly-proceeding resorption.

83. A first attack of pneumonia, if left to nature, is usually quickly resolved by resorption, without critical evacuation.

84. In many cases venesection does not hinder resorption at all.

85. Venesection favours the suppurative softening of hepatisation, and indirectly impedes resorption.

86. Abscess of the lung is a rare metamorphosis of hepatisation, and is not induced by either mode of treatment.

87. Induration, or partial destruction (verödung), of the inflamed pulmonary parenchyma, seems to be favoured by the expectant treatment.

88. Gangrene is not a consequence of an exquisite degree of pneumonia, and may occur indifferently after any form of treatment.

89. Tuberculosis is developed independently of any particular form of treatment.

90. Venesection decidedly favours the complication of pneumonia with other acute exudative processes, such as meningitis and pericarditis.

91. Venesection seems to favour the complication of pneumonia with œdema pulmonum and pleuritis.

92. Uncomplicated pneumonia, if left to nature, is very seldom fatal.

93. Uncomplicated pneumonia, treated by blood-letting, is often fatal.

94. The expectant treatment affords a far more favourable proportionate mortality.

95. Venesection is a certain, and by no means trifling, cause of the great mortality of cases of pneumonia.

96. Pneumonia treated by bleeding proves fatal for the most part, by an increase in the morbid condition of the blood.
97. Bloodletting is never necessary for the restoration of health, and is, therefore, never indicated.
98. Bloodletting is in many cases decidedly pernicious.
99. No remedy procures the patient such remarkable and prompt relief as bloodletting.
100. Bloodletting, as a means of combating symptoms, cannot be replaced by any known remedy.
101. The indications for bloodletting cannot be scientifically established.
102. The application of bloodletting to the treatment of pneumonia should be extremely limited; or, what is safer, it should be altogether dispensed with.

Now, all this is written in sober earnest. Dr Dietl is a public medical officer, in one of the district hospitals of the suburbs of Vienna, and his work bears intrinsic evidence of being the production of one who thinks much, and is anxious for the investigation of truth. We fear, however, that this object is not likely to be much promoted by the promulgation of the above given creed. Our readers will, we presume, gladly be spared the infliction of the statistical part of the work. Suffice it to say, that it includes 85 cases of pneumonia treated by bleeding alone; 106 by heroic or contra-stimulant doses of tartar emetic alone; and 189 by dietetic means, or on the purely expectant plan;—the respective mortalities being 20·4, 20·7, and 7·4, per cent. In vain we look for any details as to the age, or sex, or occupation, or social position, of the patients, or for precise information as to the epidemic constitution of the seasons during which the three different plans of treatment were in operation.

The pneumonia of Dr Dietl is a disease of the blood, with a tendency to exudation into the tissue of the lungs, characterised by certain physical signs, such as crepitation, bronchial respiration, and modifications of the sound of the chest on percussion—signs unknown to the older physicians. They indeed recommended bloodletting as the most important remedy in what they termed "inflammation of the lungs," and under this head would no doubt have included many cases of modern pneumonia, with probably an equal number of the morbid process now termed pleurisy. But very many cases of the pneumonia of our more accurate modern pathology, many examples of what has been termed *stethoscopic* pneumonia, they would either not have recognised at all, or if recognised, would not have treated as they were wont to treat the exquisite cases. Pathologically, they must undoubtedly have made many inevitable mistakes; but whether their treatment was not as rational and successful as that now adopted by the most learned of foreign pathologists, may admit of question. What we here contend for is, that, as the pneumonias of our forefathers and those of modern pathology are not identical, no just conclusions can be drawn from contrasting the results of their treatment

by phlebotomy, say in the days of Sydenham, with the statistics furnished by the experience of a physician of the present day.

Let any man, whose conscience will permit him to do so, apply an active and exclusive system of phlebotomy to the first hundred cases, with physical signs of pneumonia, which present themselves to him in hospital practice—the result may be predicted. Some will recover in spite of him, many will die in consequence of his ill-directed energy. The list of victims will include many examples of typhoid pneumonia, and a few unhappy patients, whose slighter ailments would, if less actively treated, have probably undergone spontaneous cure, instead of passing into tuberculosis, becoming chronic, or recurring in more aggravated and less manageable shape. So far, we bow to the statistics of Dr Dietl. The mortality of 20·4 per cent. might easily be obtained by such a course of proceeding.

But even if the practitioner, convinced of the inutility of the lancet, should feel justified in testing the powers of human endurance, by instituting a perfectly similar and parallel set of experiments with nothing but heroic or contra-stimulant doses of tartarised antimony, we should still expect to hear of very considerable execution done. He would poison a few children, would equally dispose of adults, whose typhoid symptoms indicated the use of stimulants rather than of sedatives, and might by perseverance easily contrive to lose 20·7 per cent. of his patients.

If now he should turn in disgust to homœopathy, or, what is the same thing, to the expectant do-nothing plan, with low diet, and should again make one hundred trials on all comers afflicted with pneumonia, we might anticipate results far more encouraging. All his trifling cases would recover; a proportion of mild ones would recover imperfectly; some of the severe ones might recover too; some would die whose lives no treatment could have saved; others, whose lives should have been saved, would die, victims of neglect. The ratio of mortality would be increased, directly as the proportion of the severe to that of the slighter cases. It is very possible that this expectant plan might, as Dr Dietl asserts, be found the least destructive of the three, and that the 7·4 per cent. might represent its mortality; for, by *systematically abusing* remedies, powerful for good or evil, more harm may be done than by neglecting their use altogether.

But it may be objected, "that in the view of these trials above laid down, we do not use Dr Dietl fairly,—that we assume that he practised bloodletting indiscriminately and largely, instead of resorting to it only in well marked cases, such as seemed absolutely to require it." Let us accept the author's own explanation of his practice. "*Bloodletting was uniformly had recourse to, when there was a certain degree of fever and dyspnœa, and no extreme degree of exhaustion to counter-indicate it. In several cases a single copious bleeding sufficed to still the storm; in most instances two or three; and*

in a small number five or six were required," (p. 126, l. 12-17). After this avowal, what becomes of the "*Numerische Ergebnisse*;" where are now the statistics, where the superstructure of practical aphorisms? We are asked to compare results drawn from 85 cases of extreme gravity, treated, perhaps *ill* treated, by bloodletting, with 175 applications of expectant practice to cases, most of which (all comers being included) probably required no active treatment at all, and in which no intelligent physician would have used the lancet. And a grave table of comparative mortality is drawn up, wherein the same precious facts are presented in divers approved fashions,—and the eyes of the weak-minded are blinded when the great statistical mesmerist shuts his own—and this is an example of statistical inquiry and the power of numbers!

Whether the 85 cases of pneumonia treated by bloodletting include all the cases which Dr Dietl met with, while still imbued with the heresy of his early preceptors; or whether (as seems tolerably certain) they consisted of *the more exquisite examples of the disease*, culled from the practice of one wavering between prejudice and scepticism, we must equally protest against conclusions drawn from comparison of this experience, with the results of his subsequent expectant practice. On the first supposition, he must have performed blood-letting unnecessarily, and did wrong to trust to it exclusively; on the second, he omits all notice of the slighter cases which occurred in his early practice, while he has included all, slight and severe, in his account of his expectant eccentricities. The great and fundamental condition necessary for a satisfactory experiment—the precise similarity of the cases—is obviously wanting; its results, and theories thereon based, are consequently worthless, and calculated to mislead.

We should not have devoted so much space to the exposure of the chief fallacy which pervades and vitiates this statistical inquiry, had it not been presented to the profession in a simple unassuming form, wearing the mask of truth, and recommended by the authority which favourable notice in various foreign journals confers.

On the fanciful nature of much of the physiology and blood-pathology by which Dr Dietl attempts to account for the whole phenomena of pneumonia, we need make no remark: the reader who endeavours to tread the misty maze of the author's own epitome of his doctrines, will hardly need to be informed where he may break his shins.

But ere we dismiss Dr Dietl from our thoughts, a word of admonition suggests itself, directed to all about to undertake the cure of disease. The responsibilities of the medical man are very awful; and the practitioner who cannot recognise them is, in the true sense of the term, a moral maniac. An honest and God-fearing man dares not to make rash experiments upon those confiding in his skill: nothing new or paradoxical will he attempt in dealing

with life and health, unless the indications for departure from established rules seem so clear as to carry conviction to his mind. We trust that Dr Dietl, and other foreign members of the same pathological school, who have made wholesale experiments upon their fellow-beings, changing from system to system, till further metamorphosis is hardly possible, could offer some moral justification for their course of error. But what shall we say for those who, repudiating the faith professed by their forefathers (men wiser than themselves) are, in fact, medical sceptics, practising a profession which their conscience assures them is superfluous and mischievous? We say that no species of imposture can be more detestable, and that cretinism were preferable to the degree of moral degradation which such conduct implies.

Pathology of the Human Eye. By JOHN DALRYMPLE, F.R.C.S. London, 1850. Fasciculi 5 and 6. Imp. 4to. 8 Plates.

Surgical Anatomy. By JOSEPH MACLISE, F.R.C.S. London, 1850. Fasciculi 6 and 7. Folio. 8 Plates.

THESE beautifully illustrated works, of which the first Fasciculi were issued in 1849, are now approaching their completion. The fifth number of Mr Dalrymple's work embraces delineations of the appearances and effects of inflammation in the anterior chamber of the eye, in the iris, and in the sclerotic. The sixth *fasciculus* is devoted to congestion and inflammation of the choroid and retina, glaucoma, hydrophthalmia, and partial staphyloma of the sclerotic. The drawing and colouring of these plates are alike admirable.

Mr Maclise's *fasciculi* include demonstrations of the nature of congenital and infantile inguinal herniæ, of the surgical anatomy of herniæ inguinal and femoral, of the blood-vessels of the groin and thigh, of the position and relations of the pelvic organs of the male subject, and of the male perineum. There is much originality in these demonstrations: Mr Maclise does not always content himself with reproducing the ideas of other anatomists, but depicts and describes the regions in his own way. We need hardly renew the praises which were in this Journal bestowed upon the earlier fasciculi of the work: the later numbers fully maintain the character which these secured for it.

We should not omit to mention, in justice to Mr Churchill, the publisher of both works, that the numbers have hitherto succeeded each other with a most praise-worthy regularity, and that a speedy completion of both series may be confidently expected.

Part Third.

CLINICAL REPORTS, LECTURES, ETC.

CLINICAL MEDICINE.—PROFESSOR ALISON.

OBSERVATIONS MADE ON SOME OF THE CASES IN THE CLINICAL WARDS OF THE ROYAL INFIRMARY, EDINBURGH, NOVEMBER 1850. ABRIDGED FROM LECTURES.

Although much has been said, and very properly said, as to different modes of conducting clinical instruction, yet a little attention to the subject will show, that more depends on the exertions and attention of the student, than on the mode taken of communicating information to him. It is, however, of the greatest importance, as regards the advantage of the student, that he should have, from the first, a clear understanding of the different objects to which his attention is to be directed, and with which it is wished that he should make himself familiar, in the course of his attendance here; and the following may be pointed out as the main objects for his observation and reflection.

1. The different *classes of symptoms,—uneasy sensations,—disordered functions,* —and *altered sensible qualities of different parts* of the body, by means of which diseases are recognised during life;—the kind of information given by those different kinds of symptoms being considerably different—and a combination and succession of symptoms being almost always requisite, to characterise any well-marked disease.

2. The observations which may be made on many patients, which indicate, not the specific disease under which they may labour, but some *peculiarity of their condition or constitution*, sometimes of old-standing, sometimes recently acquired, which may be common to many different diseases, and often regulates both the prognosis and the practice, at least as much as the diagnostic symptoms of disease,—*e.g.*, the indications of the scrofulous diathesis, of the typhoid tendency, of general failure of the vis vitæ, &c.

3. The observation of the changes which diseases undergo from day to day, making us familiar with the *usual course of diseases*, both in the cases that terminate favourably and in those that are fatal, the examination after death forming, of course, a most essential part of the history thus impressed on the mind, and that whether any distinct traces are or are not found, to correspond with the symptoms.

4. The observation of the *effects of remedies on disease*, for which we are in fact only well prepared when we have pretty clear ideas as to the course to be expected independently of such remedies; but of which we are capable of judging, in most instances, more satisfactorily by watching the progress of individual cases, than by making statistical comparisons of different sets of cases and modes of treatment;—the evidence of Statistics, being in fact, for reasons which it is not difficult to assign, much more generally applicable with good effect in inquiries regarding the causes of diseases, than in those which relate to their treatment.

5. Another inquiry presents itself for consideration, in regard to the diseases here, and the practice employed in our hospital, which requires more previous experience, but is equally important,—viz., how far, when we have recognised diseases, we can observe their symptoms, course, and the action of remedies on them, to be similar to, or different from, what we have seen or heard of the same diseases at other times or places. It is generally admitted, as to the epidemic and endemic diseases excited by specific poisons, that they vary remarkably in different seasons; and I have repeatedly stated my conviction, and find

it to be the general belief of practitioners in this country, that an equally striking difference has taken place, although more gradually, in regard to sporadic diseases, especially inflammations. Since I expressed this opinion in this place in June last, I have had several communications from experienced practitioners in confirmation of it, and showing that more caution is requisite than has been generally acknowledged, in applying the experience of one time or place to the practice in another; affording likewise another illustration of the general danger of practising merely for the names of diseases; and of the general importance of fixing in our minds principles, in regard to the modes of fatal termination to be apprehended in the course of disease, of more extensive application than the histories of individual diseases. One of these communications is from a friend in Aberdeen, who is one of the professors of medicine, and one of the physicians to the hospital, and who says—"The unusually large number of cases of pneumonia and pleurisy has been observed this season with us, as well as at Edinburgh, both in the infirmary and private practice; and with regard generally to the use of bloodletting in inflammatory disorders, it has long been a common remark among the profession, in this district as elsewhere, that that remedy is neither required nor borne, to the extent that formerly seemed necessary. About the time your lectures appeared, I had under my care a middle-aged woman with well-marked pneumonia, whose symptoms, from the very first, were such as to forbid the abstraction of a single drop of blood, and to demand wine, ammonia, and beef-tea, and who under these, with blisters and stimulating expectorants, recovered perfectly, although slowly."

Another letter, from an experienced practitioner at York, expresses "much interest in finding that the medical men in Edinburgh entertain the same views on the character of disease, as have been gaining ground with us for some time past." He adds: "I am making some inquiries with regard to the diseases of cattle, and learn, what indeed I already knew in part, that the diseases incident both to horned cattle and horses, have of late years assumed an *adynamic* character, and that bleeding is not employed to anything like the same extent as formerly."

One remarkable fact in the practice of the clinical wards this month, has been the number of cases of Erysipelas, which, from some cause unknown to us, has been unusually prevalent in the town. Eight cases have been treated in No. 2 (the small men's ward, now appropriated to diseases of the skin), and two in the women's ward, No. 11. Of these one only had originated in the house,—so that we have hardly had to consider the question, always demanding attention when erysipelas shows itself in an hospital, how may its extension be restrained? All that I can say on that point is, that, so far as I have ever seen, there is one expedient worth all others put together, for that purpose,—viz., *isolating* the cases of erysipelas that are either brought into the house or originate in it, and preventing their communicating with patients ill of other diseases, especially with such as require any incisions, bloodletting, leeching, even blistering, on the surface of their bodies; for I am sure you will find it to be the usual mode of extension of the disease in an hospital, that in some of the patients in whom the skin is thus broke, the erythematic inflammation shows itself, at the spots thus laid open to the influence of its cause; even although the eruptive fever, consequent, I presume, on the absorption of the poison, may precede the inflammation by several days;—but that this happens only *when there are already in the ward other patients, affected with this specific form of inflammation*. And knowing, as we do (and as I have repeatedly seen), that a person may be *inoculated* with the erythematic inflammation and its attendant fever, by receiving on a cut or prick on the finger a very minute quantity of matter which has formed part of the exudation from an erysipelatous inflammation in another, we advance no extravagant theory in supposing, that effluvia from the erysipelatous patients in a ward find their way into the various openings in the cuticle above-mentioned, and inoculate the patients, who become affected with the disease; and that where no patients in erysipelas are treated

in the same wards with others, there is little or no chance of this kind of communication.

As to the pathology of this disease, we can hardly go farther than this,—that although rightly arranged as a form of inflammation, it is clearly an inflammation of peculiar or specific character,—shown by its tendency to spread along the surface—by its tending to vesication or effusion of serum, much more than to the effusion of lymph, on whatever texture it takes place,—farther, by the general symptoms attending it being remarkably various, and often strictly typhoid, and by its often prevailing to a certain degree epidemically, although from unknown causes,—and undoubtedly in some instances, extending itself by contagion or inoculation. Judging by these marks, we are very sure (and can show by reference to various authors) that this form of inflammation occasionally attacks, and even affects epidemically, various internal textures, as well as the integuments; especially the cellular or areolar membrane, the mucous membrane of the fauces and air passages, the peritoneum, and the inner membrane of the veins.

In regard to the proper practice in this form of inflammation, there has been such a difference of statement by experienced practitioners, at different times and places, as must naturally appear very perplexing to a young practitioner, unless he is master of the principle above stated, that probably all diseases,—certainly all those diseases which at times prevail epidemically,—show themselves, not only in different individuals, but in different districts and seasons, under a very considerable variety of forms, particularly as regards the constitutional fever attending them,—and may therefore be fatal in different modes, and require that they may be most successfully treated, very different kinds of practice, *i. e.*, different kinds of aid to the natural process of decline, to which all modes of practice must be regarded as auxiliary. This has been, to a certain degree, exemplified here. We had one fatal case, Briggs—a man past seventy, of intemperate habits, admitted 3d November, on the fifth day of erysipelas of the head and face, closing the eyelids, and which had gone on to partial vesication on the face. His pulse was from the first small, and soon became feeble, and tongue dry, with typhoid delirium and drowsiness, as usual in severe cases in such subjects. We tried to arrest the inflammation on the face by a number of small punctures on the eyelids, the first night he was in the house, but this had no good effect on him. Afterwards the erythema spread rather slowly over the face, and neck, and back, causing no great swelling, and neither vesication nor suppuration. Besides laxatives, he had only wine and spirits, gradually increased to ℥x. and ℥iv.; and, for some days, I thought we should have pulled him through; but the whole typhoid symptoms then increased, and he died on the 17th day, rather by asthenia than coma, because, although very drowsy, he could be roused so as to answer questions, even when his pulse was hardly perceptible, and his skin cold and clammy.

On dissection in him, no appearance that could be distinctly pronounced morbid was detected, except a serous effusion into the ventricles of the brain, and on its surface beneath the arachnoid, distending those parts slightly, but not exceeding half an ounce in all. This is exactly the same appearance as in the most common lesion after typhus fever, and on which I think the most valuable observations are those of the late Dr J. Reid, long pathologist here, shewing that its amount is most generally proportioned, not to any affection of the brain during life, but simply to the age of the patient.

"We have seen cerebral derangement (before death) as strongly marked, in cases where no increased effusion of serum was found in the cranium after death, as in those where this was observed. Besides, it is not unusual to find increased serous effusion within the cranium in old people, or when the patient has been emaciated by other diseases, in quantities equal to what we have found so frequently in fever. The average age of the cases in which an increased effusion was found within the cranium (in fever) was $42\frac{1}{13}$ years,

while the average age of those in whom no increased effusion was found was 26$\frac{1}{18}$ years. The average age of the four patients in whom ʒvij or more of serum was found in the lateral ventricles, was 57$\frac{1}{4}$ years,—the youngest being 48, and the oldest 68 years of age."—*Report on the Epidemic Fever of Edinburgh, in Physiological Researches.* By Dr J. Reid. P. 460.

When we remember, in addition to these facts, that many patients, although advanced in life, recover from typhoid fever, under the use of stimuli, in whom the affection of the functions of the brain during life had been very much the same as in this man, but that the recovery of the functions of the brain is usually, in such cases, very slow and gradual,—we must regard it as highly probable, that some degree of effusion of this kind is frequently recovered from; and we cannot consider such effusions as we found here, as either an appearance peculiar to typhoid fever, or necessarily connected with its fatal event.

In one case only, that of J. Wood, æt. 22, admitted on the 3d day of erysipelas in the face, which had already produced slight vesication on the eyelids, the state of the circulation was such (pulse 100, and firm, with sense of heat, and uneasy throbbing, as well as pain of head) that I thought bleeding at the arm justified. He was bled immediately to ʒxij; the blood was sizy, and this was one of the cases, the mere watching of which I take to be more satisfactory than the counting of many would be, in evidence of the power of that remedy over some circumstances of the disease; for he expressed himself much relieved as to all his uneasy feelings immediately after the bleeding—his pulse was never above 90 after it, and there was positively no extension of the inflammation, either on his forehead or down his neck, after that night—*i. e.*, the first night he was in the house, or fifth of his disease.

A more singular circumstance connected with these cases of erysipelas was, that in two of the ten, it was complicated with Phlebitis, although in circumstances very different. Janet Duff, æt. 36, delivered ten days before she came to hospital, was admitted with a broad patch of erythematic inflammation over the sacrum, the whole of the left leg and thigh swelled, tense, and painful, and several superficial veins on the inner side of the thigh much distended and tender a little above the knee, and circumscribed tenderness, likewise distinct, though slight, in the situation of the femoral vein at the groin; and M'Naughton, a man, æt. 36, admitted on the 5th day of distinct erysipelas œdematodes occupying the left foot, extending to the ankle, and attended with œdematous swelling of the whole leg and thigh,—just such as we see in the phlegmasia dolens after delivery, or in the swelled leg after fever—although less tense and elastic than that swelling often is—and in like manner attended with distinct swelling and tenderness along the line of the femoral vein at the groin. In these cases, however, the febrile symptoms were less severe than might have been expected, and all the symptoms subsided remarkably well after two applications of leeches in each, over the affected veins, with very little additional treatment except absolute rest of the limbs. In the case of Duff, however, the erysipelas on the nates ended in pretty extensive suppuration. Comparing the case of M'Naughton with various others, of swelled legs going along with symptoms of phlebitis at the groin, I could not doubt that he was thus saved a tedious and troublesome, if not dangerous, affection of the limb. But in neither of these cases was there any indication of the dangerous secondary inflammation of internal parts, against which we must always be on our guard when we distinctly recognise phlebitis;— which we may apprehend especially when we have it complicated with erysipelatous inflammation externally;—but which, so far as I have seen, is much less frequent, and of a less malignant character, in cases that lead to obvious *obstruction* of a vein, than in cases where the effusion into the vein is of a less *plastic* character.

Another remarkable fact here has been a fatal case, at one time somewhat resembling erysipelas and attended with very typhoid fever, but which I have no doubt was an example of Glanders in the human subject—closely resembling those described by Dr Elliotson in the "Medico-Chirurgical Transactions." (Vol.

xvi.) This was John Turnbull, æt. 41, admitted evening of 18th November, having been complaining for a fortnight of febrile symptoms and pains of joints, and almost entirely disabled for work. When I saw this man on the evening of admission, finding he had been so long ill and was of intemperate habits, with a pustular eruption on the face, and had delirium, spectral illusions, and subsultus tendinum, with a pulse at 120, small and feeble, his tongue furred and dry, his breathing hurried, with cough, bronchial râles, and frequent vomiting, I thought it a bad case of bronchitis and rheumatic pains, complicated with delirium tremens, and gave directions for his reception, if necessary, into the ward appropriated for deranged patients; ordering likewise a blister, repeated small doses of aloes, calomel and opium, and a little wine. Next day we found that he had not become violent as expected, but had fallen into deep sleep with copious sweating, and although he could be roused to answer questions, and his bowels had been moved, was nearly comatose. The eruption on his face was now perceived to be attended with an erythematic flush on the forehead and closing of the left eye. His aspect immediately brought to my recollection the only other decided case of glanders in the human subject I had ever seen, which was traced to infection from the horse, and one of the gentlemen acting as assistants in the ward, who had seen a case in England last year, made the same remark; and then finding on inquiry that he was a cab-driver, I had little doubt of the nature of the case. We now found that the same kind of eruption which we saw on the face, consisting of small hard pustules (or tubercles, for they were almost solid), each surrounded with a patch of erythematic inflammation, existed on different parts of the body, chiefly at the wrists and knee-joints, which had previously been the seat of the pains. There was no running from the nostrils, although the left side of the nose was much swollen as in erysipelas. The vomiting had abated, but the cough and hurried breathing and bronchial râles continued; his pulse was more frequent, and very feeble, and his tongue dry. After this, we attempted nothing more than the stimulating treatment, by increased doses of wine, carbonate of ammonia, and two doses of quinine, which had no obvious effect, and he died at four o'clock next morning,—having been only 36 hours in the house.

If there could be any doubt as to the nature of this case during life, I think there could be none when it was found, on examination of the body, that within the larynx there was a pustule, similar to those on the skin, a little above the chordæ vocales on the right side, and several smaller pustules of the same kind in the trachea; the bronchi contained muco-purulent matter of little tenacity, these membranes being moderately injected; the lungs presented several ecchymosed circular patches, scattered through various parts of their substance, each of which contained a central yellow granular deposit, gradually breaking down into pus. These, when near the surface, had the appearance of circular pustules, surrounded by a border of ecchymosis, very similar to those on the skin. On opening the nasal cavities, a line of small circular ulcerations was observed between the inferior and middle turbinated bone on the left side, which were infiltrated with pus. There was no such appearances on the opposite side; nor was any disease found in the liver, nor in any of the veins or lymphatics which were examined.

We could not ascertain anything farther as to the history of this patient, than that he had been employed, up to the time of his illness, in a stable, in which a veterinary surgeon, who attended the examination of the body, said he knew there had been several cases of glanders. I learnt from Mr Dick, Professor of Veterinary Surgery, that he was aware of there having been cases of glanders recently in some of the stables for hackney coach horses, although he had not himself seen any.

I can say with certainty, that the appearances on the face, nose, and eye here, were exactly similar to those I saw in the one case, likewise fatal, some years ago in the hospital, which was traced to infection from a stable some

miles from town (where there were, undoubtedly, glandered horses), excepting only in this, that in that case the inflammation on the face went to gangrene, which I never saw in erysipelas of that part. In this case, likewise, from the state of the general system, it is very likely that gangrene would have ensued, if the typhoid fever had not been so early fatal. The appearances on the skin were manifestly similar to those of the *pustule maligne* of the French authors, and of the glanders in human subjects, described by Dr Elliotson, in their early stage.

What is most deserving of notice here, is the confirmation thus given of the principle, which former observations seem indeed sufficient to establish,—that this morbific poison is not only capable of being communicated from the lower animals to man, but acts (as we have reason to believe the variolous poison does) as a more virulent poison on the human body. For although the same poison in the horse, where it is diffused over the body, in the form to which the name of "Button Farcy" is given, producing numerous hard pustules which may go to ulceration, is often much more acute in its progress, than when confined to the nostrils, and having the name of Glanders, yet by far the greater number of cases of disease in the horse, resulting from this poison, are chronic, and many of them, at least for long, seem quite local,—whereas, in the human body, bad typhoid symptoms seem always to attend the first stage of the disease thus excited, and even when they are got over, as in one of the cases given by Dr Elliotson, the pustules soon degenerate into intractable ulcers, and the general health has appeared to be irretrievably broken.

We had in the Ward No. 1, five well-marked cases of Pleurisy, with extensive effusion, besides one (a young man, Kelly) with distinct pleuro-peripneumony; and on this occasion, as during the last summer, it appeared, on examination, that no one of these had such general symptoms, particularly such firmness of pulse, or heat of skin, as seemed to me (however anxious to show the good effect of that *summum remedium* in the cases suited to it) to justify general bleeding; nor were any of them bled at the arm at any period of their disease. Several of them were, no doubt, admitted in an advanced stage of the disease, but two of them I had myself seen previously in the earlier stage. Kelly was admitted on the 5th day, and Flyn (the worst case), on the 9th day, after the first rigors, both with feeble pulse and cool skin, and Flyn with so much of the typhoid aspect, especially as he had a copious eruption on the skin (which, however, soon disappeared, and had probably been from fleabites) that I hesitated for two days whether his case was not to be regarded as typhus.

All these cases have, however, improved as decidedly as could have been expected in this time—several of them with the help of a little of the stimulating treatment. In the case of Flyn the effusion extended considerably for some time after his admission, as shown by the displacement of the heart, and by the extension upwards and forwards of the dull sound on percussion, and absence of respiratory murmur; and as the affected side measured at one time 1½ inches more, horizontally, from the scrobiculus cordis, than the sound, we naturally began to think of the paracentesis. There has been, however, so much improvement under the use of calomel and opium—slightly affecting his mouth—that this expedient has been postponed, and may probably not be required. How far this improvement should be regarded as the result of the remedy, or how far it should be ascribed to the rest and antiphlogistic regimen, and to that influence on the inflammation, which we may reasonably suppose to have been exerted by the other remedies used when he came in—leeching, blistering, purging, and antimonials, it is very difficult to judge. Certainly we had several examples of more rapid and satisfactory absorption of the pleuritic effusion, in patients who took no mercury, and by taking none escaped a risk which I cannot but think attends the full use of mercury in many constitutions.

It is remarkable that the most rapid recovery has been in the young man Kelly, in whom pneumonia as well as pleurisy was distinctly marked—proba-

bly because, being admitted on the 5th day after the first attack, he had above twenty leeches and a blister applied, besides being purged and taking the antimonial solution, within three days after admission, or within eight days from the attack. After this treatment his pulse came down from 114 to below 60, and after this he had a small allowance of wine during the remainder of his stay in the house, and all the indications of effusion, except slight dulness, disappeared within ten days.

We had, in this ward, two deaths by old Bronchitis, in men advanced in life, one of whom was admitted in the last stage of lividity, cold sweats, and delirium, and died within forty-eight hours. The other had been an inmate of the ward almost continuously since April, was much emaciated, and had repeated remissions and exacerbations of the symptoms. Both bodies were examined, and exhibited the usual appearances and the usual effects of long-continued bronchitis and habitual asthma, distinctly and nearly uncomplicated;—the muco-purulent effusion in the smaller branches of the bronchi, and those two lesions of the substance of the lungs, which hardly ever fail to attend such cases, *i.e.*, the emphysema pulmonum in some parts, and the atelectasis, or carnification, as distinguished from hepatisation, of other parts. Both these we may regard as certainly the consequence of the habitual bronchitis. As to the mechanism of their production, there is still some difficulty; but as it is fully discussed, and the latter in particular, I think very satisfactorily explained, by Dr Gairdner, in recent Numbers of this Journal, I need not dwell upon it. The enlargement of the right side of the heart and of the liver, so general in such cases, and often obvious, and causing part of the symptoms before death, were observed in those cases, but in a less degree than is common.

A more striking case than any in the men's wards, in illustration of the *adynamic* character now common in inflammatory diseases, was that of H. M'Gillivray, æt. 42, admitted into No. 11 on the 4th December. She was a stout woman, of the melancholic temperament, in the sixth month of pregnancy; had been formerly subject to slight disorders of the stomach and bowels, and it appeared afterwards had been so affected for a week or two prior to this attack, but thought herself in good health till the 29th November, when she was seized with rigors, followed by pain of abdomen, but especially of right hypochondrium. She was admitted, therefore, on the sixth day of febrile disease, for which nothing but rest and confinement had been prescribed; she still complained much of the pain, increased by pressure and inspiration, but there was no decided dulness on percussion in the hypochondrium, and her pulse (about 100) was so soft, feeble, and irregular, with cold clammy perspiration, that I thought it necessary to give an opiate, and wait for some degree of reaction before using any evacuant. After this there was slight reaction, and she had ten leeches to the hypochondrium, and a dose of castor-oil that night. She had little vomiting, and no diarrhœa, but had a miscarriage the second night she was in the house, attended with little hemorrhage, and by which her symptoms did not appear sensibly altered. I stated, that if she had been first seen on the second or third, instead of the sixth, day of this inflammation, affecting pretty certainly the peritoneum, and perhaps also the liver, I should have had good hopes of seeing the pulse improve in strength, as well as abate in frequency, under an alternation of local bleeding and gentle purging or evacuation, with repeated opiates—and perhaps improve so far as to justify general bleeding; but that, after the sixth day, such improvement of the pulse after evacuations was hardly to be looked for. In fact, there was no improvement as to the state of the circulation after this time, although the pain abated; she had a little wine, as well as repeated opiates, but continued to sink, without any fresh rigors or return of pain, and died on the morning of the 10th, *i. e.* the twelfth day after the first rigor—her pulse having been very feeble, and skin colder than natural during the whole five days she was in the ward.

Now in this case it appeared, on dissection, that the liver, although hardly enlarged, was studded with abscesses, most of them very small, but two or

three nearly as large as small oranges; that there were some patches of lymph on the peritoneum, both at the liver and about the cœcum; and a number of the aggregate glands, on a limited portion of the mucous membrane of the lower end of the ileum and cœcum, were inflamed, with deposition of lymph on them, and partial ulceration, and even sloughing; and, moreover, there was a partial deposit of pus between the layers of the mesentery attached to this portion of intestine. This last appearance seems to me to make it most probable that the affection of the liver (and probably the whole of the febrile disease) had been subsequent to, and consequent upon, the disease of those aggregate glands; and different opinions may be formed, and supported by cases more or less analogous, as to the mode of connection between this affection and that of the liver. But what I wish particularly to observe is, that the inflammation, and very copious effusion of lymph and pus on the peritoneum and in the liver, must have been going on during a time when her circulation was exceedingly languid, and her surface cold and clammy—illustrating what I have often before remarked, as a leading fact regarding inflammation, that the power, whatever it be, which determines the exudation from the vessels, and the aggregation together, of inflammatory deposits, is one which often acts with the greatest force when the general circulation is preternaturally feeble; and when we know also, by experiment, that the arteries leading to the parts are *weakened* as to vital power, as compared with others; and illustrating, likewise, as far as a single case can do so, the proposition which I have stated as to the low form of fever usually observed at present to attend inflammatory diseases in this neighbourhood; of which I think the occurrence of seventeen cases of pneumonia and pleurisy during three months of attendance on two wards of the hospital this year—only one of which was fatal, and not one of which was bled at the arm, before or after admission—will be allowed to be sufficient evidence.

CLINICAL SURGERY—PROFESSOR SYME.

LECTURE INTRODUCTORY TO THE COURSE OF CLINICAL SURGERY, WINTER SESSION, 1850–51.

In commencing the course of surgical instruction upon which we are about to enter, I must, in the first place, explain the objects it is proposed to attain, and the plan to be followed in their pursuit. The best way of doing this will be to consider what are the qualifications required for the practice of your profession. It is plain that you should be intimately acquainted with the structure of the machine, the derangements of which it is to be your business to remedy—that you should, so far as possible, know the uses of its various parts in a state of health—the changes to which they are liable from diseased action, or the influence of external circumstances—and the effects so produced, whether on the animal economy in general, or the local condition of the part concerned; also, that you should know the uses and applications of the various medical agents which are employed for the restoration of health—and the operative manipulations practised with a similar view; and for your guidance in the application of all this knowledge, that you should understand the rules established for selecting and exercising the means of remedy in each of the cases requiring them. No arguments are required to show the absolute necessity of all that has been mentioned, namely, anatomy, physiology, morbid anatomy, pathology, materia medica, operative surgery, and the principles of surgical practice. But you may not be so well prepared for my next statement, which is, that possession to the utmost possible extent of all this varied and extensive instruction would not be sufficient to qualify you for the practice of your profession; and that, if you were to enter upon it without being qualified in another way, immediately to be explained, you would find insuperable difficulties at every step.

The obstacles to which I allude depend upon the difficulty of ascertaining in any given case the precise seat and nature of the derangement which requires to be remedied. For instance, let us suppose that the patient has been thrown down with force, that he has not been able to rise from the ground, and that he has been conveyed to bed complaining of pain in the neighbourhood of the hip-joint. In such a case, there can hardly be any doubt that one of three things has happened,—namely, dislocation of the thigh bone, fracture through the trochanters or neck of the bone, or bruise of the joint from violent compression of its articulating surfaces. The appropriate treatment for each of these derangements is clearly set forth in the principles of practice which are understood to have been learnt, and the different manipulations requisite may be easily carried into effect. If the bone is displaced, it must be pulled in the direction proper for withdrawing it from its abnormal position; if fracture has occurred, the long splint should be applied to prevent motion of the broken surfaces at the seat of injury; and if the joint has been bruised, leeches, fomentation, and perfect rest of the articulation are required. It may, therefore, seem that a case of this kind should form an easy subject of treatment, and yet there are few rocks on which the patient's comfort and the surgeon's credit are more apt to suffer shipwreck. Let us take another case, and suppose that the patient labours under a tumour of the breast. In all probability, the swelling here must depend upon an abscess, a cyst, a fibrous growth, a simple enlargement of the gland, or a malignant degeneration of its texture. In the first of these conditions, the matter should be evacuated; in the second, the bag may be punctured or dissected out; in the third, the tumour may be removed with every prospect of permanent benefit; in the fourth, local treatment of a soothing kind, with what other means are requisite for correcting any derangement of the system concerned in its production; and, in the fifth, either free ablation of the breast, or, as is much more frequently the proper course, merely such palliatives as may tend to allay uneasiness and delay the morbid process. In all this there is little difficulty for either the head or hands, and yet there are few subjects of surgical practice more perplexing than tumours of the breast. Indeed, you will see to-day a young woman who has come more than two hundred miles for relief from a fibrous growth, in consequence of her medical attendant—a very experienced and intelligent practitioner—entertaining doubts as to the nature of her case. In short, gentlemen, you will find that the great difficulty of practice is not in doing what is necessary, but in discovering what the circumstances of the case require, and ascertaining, in the first place, the seat and nature of whatever derangement has taken place.

In accomplishing these grand essential steps of an accurate diagnosis, you must be guided by the signs or symptoms which result from the morbid state of each part affected. Most of them are recognised by sight and touch, but some of them also by the ear and nose. According to the facility with which they are noticed, discriminated, and appreciated, as evidence of what is wrong, does the practitioner readily and surely ascertain the assistance which it is requisite that he should afford the patient. But although the indications are present, it does not follow that they make an impression on the mind so as to attract attention. You have all, doubtless, often heard or made the reply, "I must have seen, but did not observe, the object in question,"—the meaning of which is, that though within the sphere of vision, it did not make an impression on the sense of sight,—and it is the same with regard to the other sources of information. How, then, you are now prepared to ask, are we to acquire the power of observing the signs which are to guide us in our progress? And here I may refer you to the invariable practice of all persons who desire to observe with nicety and precision the distinctive characters of different objects. In all pursuits, professions, and employments, which require for their successful exercise that this should be done, you will find that the power of doing it is always obtained in the same way,—namely, by careful examination of the circumstances to be distinguished, so as to fully understand and become familiar with

them. The more familiarly acquainted with any object, the more readily you recognise it; and it is this familiarity with symptoms of disease which enables an experienced practitioner to judge quickly and correctly as to the condition of his patient.

You will now be able to see why hospital attendance has been enjoined as an essential part of your professional study. You may read or hear the symptoms of disease described over and over again, without being able really to know them, or to acquire the power of observing them. But you have here an opportunity of witnessing the actual existence of morbid derangements in the living body, with all the sensible characters by which they are distinguished. Surgical cases bear a small proportion to the subjects of ordinary medical treatment; and it is, therefore, of peculiar importance that you should study them when brought together in large establishments such as this. In the course of the ensuing six months, most, if not all, of you, will see more surgical cases than any one will have to treat in the whole duration of his private practice, however much it may be prolonged. Indeed, I have heard gentlemen of considerable standing and experience declare, that in once going round the wards which are to be your field of study, they have seen more cases of surgery than all that had ever fallen under their treatment. But it must not be supposed that you are promised an opportunity of seeing an example of every case that may occur in the practice of surgery. This would plainly be quite impossible, and fortunately is not at all necessary, since each particular disease is not distinguished by characters exclusively belonging to it alone, but rather by certain combinations of characters which it possesses in common with many others. If, therefore, you acquire the requisite familiarity with symptoms, whatever may be their source, you will be able to recognise them, though differently grouped, in connection with other morbid derangements.

The great duty imposed upon me as a teacher of clinical surgery will now also begin to appear. If left "to walk the wards" without assistance, you might be able to ascertain whether there were many operations or few, whether their performance was accomplished with ease or difficulty, and whether the patients died or recovered. But as hospital attendance, productive only of such fruits, would be of little value in your preparation for practice, it has been deemed expedient that a teacher should be provided for rendering this opportunity of observation more conducive to improvement. In discharging the duty thus confided to me, instead of merely lecturing upon the cases, and telling what may be learnt from their examination, I bring the patients before you, having them conveyed in a basket if not able to walk, and carefully point out every circumstance that seems deserving of attention. So far as possible I let you see the people who are to be under treatment at the time of their admission, so that they may be presented just as they will be to yourselves when engaged in practice; and also, that you may witness the process of investigation which is pursued, in the first instance, for the recognition of their respective ailments. In the progress of treatment, I bring them again and again into this place (the great theatre of the hospital), so that the changes which have occurred may be duly explained. On alternate days I visit the wards, when you have an opportunity of seeing the patients more closely, and obtaining further information directly from themselves. On the two remaining days of each week (Sunday being a day of rest), the operations of too great importance for extemporaneous execution are performed. There is thus a constant system of instruction; and it is pursued, I am happy to say, not only with much advantage to you, but also with great satisfaction to the patients, who, so far from feeling any unwillingness to have their cases examined in your presence, regard it as a guarantee for their careful treatment, and an expression of interest in their welfare.

In pointing out to you the distinctive characters of disease, I shall be careful to avoid perplexing you with irrelevant matter, as would seem too frequently done, if any judgment as to the practice of other schools may be formed from the lucubrations with which, under the title of "Clinical Lectures," the periodical

medical press has of late years been teeming. When we see pages upon pages of closely printed statements, as to every minute circumstance that can be noted in the patient's organisation, however remotely connected with the particular derangement in question, doubts may be entertained as to their ever having been delivered to a waking audience, but none as to their utter uselessness for accomplishing the object professedly in view. In describing any particular sort of stone, plant, or animal, we do not mention all the characters which it possesses in common with other members of the same family, but carefully select those points in which it differs from them; and you may recollect that Linnæus laid it down as a rule, in describing the species of plants, that not more than twelve words should ever be employed for the purpose. Indeed, it may be truly said, that, next to not observing the symptoms of disease at all, the greatest error a practitioner can commit is to collect them, without reference to their relative importance as distinctive features of the case. Keeping this constantly in view, I shall be careful to explain the circumstances which constitute the ground of every opinion I may form in regard to the patients that come under our observation.

And here, gentlemen, I must beg to warn you against placing confidence in the use of certain means which have been proposed as assistants to diagnosis, but are too frequently employed as substitutes for the careful investigations most conducive to the attainment of this object. I allude more especially to the exploring needle and microscope, which are at present much in fashion for facilitating the recognition of morbid growths and deposits. It may seem, at first sight, that, with exception of the risk of exciting diseased action, which must always attend the thrusting of a foreign body through the living textures, no harm, at all events, can accrue from the information obtained in this way; but when the subject is more fully considered, I trust it will appear to you in a different view. For, in the first place, it must be admitted, that reliance upon such assistance encourages a lax and imperfect exercise of the other means which we possess for discovering the truth; and, secondly, it cannot be denied that reliance may be placed with much more safety upon the latter than upon the former alone.

It is upon such grounds that I have always had a great distrust of the exploring needle. But the microscope seems to me still more apt to mislead; and I am glad to take this opportunity of protesting against the importance which at present is most unduly attributed to it, as a means of distinguishing morbid growths. After making this statement, it would be useless to quote any experience of my own upon the subject, as the results would be attributed to prejudice or want of practice; and I will, therefore, select from the numerous mistakes that crowd into my memory, one that lately happened to a surgeon who has written strongly in favour of microscopical examination, who occupies a prominent place in his profession, and who was led to investigate the particular case concerned with peculiar care. The patient suffered from a tumour, which threatened to prove fatal, and of which a small portion being subjected to the microscope, appeared to present the characters of a malignant deposition. Biassed no doubt by this impression, the surgeon finally resolved that the case did not admit of remedy. The patient then applied to me, when, paying no regard to the microscopical texture of the tumour, I carefully considered its situation, connections, and history,—with the conclusion, that it was not connected with any unhealthy condition of the system, and that it might therefore be removed with every prospect of permanent relief. Acting under this persuasion, I performed the operation which was requisite, with the most satisfactory result. In short, gentlemen, I would say that the assistance which exploring needles and microscopes afford in the practice of surgery, may be compared to that which a lame man derives from the use of crutches. If he cannot walk without their support, he must of course avail himself of it; but need never expect to walk so well as one who is independent of such artificial aid.

It is far from my intention to deny that the microscope may be of service in promoting the minute researches of anatomy, physiology, and pathology. But I maintain that it can never supply the want of that knowledge of obvious structure and sensible action in health as well as disease, which is essential for the discrimination of morbid textures with a view to their remedy.

I have now explained what may certainly be regarded as the principal duty confided to me; and have next to acquaint you with another, which, although it ranks of secondary importance, is calculated to exert a powerful influence over your professional qualifications. This is, rendering the principles of surgical practice intelligible and familiar to you, by teaching them in connection with cases admitting of their application. They can never be so fully understood, or so well recollected, as when thus taught; as Horace says,—

"Segnius irritant animos demissa per aurem,
Quam quæ sunt oculis subjecta fidelibus."

And in support of this most true statement, I might appeal to the experience of my old pupils, who often remind me, with the freshness of yesterday, of what was said and done in lectures delivered more than twenty years ago. The impressions thus made being so permanent, I shall be careful to avoid misleading you by hasty or too confident directions for your conduct; always keeping in mind, that everything stated here will, in all probability, be soon subjected to the test of experience, and may thus seriously affect the welfare of your patients, as well as your own professional character. I have, ever since taking the field as a teacher of surgery, been more anxious to establish claims for lasting confidence, than to afford merely temporary gratification by glossing over the difficulties of practice; and I trust that, through a faithful pursuance of the same course, I shall be permitted to regard you as my friends when you cease to be my pupils. The principles which I propose to illustrate are contained in a work published under this title, "Principles of Surgery," with which, I think, it would be well for you to provide yourselves, since the various schools and teachers hitherto concerned in your instruction, must otherwise give rise to a confusion that may seriously interfere with your deriving all the advantages I have led you to expect from our proceedings here. In this book I have endeavoured, not to collect all the conflicting and absurd opinions which have been from time to time inflicted upon the patience and credulity of the profession, but rather to select and arrange, in the best order for understanding and recollecting, all that was really of practical value.

Part Fourth.

PERISCOPE.

MEDICINE.

ON ANEMIC MURMURS AND THEIR DIAGNOSIS. BY DR H. M. HUGHES.

Vol. vii., Part I., of Guy's Hospital Reports, contains an elaborate essay on the above subject. After pointing out the great practical importance of being able to distinguish the murmurs of anemia from those characteristic of organic disease, and stating his opinion that this knowledge is not very generally diffused in the profession, Dr Hughes proceeds to enumerate the signs and causes of anemic murmurs in the heart or large vessels:—

"If a young person, particularly a young female, complain of palpitation of the heart, difficulty of breathing upon exertion, slight tendency to œdema of the lower extremities towards night, or after standing, with pain at a fixed

point of the left side; if she have not previously suffered from rheumatism, and she have no obvious disease of the lung, it is highly probable that, upon examination, she will be found to suffer from an irritable condition, and not from organic disease of the heart. If to these symptoms be added a pale face, and sallow countenance; a dark areola around the eyes; a pale, broad, and indented tongue; a small, sharp, frequent, and vibratory pulse; occasional giddiness, and throbbings of the temples, it is, moreover, in the highest degree probable, that this irritability of the heart is dependent upon anemia, from whatever cause arising; and that an anemic murmur will be heard in the vicinity of the aortic valves. It should, however, be borne in mind, that it by no means necessarily follows, that a person suffering from anemia should have a pale face, as not unfrequently persons present themselves for advice, with a full, florid, and even ruddy countenance, in whom is observed every other sign of a watery condition of the blood, including the anemic murmur."—P. 160.

The probable seat of the anemic murmur is, in the author's opinion, the pulmonary artery; its extremely local character, and the circumstance that it is seldom to be heard in the course of the aorta, being the chief grounds for this conclusion.

"It is ordinarily accompanied with the general appearance and constitutional signs of anemia, with a continuous venous hum in the neck, with an irritable sharp beat of the heart, with a small, sharp, frequent, and vibratory pulse, and increased harshness of the arterial whiz, when a vessel is partially obstructed by pressure. The general causes of that condition of the system with which the anemic murmur is usually associated, are such as either detract from the quantity, or deteriorate the quality, of the circulating fluid."—P. 161-2.

The efficient or proximate causes of the arterial anemic sounds are, according to Dr Hughes, "First, the watery state of the blood itself, by which the celerity of its motion may be augmented, and its molecules may be more easily passed over each other, and by which, therefore, eddies and waves are more easily produced, and attrition is necessarily increased; secondly, the increased *velocity* with which the blood is propelled through the large arteries arising from the heart—a velocity which is indicated by the peculiarly sharp, thrilling, or wave-like pulse, common in such cases (and so peculiar is it as to be ordinarily recognised as the "hemorrhagic pulse"), and also by the especially sharp, smart, and knocking impulse of the heart, continually mistaken by young auscultators for the heaving impulse of hypertrophy, but in reality pointing out only the existence of abnormal irritability of the organ.

"That this smartness of the systole of the ventricles and consequent velocity of the current of the blood, is a most essential feature, if not a necessary element, is rendered theoretically probable by the increased attrition necessarily thereby induced, and is practically proved by a variety of circumstances with which the attentive bedside observer is sufficiently familiar. By way of example, a chlorotic girl, while lying at her ease in bed, and unexcited, may have no murmur, so long as her circulation is comparatively slow; but if the frequency of the circulation be increased by rising up, by moral emotion, by the act of coughing, or of progression, the murmur immediately appears. It is clear that the capacity of the ventricles, and the area of the apertures leading therefrom, are not likely to be altered by the simply augmented velocity; but supposing them to be the same, it is also clear that a given quantity of fluid being forced through a given aperture, the quicker it passes the greater will be the amount of attrition, the greater, practically speaking, will be the obstruction; the consequences whereof are waves and eddies, which, as in organic obstructions, give rise to murmurs. The amount of fluid and the area through which it passes being equal, the passage cannot be effected quickly without producing more attrition than if it passed slowly; any more than a flock of sheep can be driven quickly through a narrow gateway without knocking more rudely against the gate-posts, than if they passed slowly, and at their leisure. Has an anemic murmur been ever heard with a slow pulse? By the author, *never*

But while he has again and again, in a suitable subject, induced it by moral excitement, or physical exertion, he has also, from the mere result of agitation caused by placing the stethoscope upon the chest, been often led to suppose that a patient, in consequence of a loud bellows murmur, must have diseased heart, and has therefore directed the individual to remove his or her outer garments, but has then found that, when the temporary excitement had ceased, the murmur had entirely disappeared."—P. 166-7.

After devoting a short chapter to the consideration of the venous murmurs (a subject which has been so frequently discussed in this Journal, that we may spare further allusion to it at present), Dr Hughes proceeds to lay down some rules by which anemic may usually be distinguished from organic murmurs. He concludes this part of the subject with the following tabular statement of the respective diagnostic signs :—

Anemic Murmurs.	*Organic Murmurs from Valvular Disease.*
1. The anemic condition necessary.	1. The anemic conditions occasional, but not necessary.
2. Congenital disease, or rheumatic fever, in young subjects, almost necessarily excluded.	2. Congenital disease, or rheumatic fever, in young subjects, almost necessarily included.
3. Absent while the pulse is slow.	3. May be, and usually are, present while the pulse is slow.
4. The pulse assimilates to the impulse of the heart.	4. The pulse does not assimilate to the impulse of the heart.
5. Almost exclusively confined to those little advanced in age.	5. Most common in, though not exclusively confined to, those advanced in age.
6. Venous murmur universal at one time or another, under favourable circumstances.	6. Venous murmur absent, unless combined with anemia.
7. Confined to the vicinity of the aortic valves.	7. Not confined to the vicinity of the aortic valves.
	Sound from Pericardial or Pleuritic Roughness.
8. Audible at all times during the systole of the ventricle, but only if the pulse be frequent and vibrating.	8. Audible only, or more especially, during the inspiration, and whether the pulse be slow or frequent.
	Arterial Murmur from Disease of the Lung or Pleura.
9. Equally audible during expiration, as well as during inspiration.	9. Audible only, or more loud, during inspiration.—P. 172.

The remainder of the essay is occupied with some interesting illustrative cases, constituting, however, but a very small proportion of those upon which the opinions quoted from the preceding sections of the memoir have been founded.

ON THE ALLEGED OCCURRENCE OF STARCH GRANULES IN THE URINE.

The "Medical Gazette," vol. xlvi., pp. 799 and 879, contains some observations, by Mr Balman, upon the detection of globules of starch in certain specimens of urine, by means of the microscope ; and some remarks upon the same subject by Dr Thomas Inman, who offers an explanation of the mode in which the starch is probably introduced :—

"We may imagine many ways in which they [starch granules] may be introduced ; the most common is probably the following :—Bread being an universal article of diet, its crumbs are to be found in every house. Falling

on the floor, they harden, and get trodden to dust or powder. When the floor is swept, or the foot passes over, the dust rises, and part gets deposited on the internal surface of all the chamber utensils, and of every unclosed bottle. Urine is voided into its usual receptacle, and poured thence into a bottle for examination, and is then found to contain wheat starch."

[In examining different fluids microscopically, we have repeatedly observed granules of starch, of the nature of which we satisfied ourselves by the unequivocal chemical test of iodine, and by the use of polarised light. But it seemed unnecessary to speculate upon the mode in which starch had gained access to the fluids under examination; for we had long been aware that the squares of very thin glass, used for covering objects before laying them on the stage of the instrument are *commonly packed in starch*, and we had found that, without careful washing and rubbing, it is not possible to free the glass from traces of this substance. The use of packing in starch is probably to prevent the too firm cohesion of the small squares of glass.

It is, of course, quite incredible that starch could pass from the intestinal canal into the kidneys or bladder; and the explanation which we have just offered may, at least, assist in accounting for the appearances described by Mr Balman.]

HÆMATURIA FROM THE VAPOUR OF TURPENTINE.

The following case is reported by Dr Colton, of the New York Hospital:—"September 17, 1850.—Bennett Jones, 38, English seaman, recently arrived in port, in a ship employed in the exportation of turpentine from Wilmington, in Carolina, was attacked four days ago with severe pain in back, fever, anorexia, and thirst; urine became scanty and bloody, and thus continued until admission. Now no blood is perceived on applying the usual tests for albumen. Patient states, that during the passage the odour of the turpentine was so strong as to drive the men from their berths below; and that the urine of all hands had a strong terebinthinate smell. Also states, that he has been engaged in the turpentine trade for two years, and that he has during that time known many—not less than a dozen—who have been attacked in the same way, with lumbar pain and bloody urine, which has continued for some days, and then passed off, leaving the subjects perfectly well. Has himself been attacked once before, and recovered in a few days. Now the patient is free from fever, has fair appetite, and feels quite well, but feeble.

"September 24th.—Discharged, cured.

"Dr Harris of the city dispensary informs me, that last summer three cases with the same symptoms fell under his notice. They lived in the city, but had been hard at work for two days and two nights, engaged in unloading a vessel freighted with turpentine."—*New York Journal of Medicine*, November, 1850.

MIDWIFERY AND DISEASES PECULIAR TO WOMEN.

EXTRA-UTERINE PREGNANCY. BY DR CARTER JOHNSON.

After detailing an interesting case of this affection, in which gastrotomy was performed shortly after the completion of the full term of pregnancy,—the mother dying in the third week after the operation,—Dr Johnson appends the following remarks:—

"An interesting point in the history of the case is the unusual length of time that the fœtus must have lived. Velpeau (Dictionnaire de Médecine, vol. xiv., Art. Grossesse Extra-Uterine,) says, 'That extra-uterine pregnancy usually terminates before the fifth month by the death of the fœtus or by the rupture of the cyst;' and again, 'It is rare that the fœtus continues to live beyond the third or fourth month.' In this case the development was fully as great as at ordinary term, and the weight of the fœtus was nearly two pounds greater than the average weight of a full grown fœtus. May not indeed the fœtus have continued to be nourished and to grow, subsequently to

the termination of the ninth month? I can conceive of no reason why it should not, unless it be that the compression to which it was subjected by the contraction which took place at the close of the ninth month was sufficient to cut off its circulation, and thus to produce its death; and this we can scarcely suppose, as these contractions were exerted upon a sac filled with fluid, and were moreover so feeble as not to produce a separation of any portion of the placenta. The cause of death in these cases previous to the rupture of the sac is a most interesting subject, and one upon which the profession has but little information. In opposition to the views of Velpeau, the statistics given by Dr Campbell in his essay on this subject, would show that it is not uncommon for the fœtus to live during the full term of utero-gestation. 'In ninety-eight cases,' he says, 'in which we can decide, or nearly so, on the period of pregnancy, the fœtus in *seventy-nine* patients died at the close of nine months, or soon thereafter.'

"According to the statistics given by Dr Campbell in his essay, above referred to, the operation of extraction in these cases has been remarkably successful. He tells us 'that of thirty cases in which gastrotomy was performed *twenty-eight* patients recovered. Such success, greater than that which attends ordinary amputations of the extremities, would afford a strong indication for the operation.'"—*Philadelphia Medical Examiner*, September 1850.

ALBUMINURIA DURING PREGNANCY.

In a report on the thesis of M. Blot, lately laid before the Surgical Society of Paris, by M. Forget, he states the following conclusions as the result of his examination:—

1. Albuminuria in pregnant women, must not be regarded as synonymous with the expressions albuminous nephritis, Bright's disease, &c. This albuminuria should be studied as a symptom, not as a disease.
2. Albuminuria is very frequent in pregnant women (41 in 205).
3. In almost every case, it has for its cause a simple renal hyperæmia,—a functional disorder of the kidneys.
4. A *first* pregnancy is one of the most evident predisposing causes.
5. Albuminuria in pregnant females is complicated with symptoms more or less variable.
   a. Dropsy of the cellular tissue, or of the serous cavities.
   b. Lumbar pains.
   c. Absence of attendant fever; also, of amaurosis.
6. Most frequently the albuminuria disappears almost immediately after labour.
7. It is an affection of little importance, having no marked influence upon the different stages of pregnancy or on the puerperal state.
8. In every case of convulsions observed, albuminuria has been found present; but the reverse of this is very far from holding true.
9. There is no doubt that a certain relation exists between albuminuria and convulsions. But it does not follow from this that these two morbid conditions are connected together as cause and effect.
10. Albuminuria, by exhausting more or less the source of the plastic elements of the blood, predisposes to hemorrhage after delivery.
11. In the treatment we should be chary of bloodletting.
12. If the symptoms of cerebral congestion are present, bleeding may be replaced by powerful revulsives.—*L'Union Médicale*, 12th November 1850.

PRURITUS OF THE VULVA IN CHILDREN. BY M. VALLEZ.

It is not very uncommon to find young children complaining of distressing itching of the vulva and anus. This forces them to rub these parts, which leads sometimes to violent irritation in them. The affection is serious in consequence of its occasionally inducing bad habits which may continue to be practised after the original cause of the itching has disappeared, namely the

presence in the genital regions of small ascarides, designated by Rudolphi by the name of *oxyures*.

Of this affection M. Vallez has observed two cases. In one, after having unsuccessfully used a local treatment for some days, he carefully examined the parts, and was astonished to find in the fossa navicularis, and around the fourchette, a quantity of small worms which by their motions produced the irritation. He immediately prescribed hip baths, each containing in solution half a pound of saltpetre. After the third bath, the child was quite cured.

In the other case, the patient, a young girl, had for two years endured great suffering from the irritation of the vulva. A great variety of means had been resorted to in order to relieve her, but with no effect. On examination, M. Vallez detected the presence of the *oxyures*. Two of the saltpetre hip-baths effected a cure.—*Bulletin Général de Therapeutique*, 15th Oct. 1850.

MATERIA MEDICA AND THERAPEUTICS.

ANTHELMINTIC PROPERTIES OF THE MULBERRY. BY PROFESSOR LANDERER, OF ATHENS.

According to Dr Landerer, the bark of the root of the black mulberry, *Morus nigra* (not the white), has in Asia-Minor an equal, if not greater, reputation than pomegranate root bark, as a remedy for tape-worm. It is employed in the fresh state to form a strong decoction, which is drunk by those labouring under worms. The discharge of the parasite is almost unfailing, if a purgative is given subsequently, and for this purpose they commonly prefer a decoction of three or four drachms of the dodder (*Cuscuta Epithymum*), which is a very common plant in the East. This last is there much in vogue as an effectual purgative, and especially as a specific in jaundice.—*Vierteljahrschrift für die Praktische Heilkunde*, vol. i. 1850.

ANTISEPTIC PROPERTIES OF CHLOROFORM. BY M. AUGEND, OF CONSTANTINOPLE.

M. Augend has communicated to the French Academy of Sciences the following experiments, which establish a marked line of demarcation between ether and chloroform, and point out in the latter a remarkable property which has hitherto escaped the attention of chemists.

If three wide-mouthed, ground stoppered bottles are taken, and into one we put a few drops of ether, into another a few drops of chloroform, and leave the third *in statu quo;* then place in each a piece of beef, secure the stoppers, and leave them during summer weather, the following phenomena are observed:—The flesh, naturally of a reddish-brown colour, passes at once to a vermilion-red under the influence of the vapour of chloroform mixed with air, whilst it undergoes no change in the ether. Such are the immediate effects, but at the end of a week the results are still more distinct. The flesh preserved in the air has changed its colour slightly, that which is preserved in the ether has become brown, whilst that in the chloroform has acquired the appearance of boiled meat. On opening the bottles the meat in the air will have become offensively putrid, and the same thing occurs in the presence of the ether, whilst there is no change in the flesh kept in the chloroform, apart from the sweet taste and peculiar odour of the latter.

These antiseptic properties of chloroform have furnished interesting results to M. Augend. He has found that $\frac{1}{1000}$th part of it suffices to preserve a mass of muscular flesh. Not less remarkable is the facility with which the vapour permeates the densest tissues. Chloroform has this advantage over creasote, that it does not coagulate the albumen; nor is it on its part decomposed by the muscular fibre.

The most apparent action of the chloroform, not only on muscular substance, but also on the fleshy pericarps of fruits and seeds, is an immediate contraction of the fibre or the parenchyma, which causes the watery juices to flow out on the bottom of the vessel in which the experiment is made.

The author reports a number of trials made on different animal substances, and modified in various ways, from which it results that the antiseptic properties of chloroform may be of use to naturalists and anatomists, who wish to preserve specimens for some time. He even thinks that it might be applied to the preservation of bodies in cases of homicide, where these might require to be kept for some time for medico-legal purposes.—*Gazette Médicale*, 16th November 1850.

ON THE CHANGES WHICH CALOMEL UNDERGOES IN THE INTESTINAL CANAL. BY DR GEORGE V. ŒTTINGER.

The author has published his researches in a thesis at Dorpat, and his experiments have led to the following conclusions. They establish, in the first place, the truth of Mialhe's statements, that calomel, when boiled with concentrated solutions of common salt, or of sal ammoniac, undergoes conversion, but only in a small proportion, into bichloride of mercury; red precipitate, on the contrary, forms a soluble oxychloride very readily with sal ammoniac, and sparingly with common salt, and also with gastric juice. Metallic mercury and "Hahnemann's oxide" (an ammonio-subnitrate) undergo an almost unappreciable change with sal ammoniac, and only at a boiling heat; white precipitate undergoes the change with sal ammoniac at ordinary temperatures.

On the other hand, however, he found that the alkaline chlorides in the gastric juice were in much too minute quantity, and too diluted, to exert any obvious action on calomel; and that the gastric juice, even when it contained a much larger proportion of chloride of sodium than usual, remained without action on calomel and metallic mercury. Mialhe's experiments thus lose all value in reference to the explanation of the modus operandi of calomel.

The author further found, that the albuminous substances in the stomach dissolve a small proportion of calomel, and that this property which albumen possesses is not dependent upon the presence of acids or alkalies.

Lastly, Dr Œttinger found that, after large doses of calomel, there were found in the stomach, and both larger and smaller intestines, along with traces of inflammation of the mucous coat (*spuren eines katarrhs*), always blackish grains and masses of sulphuret of mercury, and an increased proportion of biliary matter. The last is the cause of the green colour of calomel stools, and not the sulphuret of mercury, which can be readily distinguished from the green matter beside it. The increased secretion of bile shows itself also by the distension of the gall bladder.—*Vierteljahrschrift für die Praktische Heilkunde*, vol. iii. 1850.

[We never had any belief in M. Mialhe's hypothesis, which accounted for the passage of the insoluble calomel into the system by its being converted into the soluble bichloride by the action of the alkaline chlorides in the gastric secretions. What we disbelieved on theoretical grounds, appears to be amply refuted experimentally by Dr Œttinger.]

NEW RE-AGENT FOR SULPHATE OF QUININE. BY M. VOGEL, JUN.

Brandes showed, many years ago, that sulphate of quinine, exposed to the contact of chlorine, and then moistened with ammonia, assumes a green colour. This was the only test known as characteristic of this salt. M. Vogel has discovered another test of this kind, which is applied as follows:—Into a solution of sulphate of quinine, in water or alcohol, drop a little chlorine water, and then add a concentrated solution of ferrocyanide of potassium; the liquid immediately assumes a bright red colour. If the solution of the ferrocyanide is not concentrated, the re-action will be incomplete; but the colour is developed on adding a few drops of ammonia. Dry crystals of the sulphate may be tested in the same way. The red colour passes, after some time, to green. M. Vogel finds no other organic base to present this reaction; it may, therefore, be taken as characteristic of quinine.—*Bulletin de Therapeutique*, July 30, 1850.

PREPARATION OF ATROPINE BY MEANS OF CHLOROFORM. BY M. RABOURDEN, OF ORLEANS.

The following is M. Rabourden's process:—Take the belladonna fresh, at the time when it is commencing to flower, bruise it in a marble mortar, and subject it to the screw press to extract the juice, which is to be heated to about 180° Fahr., to coagulate the albumen, and filtered. When the juice thus clarified is cold, add a drachm of caustic potash and an ounce of chloroform to every thirty-five ounces; agitate it for a minute, and set it aside to rest. In about half an hour, the chloroform charged with atropine settles to the bottom, having the appearance of a greenish liquid; the supernatant liquid is then to be decanted, and measured portions of water added and again poured off, till it comes away limpid. The chloroform liquid is then to be transferred to a tubulated retort, and distilled on the water-bath till the whole chloroform has been recovered. The residue is then to be taken up with a little water, acidulated with sulphuric acid, which dissolves the atropine, leaving behind a green resinoid matter; the filtered solution is colourless, and to obtain from it pure atropine all that is required is, to add to it a slight excess of carbonate of potash, to collect the precipitate, and dissolve it in rectified spirit. The alcoholic solution, by spontaneous evaporation, gives beautiful acicular groups of atropine.

In the absence of fresh belladonna, well prepared officianal extract may be used. An ounce of extract of belladonna, prepared from the clarified juice of the plant, was dissolved in about four ounces of distilled water, and half a drachm of caustic potash, and an ounce of chloroform added. The whole was agitated for a minute, allowed to settle for half an hour. The chloroform charged with atropine was deposited, the supernatant liquid was poured off, and replaced by water which was renewed three times. The chloroform solution, on being exposed to the air, rapidly evaporated, leaving a greenish crystalline mass, composed almost entirely of atropine. This was taken up with acidulated water, filtered, and precipitated by carbonate of potash. The precipitate dissolved entirely in rectified spirit, and gave, by spontaneous evaporation, fine needles of atropine, grouped in tufts.

M. Rabourden thinks this process capable of being applied to a variety of substances containing organic alkalies.—*Report of Académie des Sciences*, in *L'Union Médicale*, October 19, 1850.

Part Fifth.

MEDICAL NEWS.

EDINBURGH MEDICO-CHIRURGICAL SOCIETY.

SESSION XXX.—MEETING I.—*November* 20, 1850.—JAMES SYME, Esq., President, in the Chair.

CASES OF HYDROPHOBIA.

Dr J. A. Sidey communicated a recent case of hydrophobia. The patient, a boy 15 years old, was severely bitten by a dog on the 5th July. The wounds were immediately washed, and then cauterised with lunar caustic. The dog was soon afterwards destroyed. On the 16th the boy witnessed the execution of Bennison, for murder. The scene frightened him, and greatly affected

his spirits. On the 18th symptoms of hydrophobia showed themselves, and on the 20th the disease terminated fatally. The body was carefully examined, but neither by the naked eye, nor by the microscope, could anything abnormal be observed in the brain, or in the course of the eighth pair of nerves.

(Dr Sidey's case has been published in the December No. of this Journal.)

Dr James Struthers, read the details of a very similar case, which he had lately met with. The patient was a boy aged 8½. He was bit by a dog, which was soon afterwards destroyed. The bitten parts were excised, and the wounds touched with caustic within one-and-a-half hours of the time of their infliction. The patient was frightened by some of his companions "playing the ghost," a month after the bite. Symptoms of hydrophobia first showed themselves on the thirty-first day after the bite, and the patient died on the thirty-third day. A post-mortem examination was made. Redness of the pharynx, accumulation of mucus in the air-passages, and the presence of frothy blood in the heart, were the chief morbid appearances.

(Dr Struthers' case is published in the present No. of the Monthly Journal.)

The *President* had seen Dr Sidey's patient about an hour before death. Of the nature of the disease there could be no doubt. The cases just read were narrated in distinct impressive terms. He observed that they added nothing to our knowledge of the treatment of hydrophobia.

Professor Dick, of the Veterinary College, said, that the two cases just related did not in the least degree shake the opinion which he had formerly expressed at a meeting of the Society, viz., that hydrophobia was not communicated by the bite of the dog, but was a nervous affection induced by the imagination. He recapitulated the arguments, in support of this opinion, which he used in discussing Mr Lucas' case, 16th May 1849, and remarked that the boys treated by Dr Sidey and Dr Struthers, had both been subjected to causes which created a profound impression on the nervous system. The only point in which the post-mortem appearances of rabies and of hydrophobia seemed to agree, was in the existence of ecchymoses in various parts of the pharynx and air-passages. These extravasations had been observed in both the cases just communicated; and he had himself, in dissecting rabid animals, often noticed an injected condition of the superior turbinated bones, and cavity of the nose, sometimes of the frontal sinuses. In 1849 he had seen some cattle, which had died of rabies, in the neighbourhood of Biggar, and the Society might recollect that he had predicted the probable spread of the disease, in an epidemic form, in that district. Within six weeks of the occurrence of cases at Biggar, rabies had spread very extensively throughout Lanarkshire; it soon passed into other counties, and the last cases which he had heard of occurred in some kennels in Aberdeenshire. He had also heard of cases in horses at Piershill.

Dr Christison felt unwilling to renew the debate of 1849. It was only necessary to study the writings of the foreign veterinary surgeons, and above all, of Hertwig, to become convinced, by the most distinct evidence, that rabies could be communicated by inoculation, from one animal to another, and that hydrophobia was caused in the human subject by the bite of a rabid animal. There was a disease which, in many respects, nearly resembled rabies, and which was incommunicable by inoculation. Now, as he was no believer in the extreme frequency of rabies, he suspected that a number of Mr Dick's cases of epidemic rabies might with propriety be ranged under this head. The great practical point to which he desired to direct attention was, that hydrophobia—the manifestation of rabies in the human subject—was undoubtedly more common in Edinburgh now than formerly. Mr Syme had, in 1838, treated the only case which had been known to occur in Edinburgh for fifty years, and now *three* cases, all occurring in the town or in its vicinity, had been communicated to this Society within the short period of eighteen months. It was known that when rabies appeared among dogs, it was eradicated with extreme difficulty; and it had been shown in Paris, that the disease usually spread by the agency of the curs without owners, which were permitted to prowl about

the streets, and were to be seen at night congregated about the heaps of refuse, prepared for the scavenger. That numbers of these useless, masterless dogs, infested this town, was well known, and some very summary means of reducing their number should be devised. The mere issuing of an edict, enjoining the muzzling of dogs during hot weather, was not sufficient to prevent the risk which the inhabitants of a town were subjected to from rabid animals.

Professor Dick stated his opinion, that the annual edict directing dogs to be muzzled, was likely to be productive of more harm than good. He had always opposed the issuing of this temporary regulation, and at his suggestion it had, on one occasion at least, been relaxed, without unpleasant consequences following.

Dr W. T. Gairdner made some observations on the pathology of the disease. The thick frothy mucus was evidently a secretion from the air-passages, and not merely saliva mixed with air, as was vulgarly supposed. In Dr Struthers' case, air was found in the right side of the heart, and in the veins of the pia mater; there was no other evidence of putrefaction detected, and the air was not found in the pulmonary veins. Yet, during life, there were neither the signs of air in the circulation, nor was cough observed.

Dr Simpson remarked, in relation to the theory which ascribed the symptoms of hydrophobia to a lesion of the eighth pair of nerves, or of its origins, that physiologists had observed, after section of these nerves, that the efforts of expectoration ceased, and that the pulse of the animals subjected to the experiment sometimes became irregular.

Some remarks were then made by various members, upon different cases in which death from hydrophobia had resulted long after the infliction of the bite of the dog. One case was instanced, in which death had taken place, and in which no abrasion of the cuticle was known to have taken place. This case occurred at Leeds.

Professor Dick having mentioned his intention of inoculating a horse with some of the saliva procured from Dr Struthers' patient,

Dr John Gairdner begged to protest against any conclusions which might be drawn from such an experiment. The investigations of Hertwig, and other foreign authorities, tended to prove that, after a certain period, the virus lost its power. The period usually mentioned was twenty-four hours; and a full week had now elapsed since the death of Dr Struthers' patient. If it were desirable to re-investigate the subject, the experiments should be very numerous, undertaken by a number of individuals interested in the inquiry, and performed upon a variety of different animals, with all the precautions requisite to ensure success. A single experiment, such as Mr Dick proposed, would be absolutely worthless.

FISTULA OF THE STOMACH.

Dr Robertson gave a short sketch of the history of a patient at present under his care in the Infirmary, in whom a communication between the stomach and external surface of the abdomen, had existed for several years. The perforation had resulted from internal causes, and the present condition of the patient encouraged the hope that a series of experiments upon the digestive process, might be undertaken without prejudice to the woman's health. Dr Christison, Dr Spittal, Dr Maclagan, and Dr Simpson, made some observations on the case, and a committee was named to make the necessary investigations on the subject. The committee consists of the President, Dr Christison, Dr Bennett, Dr Douglas Maclagan, Mr Goodsir, and Dr W. Robertson.

(This case is published at page 1 of this No.)

EDINBURGH OBSTETRICAL SOCIETY.

SESSION X.

MEETING I.—*December* 11, 1850.—Professor SIMPSON, President, in the Chair.

CASE OF THE DELIVERY OF A DOUBLE MONSTER. BY DR STUART, OF CHIRNSIDE.

[This case will be found detailed at full length among our Original Articles, p. 5.]

CASE OF PERFORATION OF THE PERINEUM BY THE ARM OF THE CHILD DURING LABOUR. BY DR GRAY, OF PERTH.

[This communication will be found at full length among our Original Articles, p. 7.]

CENTRAL LACERATION OF THE PERINEUM. BY DR THATCHER.

Mrs C——, in her first pregnancy, had been in labour for some hours previous to her sending for me. The first stage was over, and the head advancing correctly in the pelvis, and nearly on its outlet. The parts were well relaxed; the pains moderate; but the patient unhappily most restless and impatient,—in fact scarcely controllable. The sacral part of the vagina appeared much *deeper* than natural, and the head was constantly pressing backwards forcibly, as if wishing to be extruded through the posterior part of the perineum and anus, instead of the superior and natural direction. As the perineum distended, it was quite evident that it was unusually elongated; the *set* of the pelvis reminded me much of that of the Hottentot Venus, pressing pubis and sacrum *out* of the ordinary line. Every exertion was made to guide the head superiorly to its proper position by the fingers, but this was of little use; and before the forceps could be employed, a sudden terrific pain, aided by the *over-exertion* of the patient,—to my surprise and great distress, and despite of every retarding exertion,—forced the head through the perineal space, between the lower commissure of the labia and the anus, keeping the lower fraenum of the labia entire, as also, fortunately, the sphincter ani. The body was extracted in the same manner, as also the placenta. The patient was told that she had hurt herself by this unhappy exertion.

In the afternoon the parts were examined, and the above statement confirmed. The divided perineum was uniting at its edges, and appeared like two portions of a saw closing. They were kept united by ligature, and healed most favourably. The vagina also was kept clean, and after a fortnight all the parts were in their normal state. I have heard of two other similar cases, but this rare case is one evincing what may occur in any deviation from natural conformation, in defiance of the best care and aid.

LOCAL PARALYSIS IN INFANCY. BY DR SIMPSON.

Dr S. called the attention of the Society to the frequency of local paralytic attacks during infancy and childhood, and pointed out the following circumstances as the most important points in their history:—

1. The paralysis most frequently seems to affect a single limb—as one leg or one arm—sometimes a few fingers only; occasionally it appears in the form of hemiplegia, affecting one whole side; sometimes in the form of paraplegia. Dr S. mentioned a case in which the paralysis occurred in early infancy, and affected both lower extremities, the left upper extremity, and the left side of the face, the child, now several years old, being very acute and intelligent. 2. The side of the face, but more particularly the upper and lower extremity, when paralysed in infancy, do not grow in relative proportion with the corresponding healthy parts; so that, when the individuals affected reach adult life, the para-

lysed extremity appears small, diminutive, and shorter than natural. 3. The paralysed limb does not appear to want sensation, and the motory muscular power, although greatly diminished, is not entirely abolished. When the local paralysis is seated in the leg, the person hence usually walks imperfectly, throwing out the foot at each step with a flap-like motion, and often with the toes or external surface of the foot somewhat drawn in, as the leg is each time extended. 4. The disease generally comes on during the first three years of life, and especially during the currency of that morbidly irritable state of the nervous system, which co-exists with teething. Dr S. had seen an instance in which two children of the same family were affected within a week of each other. 5. The disease generally supervenes very suddenly, sometimes in the course of a single night, and is often, at the time of the attack, accompanied with little or no constitutional derangement; but occasionally it comes on with a fit of convulsions, or other symptoms of some temporary cerebral derangement. 6. The affection is frequently first noticed immediately after fever, especially after the eruptive fevers; and occasionally it comes on during the period of convalescence from them. Dr S. described a case of paraplegia in a child three years old, which came on during the convalescence from scarlatina—the patient going to bed apparently well, and waking paraplegic, and astonished at her own want of power of movement in both her legs. Intestinal irritation in some cases appears to be the exciting cause. 7. When the patients do not recover from the paralysis within a few days after the attack, under antiphlogistic measures (when the state of the system has allowed them), and careful correction of the condition of the intestinal canal and other functions, the paralytic affection almost always proves chronic, and, indeed, permanent. Dr S. had seen counter-irritation to the spine, galvanism, &c. &c., employed, but without success. He had seen three instances in which small and long-continued doses of nux vomica had appeared to act beneficially in diminishing the state of paralysis. Keeping all the functions of the body as near the standard of health as might be, friction and bathing of the affected limbs, and inculcating as much muscular exercise of them as possible, seemed to be the principal indications of treatment, when the disease had already passed into the chronic type. 7. The true pathology of the disease was as yet little known, though the affection was of frequent occurrence. No autopsic investigations appear to have been made with the view of ascertaining the state of the brain, spinal cord, and nerves, in the local paralysis of infancy.

RAVAGES OF THE CHOLERA IN JAMAICA.

A letter, of the 27th November last, from Dr Gilbert Macnab, of Kingston, Jamaica, to his brother, superintendent of the Botanic Garden here, gives a fearful account of the devastations of the malignant cholera on that island. In Kingston alone, out of a population of about 30,000, no fewer than 6,000 have been already buried. Among these, we regret to learn, is the well-known physician and botanist, Dr M'Fadyen, author of the "Flora of Jamaica." He expired on the 24th ultimo, of cholera, brought on apparently by excessive exhaustion and fatigue, from attending to his practice. Those who had to do with the comparatively mild visitation of the disease in the large cities of this island, can alone form an adequate idea of the labours and anxieties of their brethren in Jamaica; who, alas! can, in these straits, receive, from their distant professional friends, nothing but sincere sympathy. Dr Macnab, "fagging night and day, and having himself had two smart attacks of cholera," alone attended Dr M'Fadyen on his death-bed, put the body unaided into the coffin, and, without help, buried him. The second volume of the "Flora of Jamaica" was nearly finished, and will be published by Dr Macnab, if he escapes the fearful epidemic.

VARIETIES.

COLLEGE OF PHYSICIANS OF EDINBURGH.—At the annual meeting of the College, held in December, Professor Simpson was elected President of the College for the ensuing year.

The Council have, with the sanction of the College, arranged a series of evening lectures to be delivered by Professors Goodsir, Bennett, Christison, Gregory, and Balfour, in the new hall of the College. The first lecture of the course, which Professor Goodsir has kindly undertaken, will take place on the evening of the 8th January, and its subject will be, "The Progress recently made in the Anatomy of the Circulating System."

MEDICO-CHIRURGICAL SOCIETY.—At the election meeting, held on the 18th December, Dr Begbie was unanimously elected President of the Society.

ROYAL SOCIETY.—Professor Faraday delivered the Bakerian lecture on Thursday the 28th of November, when he explained his recent discoveries in reference to the magnetism of gases. He finds that oxygen is magnetic like iron, and that nitrogen is not affected by the magnetic force, being, in this respect, in the same condition as a vacuous space. He also finds that the magnetism of oxygen decreases as the temperature of the gas increases, from which circumstance the magnetism of the atmosphere must be subject to constant variations, and it is probable that this will be found to account for the variations in the magnetic dip.—*Pharmaceutical Journal.*

PREPARATION OF ANHYDROUS PRUSSIC ACID.—The preparation of anhydrous prussic acid is much facilitated, according to Wöhler, by elevating the neck of the retort to an angle of 45°, and now and then slightly cooling it, so that most of the water may be condensed and flow back: the liquefaction of the chloride of calcium is then not to be feared. Between the neck of the retort and the U tube containing the chloride of calcium, he recommends the introduction of another vessel, with a small quantity of chloride of calcium or rough cyanide of potassium. The latter, as well as the tube with the chloride of calcium, is to be surrounded with water of 86° Fahr. If the high condensing vessel be surrounded with a mixture of ice and common salt, the prussic acid is immediately crystallized. The latter is developed by boiling a mixture of ten parts of prussiate of potash, seven parts of sulphuric acid, and fourteen parts of water, over an open charcoal fire. The residue may be converted into Prussian blue by boiling it with commercial nitric acid.—*Central Blatt. and Pharmaceutical Journal.*

ANECDOTE OF MARJOLIN.—While Marjolin was an *interne* at the Saltpêtrière, his surgical superior used to examine once a-month the notes which his pupils were expected to make, regarding the cases which had fallen under their observation in the hospital. At one of these examinations the eye of the master fell upon a case of cataract, treated by extraction, the narrative of which had been drawn up by Marjolin. It terminated as follows:—"*L'operation a parfaitement reussi; tout est sorti, excepté le cristallin,*"—The operation was perfectly successful; everything came out except the lens.—*Revue Médico-Chirurgicale de Paris.*

AMERICANA—The *New York Express* has a leader on Mr Littlefield, late janitor of the Medical College, where Dr Parkman was murdered, wherein Mr L. receives a severe castigation for travelling with a model of the college and wax-figures of Parkman and Webster.

Medical lectures in New York, it appears, have commenced under promising circumstances. A poem, instead of a scientific lecture, was delivered as an introductory, by Professor Paine of the new school.—*Boston Medical Journal.*

THE EL DORADO FOR PHYSICIANS.—*Extracts from the Fee Bill of the San Francisco Medical Society.*—For a single visit or advice, in a case in which no further visits are required, 32 dols.

For each visit in a case in which the physician is in regular attendance, or for advice at his office, 16 dols.

Every necessary visit on the same day to be charged, whatever their number, at the same rate.

When detained, for each hour, 32 dols.

For a visit as consulting physician during the night, 100 dols.

For visiting distant patients, 10 dollars to be charged for every mile from the city.

For an opinion involving a question of law, 150 dols.

For a *post-mortem* examination in case of legal investigation, 200 dols.

For a *post-mortem* examination made at the request of the family or relatives of the deceased person, 100 dols.

For the application of the forceps, 3000 dols.

For reducing recent luxations, 32 to 100 dols.

For removing of stone from the bladder, 500 to 1000 dols.

For the introduction of the catheter in ordinary cases, 16 to 32 dols.

For division of stricture of the urethra, 300 dols.

For examination per anum or vaginam, 50 to 100 dols.

For amputation of a finger or toe, 100 dols.

For trephining, 1000 dols.

For the operation for cataract, 1000 dols.

In all surgical operations not included in the Fee Bill, the charge to be discretionary with the surgeon.—From *British American Journal*, Dec. 1850.

REPORT OF AN OPERATION THAT WAS NOT PERFORMED.—In the "Standard" of Monday, November 18, there is a circumstantial and complimentary account of an amputation at the hip-joint, performed by Mr Guthrie, jun., with copious lists of all the distinguished surgeons who assisted, or were merely present on the occasion. It turns out from subsequent information, that the operator found it unnecessary to remove the limb higher than the trochanter, so that all this flourish of trumpets has been sounded for an affair no greater than amputation of the thigh. It is deemed prudent to deliver speeches before they are reported, and we beg to recommend a similar precaution in regard to the performance of operations.

PUBLICATIONS RECEIVED.

The Surgeon's Vade Mecum. By Robert Druitt, F.R.C.S. Fifth Edition. London, 1851.

Operative Surgery. By Frederic C. Skey, F.R.S. London, 1850.

Contributions to Clinical Medicine. By Robert L. Macdonnell, M.D. Montreal, 1850. (From the British American Journal of Medicine.)

The Crystalline Lens and Cataract. By Bernard Edward Brodhurst. London, 1850.

New York Register of Medicine and Pharmacy. Edited by C. D. Greswold, M.D. Vol. I., Nos. 4 and 5.

The Correlation of Physical Forces. By W. R. Grove, M.A., F.R.S. London, 1850. Second Edition.

Dr Friedrich Günsburg of Breslau requests us to intimate that the "Zeitschrift für Klinische Medizin," which he edits, will in future contain critical notices of English medical works and journals. Authors, editors, and publishers are requested to address their communications to *Dr Günsburg*, and to forward them through some of the foreign booksellers to the care of Ed. Trewdent, publisher in Breslau.

EXCHANGE LIST.

The following journals reach us regularly :—

British and Foreign Medico-Chirurgical Review.
Dublin Quarterly Journal of Medical Science.
American Journal of Medical Science.
Psychological Journal.
Annales Médico-Psychologiques.
Präger Vierteljahrschrift.
Veterinary Record.
Pharmaceutical Journal.
London Journal of Medicine.
Chemical Gazette.
Provincial Medical and Surgical Journal.
Dublin Medical Press.
Medical Times.
Medical Gazette.
Boston Medical and Surgical Journal.
British American Journal of Medical Science.
Philadelphia Medical Examiner.
Gazette Médicale de Paris.
Gazette des Hôpitaux.
L'Union Médicale.
Bulletin Général de Thérapeutique.
Henke's Zeitschrift für die Staatsarzneikunde.
Journal für Kinderkrankheiten von Behrend und Hildebrand.
Caspar's Wochenschrift.
Zeitschrift für Rationelle Medicin, von Henle und Pfeufer.
Schmidt's Jahrbücher.
New York Journal of Medicine.

The following reach us irregularly :—

Archives Générales de Médecine.—(Six Nos. still due.)
Bibliothek for Læger.
Hygiea.
Journal de Médecine et de Chirurgie Pratiques.
Revue Médico-Chirurgicale de Paris.
Gazzetta Medica Lombarda.

Of the following we have received no numbers for a year past :—

Zeitschrift der K. K. Gesellschaft der Aertzte zu Wien
Oesterreichische Medicinische Jahrbücher.

N.B.—French journals are forwarded to us by J. B. Baillière, Paris. German journals should be sent through the house of Hartmann in Leipsic, to care of Williams and Norgate, Henrietta Street, Covent Garden, London.

No foreign parcels can be received unless sent free. This rule is absolute, and its infringement has frequently compelled us to stop exchanges, especially with American journals.

TO CORRESPONDENTS.

An "English Subscriber," a "Constant Reader," &c., will receive the information which they seek, if they will forward their names and addresses to the Editor. For many reasons, we must decline to answer any questions proposed to us in letters not authenticated with the writer's name. We are anxious that it should be directly understood, that in future no letter or communication can be acknowledged unless so authenticated.

Part First.

ORIGINAL COMMUNICATIONS.

ARTICLE I.—*Case of Hydrophobia, occurring Twenty-eight Days after the Bite of a Rabid Dog, and Terminating in Death on the Fifth Day after the Commencement of Symptoms ; with Remarks by* P. REDFERN, M.D., Lecturer on Anatomy and Physiology, and on Histology, at the University and King's College, Aberdeen.

THE subject of this case was a female child, living at Blackburn, eight miles and a-half from Aberdeen, and attended during its illness by my friend, Dr Rainy of Kintore, who has very kindly furnished me with the following account of the symptoms and progress of the case, which I can substantiate as corresponding precisely with the statements made by the parents and friends of the patient to myself immediately on the termination of the case.

The child was two years of age, and was bitten by a dog on the 1st of March 1850. Its right cheek was bitten completely through, and severely lacerated; and on the forehead were two other severe lacerated wounds, reaching to the bone. The wounds were carefully washed and dressed by my colleague, Dr Rainy of Aberdeen, who happened to be in the neighbourhood at the time, and they soon healed perfectly. The dog was pursued for a distance of two miles, and was lost sight of in a wood ; but he was found next morning at a farm-house in the neighbourhood, apparently in a state of great exhaustion, and was killed by the servants. It was afterwards ascertained that, three days before he appeared at Blackburn, the dog was rabid, and was pursued and fired at in a neighbouring parish. At that time he bit several dogs, which were instantly killed. A dog which was bitten by him, on the same day as the child, is said to have become mad within twenty-four hours.

The child's general health continued good until Friday, March 29th (twenty-eight days after the bite), when an unusual degree of alarm and anxiety were manifested by it ; these symptoms becoming so much worse on the Saturday and Sunday, that little doubt remained that hydrophobia was commencing. On offering fluid out of a polished tin can, out of which the child had always been in the habit of drinking, it refused to take it, closing its mouth, and appearing excessively alarmed lest this vessel should be again offered to it ; yet, at the same time, and during a period of twenty-four hours from the commencement of the attack, it drank freely out of any other vessel. The state of alarm and anxiety increased steadily ; intermittent general spasms came on, and became more and more violent as the disease advanced. The entrance of any person into the room—touching the surface to feel the pulse—even looking at the child, or offering it fluid, caused violent convulsions, with spasmo-

dic action of the diaphragm, indicated by pain and constriction in the epigastric and hypochondriac regions. During the first days the intermissions were occasionally of an hour's duration, but later in the disease the spasms occurred every ten or fifteen minutes. There was neither opisthotonos nor other form of tetanic spasm,—for the child lay on its side or clung to its mother during the spasms, or when any one attempted to touch it. Moreover, the intermissions were well-marked throughout. Not a moment's sleep took place during the whole illness, the spasms always coming on when, on the first days, the child attempted to sleep; whilst no tendency to sleep was noticed later in the disease. On the day previous to death, the little patient was at times furious, "*wicked*," and attempted to scratch and bite. The pulse during the first and second days of the illness was from 95 to 100 in the minute, feeble, and easily compressed, afterwards becoming more feeble, and occasionally above 130 in the minute. The skin was rough and dry at first, but became soft and moist during the last twenty-four hours. The bowels were confined at the commencement, but there were two free motions on each of the last three days of the illness. The urine was passed regularly, but in small quantity. On the third day after the attack, a reddish blush appeared in the cicatrices, and extended fully an inch around them, but no suppuration occurred. During the last twelve hours considerable foaming at the mouth took place, and the frothy fluid was immediately removed by the attendants. Death occurred in the interval of convulsions, apparently from exhaustion, at half-past 9 o'clock A.M., on Wednesday the 3d of April, the fifth day after the first symptoms were noticed, and thirty-three days after the bite of the dog. During the whole illness, two grain doses of calomel were given every two hours, without purging or other sensible effect. Two drop-doses of solution of morphia were administered during the last thirty-six hours, between the paroxysms, but without any apparent effect. Not more than twenty drops were given altogether.

The *Post-mortem Examination* was made by myself with the greatest care, in the presence of Drs A. and G. Rainy, twenty-seven hours after death, and the appearances were as follows:—

Body tolerably plump; abdomen distended with flatus; countenance expressive of great distress. A deep radiate cicatrix indicates the position of the wound on the right cheek; and two cicatrices, 1 inch and 1½ inch long respectively, adhering to the frontal bone, show the position of the other wounds.

Head and Spine.—Cranial integuments healthy; calvarium removed with great difficulty, owing to adhesions of the dura mater along the line of the sagittal suture, and in the position of the anterior fontanelle, which is yet open to the extent of three-fourths of an inch. On the external surface of the dura mater of the spinal cord, opposite to the 4th, 5th, and 6th dorsal, to the 12th dorsal, and two upper lumbar vertebræ, a thin layer of extravasated and partially coagulated blood exists, but every other part of the membrane appears healthy. The pia mater of the brain and cord appears slightly increased in vascularity. The posterior sub-arachnoid space, at the lower part of the cord, contains an ounce and a-half of clear serous fluid, and a small quantity of similar fluid exists at the base of the brain. The substance of the brain and cerebellum present larger and more numerous bloody points than usual; the lateral ventricles contain half a drachm of clear serum; the velum interpositum is considerably congested; but all the other parts of the encephalic and spinal mass are perfectly healthy, as shown by microscopic examination of every part presenting the slightest appearance of softening or other disease. The medulla oblongata, and the origins of the cranial nerves, especially of the fifth and eighth pairs, were subjected to a most careful microscopic examination, but not a trace of disease could be detected.

Neck and Chest.—The pharynx, fauces, tongue, salivary glands, larynx, and trachea, have been carefully examined, but show no evident traces of disease.

The papillæ circumvallatæ appear a little larger than usual. No vesicles exist under the tongue. Pleuræ healthy; lungs both crepitant throughout; lower lobes of both slightly congested; bronchi contain a reddish frothy fluid. Heart and pericardium healthy.

Abdomen.—Two reddened spots, about the size of the end of the little finger, exist on the mucous membrane of the back of the stomach near the œsophagus. The stomach contains two ounces of dark fluid like coffee-grounds. The small intestines are distended with gas, and the ileum contains a large quantity of very dark-greenish matter of considerable consistence. Along the whole length of the small intestine the solitary glands are of large size, closed, and they project considerably, appearing to be in equal number in the jejunum and ileum. The patches of Peyer are not unusually prominent. The large intestines are healthy, and contain dark feculent matter in small quantity. The liver appears very large, but cannot conveniently be weighed; it is of a pale yellow colour, mottled, and has all its cells full of very large oil globules, as in an extreme state of fatty degeneration. Pancreas healthy. Spleen small, firm, and healthy in structure. Kidneys pale but healthy. Bladder contains three and a-half ounces of very turbid urine.

Remarks.—The very decided madness of the dog which caused the wounds in the subject of this case,—the complete inoculation which such extensive wounds on a part not covered with clothes were calculated to produce,—the total absence of the slightest suspicion that the disease owed its origin to the influence of the imagination, or to any other cause than the bite of a rabid dog,—the gradual and insidious commencement of the symptoms after an interval of perfect health and the healing of the wounds,—the occurrence of redness in the neighbourhood of the injuries,—the steady, determined, and dreadful increase in the intensity of the symptoms as time wore away,—the inexpressibly horrible nature of the symptoms themselves, and their termination in death without the indication of any physical change capable of producing or explaining them, alike lead to the conclusion, that the disease was the most horrible of all human afflictions—hydrophobia. The certainty attending the nature of the case rendered me doubly anxious to make a careful examination of the body, and I was enabled to do this under the most favourable circumstances by the kindness of my friends, Drs A. and G. Rainy. The post-mortem appearances most worthy of notice are the slight congestion of the pia mater of the brain and cord, the more considerable congestion of the velum interpositum, the quantity of the fluid in the posterior sub-arachnoid space, and at the base of the brain, and the slight extravasations outside the dura mater of the cord in two circumscribed spots. The thinness of the clots of extravasated blood, and their existence outside the dura mater, preclude the idea of their having been in any way concerned in the production of the symptoms. It is impossible to state with certainty how they were produced, but the conjecture that they resulted from the convulsions appears liable to fewer objections than any other. What influence may have been exerted by the slight congestion of the pia-mater of the brain and cord, of the velum interpositum, and of the substance of the cerebrum and cerebellum, it seems quite im-

possible to determine in the present state of our knowledge; for, whilst in amount it appears to fall very far short of a congestion capable of influencing the nervous centres to any material extent by compression, we must not forget the lesson taught us by the pathology of those cases of cerebral apoplexy in which no trace of congestion or effusion is found after death. Moreover, congestion appears by no means a constant occurrence in hydrophobia, as it was absent in Dr J. A. Sidey's case [1], examined by Professor Bennett, and in many instances occurring to M. Gendrin, and others. In the cases of Dr Lucas[2] and Dr James Struthers[3], Dr W. T. Gairdner found slight congestion; but neither he nor Professor Bennett could detect any change of structure in the nervous centres, or eighth pair of nerves, though we may be satisfied that they used every existing means for that purpose.

In the present instance, I was led to the careful microscopical examination of every suspicious part of the nervous centres, and especially of the eighth pair of nerves, by the suggestion of Dr Bennett, in his excellent article on Hydrophobia in the "Library of Medicine," but the structure of every part appeared perfectly healthy. Such results, in several instances, after the use of our most important means of research by different individuals, point to the imperfection of our knowledge, too plainly to be misunderstood. We are still in great darkness regarding the normal action of the nervous centres, and it is, therefore, extremely improbable that we shall be able to comprehend their diseased actions as long as this state remains. Yet it is the duty of every one who is furnished with the opportunities, to add to the number of carefully conducted pathological inquiries; for, as physiological and pathological phenomena and laws mutually explain each other, the inquiry may be successfully carried on in both directions at the same time. At present we know nothing of the structural characters of the nervous centres after death from rabies, in the lower animals, though the disease is so common: we have not even ascertained the absence of any lesion capable of being detected by our present means of research. This is, therefore, a very important part of the inquiry; for, if a structural lesion causes the fearful symptoms of this disease, there cannot be a doubt that it is capable of being detected in the lower animals as readily as in man; and if the symptoms occur without a material change, this, too, may be ascertained with equal satisfaction by the same method. I may here remark, that I shall feel greatly obliged for any opportunities of examining animals recently dead of hydrophobia, which may be afforded me by those into whose possession they may happen to fall; and that, should such examinations accumulate and appear of sufficient importance, I shall not fail to lay the results before the profession. But it is not at all improbable that no

[1] Monthly Journal, vol. xi., p. 506.
[2] Ibid, vol. ix., p. 936.
[3] Ibid, vol. xii., p. 9.

structural lesion whatever exists in this and in many other nervous diseases, for there is much more than matter to be attended to, and every observable phenomenon is doubtless of the utmost importance. We have scarcely achieved more in the examination of purely vital actions than to ascertain the existence of a vital force, energy, or principle, which has hitherto eluded our grasp, and is yet most powerful in determining the action of various forms of matter. Matteucci and others have opened up a vast field of research in the observation of several of the most wonderful phenomena of organization, and have abundantly proved to us that there are yet many volumes of the book of nature, the perusal of which we have not commenced. It is, indeed, already shown that a peculiar force is manifested in animals in the physiological actions of the nervous and muscular systems, and that, though this force resembles electricity, yet it is perfectly distinct from it. How possible, then, does it appear, that an increased or perverted action of such a force upon the tissues of an animal body may give rise to the whole phenomena of alarming and destructive diseases, and yet leave their structure without a single evidence of material change?

ARTICLE II.—*Case of Strangulated Hernia.* By JOHN TRAILL, Esq., Surgeon to the Arbroath Infirmary.

WEDNESDAY, Jan. 1, 1851.—This morning, at five o'clock, I was requested by my friend, Mr Finlay, to visit David Shuld, a seaman, aged 60, who had been brought ashore from a ship, about four hours previously, with all the symptoms of strangulated hernia. Mr Finlay had seen him shortly after he reached his home, and had made a persevering attempt to reduce the hernia, but without effect. The man's statement—which was rather confused—was, that for many years he had laboured under hernia, for which he had worn a truss; but that the tumour frequently came down, and was returned on several occasions with difficulty, and after having caused symptoms resembling his present attack; that on Monday forenoon (December 30), feeling costive and unwell, he had taken a dose of castor oil, which had acted three times; that the same evening, about nine o'clock, whilst engaged in the ship's rigging, he had felt the tumour come down suddenly; that he immediately became sick, and vomited, and had been in great agony ever since.

He was evidently in great pain, his pulse small and wiry, the whole surface of the body cold and damp (he had not been able to change his wet clothes for two days and nights before coming ashore); but his countenance was not much sunk, nor his muscular strength much lessened. The belly was hard, tense, and tympanitic; but without much tenderness on pressure. He complained of intense sickness, and had frequent retching and vomiting of bilious-looking matter, with dragging pains about the navel, and constant thirst.

The hernial tumour occupied the right side of the scrotum, and was fully the size and about the shape of a *Bon Chretien* pear, of twelve ounces or a pound weight, having the testicle at its lower part, and feeling very tense, but with no great degree of tenderness.

We placed him under the full influence of chloroform, which produced complete relaxation of the muscular system, and the taxis was again tried. At first it seemed as if the tumour would be easily reduced, a considerable part

of its bulk apparently passing up, but immediately returning on the withdrawal of pressure.

He was now directed to be sent to the Infirmary, and placed in the warm bath as soon as possible.

About nine o'clock he was seen by Dr Bruce, who, after keeping him for about half an hour in the bath, again placed him under chloroform, and attempted reduction, without effect.

At ten o'clock, the symptoms continuing unmitigated, it was resolved, in consultation, to operate. On cutting through the integuments, the fascia and cellular substance were found much thickened, matted together, and infiltrated with serum, and at several points with air. The sac was strongly and uniformly adherent to the parts around, was very tough, and much thickened, being at no point less than a line, and at one part at least half an inch, in thickness. On making a small opening into it, a stream of yellow, turbid, and very fœtid fluid was thrown into the air with great force, and this continued to flow until at least ten or twelve ounces were discharged. On a careful examination I now became convinced that the sac was empty, at least of any solid body; and, on introducing a director through the opening, it passed without any obstruction through the inguinal canal into the abdominal cavity. From these circumstances, and the large quantity and nature of the fluid passed, some of my colleagues were of opinion that the part opened *might* prove to be intestine.

Although satisfied in my own mind, from the colour, density, toughness, great and unequal thickness, and uniform adhesion to the fascia, that the part opened must be the sac, yet, being also satisfied that it contained neither omentum nor intestine, it was agreed to place a ligature on the small opening, and, after closing the wound with sutures, to wait for some hours the result.

In the evening—no relief having resulted from the operation—the wound was opened up and the sac cut into, which, from its structure and smooth serous surface, was now evidently seen to be thickened peritoneum. The finger could now be passed without difficulty through the neck of the sac into the abdominal cavity, and there encountered what felt like a mass of omentum, and adhered slightly to the peritoneal surface.

The man died the same evening, about ten o'clock, after having vomited a large quantity of stercoracous matter, and without any abatement of his sufferings.

On opening the body next day, we found the appearances usually presented after death from hernia,—considerable effusion of turbid serum, the intestines agglutinated together by soft fibrinous matter, and the whole abdominal viscera indicating, in greater or less degree, the existence of inflammatory action. Immediately corresponding to, and lying loosely over, the internal orifice of the sac and inguinal canal, was a portion of the ileum, in a completely sphacelated state, about three inches in length, and its margin sharply and regularly defined, so as to indicate accurately the line of constriction. The serous surface of this part had lost its smoothness and elasticity, and the mucous membrane was soft and pulpy, and so much thickened as completely to close up the canal. Above this part, the intestine was, as usually seen, enormously dilated.

Remarks.—The opinion which I formed of this case at the time of the operation, from the phenomena which presented themselves during its progress, was, that the part strangulated must have passed up during some of the attempts at reduction; but that the morbid action was so far advanced as to have prevented any relief being derived from the removal of the strangulation. This view, I think, the post-mortem examination proves to have been correct.

It was also evident, from the appearances presented both during

the operation and on dissection, that the hernial sac had been irreducible for a long period, probably for many years; and, on minutely inquiring into this, we found that, although a strong circular truss had been constantly worn up to the commencement of the attack, the right side of the scrotum had always remained considerably enlarged.

The constant pressure of this strong truss over the unreduced sac will also account for its strong adhesions to the fascia, and for the congested and infiltrated state of the surrounding cellular tissues. The circumstance of our being able, during the attempts at reduction, to lessen considerably the bulk of the tumour, led probably to a longer delay of the operation than would otherwise have been the case.

The more I see of hernia, the more strongly am I convinced that all danger lies in delay; from the operation itself I have never seen the smallest danger result.

Notwithstanding this apparent reduction in bulk during pressure, yet the instant pressure was withdrawn, the tumour resumed its former size, and at the time of operation it was as large, and felt as tense, as it ever had been. This, and the persistence of all the symptoms, rendered it impossible to suspect reduction of the strangulated bowel.

ARBROATH, 5th January 1851.

ARTICLE III.—*On the Favourable Terminations of Pericarditis, and especially on Adhesion of the Pericardium, with cases illustrating its Secondary Effects on the Heart.* By W. T. GAIRDNER, M.D., Pathologist to the Royal Infirmary of Edinburgh.

THE permanent results of the effusion of fluid and lymph into the pericardium, in cases not fatal in the first instance, are now generally admitted to be the following:—

1st. The formation of limited thickenings, or opaque white patches, on the surface of the membrane, more or less smooth on the surface, and more or less firmly adherent to the serous tissue, from which they are sometimes indeed entirely inseparable.

2d. The formation of *partial* fibrous adhesions, commonly about the root of the great vessels and base of the heart, but occasionally nearer the apex, between the fibrous and vascular layer, or between the portions of the latter covering the aorta and pulmonary artery.

3d. *Universal* fibrous adhesion, such as to obliterate the serous cavity; the two surfaces being generally very closely applied to one another, but occasionally connected so as to allow of a limited amount of motion between them.

4th. Adhesion (which in this case is almost invariably universal), with marked thickening, induration, and sometimes calcareous transformation of the uniting medium. Under this head are comprised

most of the cases known in pathological works as ossification of the heart or pericardium, and a number of others in which that membrane is said to be thickened, rigid, or cartilaginous.

I shall not here detail the evidence which leads to the consideration of all these appearances as the results of different gradations of the same pathological process. This point has been fully discussed, and satisfactorily settled, by the various writers on disease of the heart since the time of Corvisart, and abundant details will be found on the subject in the greater works on morbid anatomy, in the treatises of Corvisart, Laennec, Bouillaud, Hope, Latham, and Watson, and in the instructive series of cases published by the late Dr Abercrombie, in the first volume of the "Transactions of the Medico-Chirurgical Society of Edinburgh." The point which remained longest unsettled was the origin of the thickenings or lymph-patches on the surface of the heart, which constitute the first form of permanent lesion to which I have referred; and the very general connection of this with the second form may be considered as established by the paper of Mr Paget in the twenty-third volume of the "Medico-Chirurgical Transactions of London"; so that there need now be little hesitation in considering these white patches, like most other thickenings of serous membranes, as the consequence of a morbid condition more or less allied to inflammation.

The question has, however, been raised, whether (admitting this pathological view to be correct) an inflammation of the pericardium of sufficiently marked and acute character to give rise to the known general and local symptoms of that disorder, can subside without leaving behind it obliteration of the serous cavity by adhesion over the greater part of its surface. Dr Latham, who seems to have taken a broader and more considerate survey of this subject than any recent systematic author, though giving a very cautious opinion,[1] obviously inclines throughout to the view, that the white patches, without considerable adhesion, are produced by morbid processes so slight as generally to escape attention altogether during life. On the other hand, my friend Dr Kirkes, of St Bartholomew's Hospital, has lately argued from the results of his dissections there, that "an acute and general attack of pericarditis" does not necessarily, nor in the majority of cases, terminate in adhesion; and that instances of this disease terminating in complete absorption of the products of inflammation, or in the formation of white patches without adhesion, must be of very common occurrence.

It is obvious that this is a question which can only be positively decided by very exact clinical histories of individuals presenting simple white lymph-patches in the pericardium, a kind of evidence somewhat difficult to procure, at least in hospital experience. But considerations derived from the morbid anatomy of the serous mem-

[1] Clinical Lectures, vol. ii., Lecture 23.

branes in general, the statement of which in detail would lead me too far from the principal subject of this paper, appear to me to render very improbable the complete disappearance of inflammatory lymph from a serous cavity which has been acutely inflamed, and on the *whole* surface of which the products of exudation have been deposited; and the special clinical and anatomical facts known in respect to pericarditis are, moreover, very favourable to Dr Latham's view. All the cases of acute pericarditis related by Abercrombie, Bouillaud, and others, in which death has occurred soon after complete disappearance of the fluid effusion, have demonstrated the termination of this disease in adhesion; whereas, had acute disease of this kind been the source of any considerable proportion of the chronic thickenings and partial adhesions which are found in so large a number of the bodies examined in hospitals (more than 50 per cent., according to Mr Paget), instances could not have been of rare occurrence in which the history and progress of the symptoms had been circumstantially detailed. Dr Kirkes, indeed, narrates what he considers to be such a case; it is one in which, in the course of acute rheumatism with endocarditis, a friction-murmur was "distinctly audible for four or five days, and then disappeared." Three months afterwards dissection showed a non-adherent pericardium, with "numerous vascular tufts and fringes of fine cellular tissue about the base, and several patches of equally fine and vascular tissue spread over the surface of the heart."[1]

Now, on carefully considering this most interesting case, I think it will appear that it scarcely warrants the inference drawn from it by the author. That a friction-sound over the pericardium may appear and disappear, lasting for periods from a few hours to several days, without being accompanied or followed by fluid effusion, dyspnœa, pain, fever, or indeed any one symptom, general or local, of distinct inflammation in that region, is an observation which I have had occasion more than once to make, in the course of pneumonia and other acute diseases, as well as in rheumatism; and which would doubtless be made much oftener were it not for the non-existence of symptoms to direct attention to the diseased part. Such an occurrence justly attracts the immediate attention and watchful care of the physician, especially in acute rheumatism, a disease in which pericarditis of marked and grave character is known to be frequent; but are we sure that the alarm given may not sometimes be a false alarm, and that the friction-sound may not in some, even in frequent cases, be the indication only of a more partial and evanescent morbid action than any to which the name of acute inflammation could with propriety be applied? It is highly probable that no abnormal deposit can take place upon the pericardium without establishing at one period or other the conditions of a friction-murmur; and yet it

[1] See the paper above-mentioned, in the "Medical Gazette" for 1849.

is very clear from morbid anatomy that such deposits must occur in numberless instances without severe or dangerous accompaniments, perhaps even without exciting the remark of either patient or physician. Under these circumstances the question arises, whether we are not in the habit of attaching too formidable a significance to the pericardial friction-murmur, considered simply by itself, apart from dull percussion and rational symptoms; and whether in this, as in some other cases in which the stethoscope has introduced us to a new realm of observation, we may not have been seduced into forsaking the old landmarks of disease before we have well established the new. In Dr Kirkes' case, for instance, every symptom and every sign noticed, with the single exception of the friction-murmur, may be accounted for without reference to the supposed acute and general pericarditis; and taking this circumstance in connection with the result of the post-mortem examination, it seems to me to indicate, not that the products of acute and general pericarditis may be rapidly absorbed, but that comparatively slight and innocuous exudations may be accompanied by a friction-murmur lasting for several days,—a doctrine which, as I have already said, there are both anatomical and clinical grounds for believing to be true, independently of this case. The complication of rheumatic endocarditis renders the symptoms difficult of analysis in Dr Kirkes' case; but for this very reason it cannot be adduced as a proof that *acute* and *general* pericarditis is consistent with a termination short of adhesion. On this point I must confess myself strongly disposed, in the meantime, to adhere to Dr Latham's opinion; while it is obviously a question of no small importance for future inquiry, how far the formation of the slighter forms of pericardial adhesions and thickenings is attended by symptoms entitling it to the name of inflammation?

Undoubtedly the most important question which has arisen in regard to the termination of pericarditis by adhesion, is that of the secondary influence of this condition on the structure and functions of the heart. It is to the examination of this question, and its illustration by a series of cases, that I propose to devote the remainder of this paper.

The opinions and statements of authors upon the effects of adherent pericardium are for the most part rather vague; nor do they appear to be entitled to much credit where they are more precise, if we consider their conflicting character, and the nature and amount of the evidence on which they are founded. A number of the older writers have narrated cases in which this lesion was connected with particular symptoms, and some have indicated opinions as to its diagnostic signs, placing reliance chiefly upon palpitations, smallness of the pulse, &c., which are now well known to be ordinary concomitants of many different cardiac affections. Morgagni, who alone of all these authors appears to have understood the true method of the inquiry, has collected from various sources forty-five cases of

adherent pericardium,—in regard to which he remarks, that mention is made of palpitations only in fifteen, and that in the greater number of these there were also other lesions of the circulating organs. In seven dissections particularly recorded by Morgagni himself, and in three others by Valsalva, there was in one only notice of an intermitting pulse and palpitation; yet one of the men is stated to have been, "a little before his death, equal to the laborious business of fishing."[1] The greater part of these dissections are regarded by Morgagni as useless in relation to the determination of symptoms, either on account of imperfection in the histories or from the coexistence of other important lesions with the pericardial adhesions. He also concludes that none of the alleged pathognomonic symptoms can be considered as in any degree constant; while some cases appear to prove the occurrence of the lesion in question without any one of them. Add to this, that it is impossible to gather from Morgagni's collected cases sufficiently exact details as to the existence or non-existence of hypertrophy of the heart, and it will be apparent that nothing more than very vague and negative information can be derived from them.

Lieutaud has made a large collection of cases of adherent pericardium, in many or most of which there were cardiac symptoms; but the records are throughout so imperfect that they are only worthy of notice as a specimen of industrious compilation.

Corvisart[2] admits that all the signs given as pathognomonic of this affection have been invalidated by experience, but thinks that their union would in some cases lead to a true diagnosis. He repudiates the idea that either a firmly and closely adherent pericardium, or close adhesions of the pleuræ, are consistent with sound and healthy life; and he gives, as an example bearing upon this opinion, a case in which pain at the epigastrium, difficulty of breathing, smallness of the pulse, and irregularity of the heart's action, were connected with the former affection, and where the diagnosis was formed by him during life. In this case, however, there was a large pleuritic effusion on the left side, which had not been discovered during life; there was also abdominal dropsy, and "the peritoneum was covered with granulations;" the heart was, moreover, enlarged and dilated, and although a general statement is made as to the normal condition of the organ in other respects, neither the valves nor the great bloodvessels are particularly mentioned. As we have no means, therefore, of knowing which affections were primary and which secondary in this instance, and as the hypertrophy of the heart may have been owing to other causes, I think that accurate reasoning almost demands that the case be excluded from consideration, or at least accepted with many reservations.

Laennec opposes himself entirely to Corvisart's conclusions. He

[1] De Sedibus et Causis Morborum, Epist. xxiii.
[2] Maladies du Cœur, Ch. i., art. 2.

says:—"I can positively state, that I have opened a great number of subjects, who had never complained of any disturbance in the respiration or circulation, and who had never presented any sign of it in their fatal malady, although there was intimate and total adhesion of the lungs or heart; and, as regards this last organ in particular, I am much disposed to believe, from the number of cases of this kind which I have met with, that adhesion of the heart to the pericardium often takes place without disturbance of its functions. It has appeared to me only, that the contraction of the auricles becomes often more obscure when they are adherent to the fibrous pericardium."[1] Laennec has not given any cases illustrative of these statements; the only instance of adherent pericardium in his work being one which must be placed altogether out of the category of simple adhesions, the membrane being so much altered as to form an osseous and cartilaginous case, inclosing the heart. Of this case, of course, no use can be made in relation to the present subject.

In the work of Bertin there is only one vague statement of opinion upon the point in question, which seems intended to include both the alternatives offered by Laennec and Corvisart.[2] Bertin adduces, however, one case (Obs. 68) of adhesion of the pericardium to the heart, apparently uncomplicated, in which there existed palpitations, obstructed respiration, with orthopnœa, cough, and bronchitic expectoration, œdema of the limbs, lividity, dulness (increased?) of the precordial region, and irregular intermittent action of the heart, which was found after death "very soft, a little thickened, and perhaps a little dilated."[3] Had the dissection in this case been detailed with ordinary care, and in particular had the state of the valves been mentioned, it might have proved an important one; but, from the omission of every important particular, it is impossible to say whether there was, or was not, any concomitant cause of all these symptoms, and of the hypertrophy of the heart which accompanied them.

Louis, in his "Mémoires Anatomico-Pathologiques" (p. 299), incidentally mentions seven cases of simply adherent pericardium which had fallen under his notice. Of these, three had entered the hospital too late in the disease to be carefully examined during life; and he says that "the detailed history of the other four offers us no phenomenon *which we could not attribute to their principal diseases*, phthisis, aneurism of the heart," &c. Without a more full statement of these principal diseases, it is obvious that M. Louis's cases cannot be of any use in the settlement of the question, whether secondary diseases of the heart, attended by symptoms, are produced by adherent pericardium in a large proportion of cases.

Bouillaud has recorded, in his treatise on diseases of the heart, a considerable number of cases of pericarditis, and among them thir-

---

[1] De l'Auscultation Mediate Part. 4, Sect. 2, Ch. 20.
[2] Maladies du Cœur, &c., p. 271. [3] Op. cit. p. 254.

teen in which a perfect cure is said to have taken place; but the early dismissal of these cases from the hospital, and the absence of any subsequent history, leaves it quite doubtful whether hypertrophy and other chronic lesions of the heart, with all their symptomatic consequences, may not have supervened at a future period. Nor can it be regarded as certain that in all these cases adhesions had actually place, at least to such an extent as to be comparable with those the effects of which we are considering. Amid the fatal cases recorded by Bouillaud, I have looked in vain for a case of uncomplicated pericardial adhesions,—*i. e.*, one in which the valves and great vessels were sound. In one case, indeed, in which the state of these parts is not mentioned, there was firm adhesion of the posterior surface of the heart to the pericardium, along with the effects of a recent pericarditis.[1] It is difficult, however, to tell in this instance what proportion of the symptoms were due to the adhesion, and what to the more recent attack; but hypertrophy of the heart to a great extent was found after death. The case must be regarded, therefore, as imperfect in relation to the present subject, from the want of precise information as to the presence or absence of other possible sources of hypertrophy, and particularly disease of the valves.

I can find no cases in Bouillaud's work bearing on his assertion, that "adhesions of the pericardium are found in persons enjoying the most flourishing health;" nor does he himself say that he has seen such cases. The assertion, therefore, is probably a mere repetition of Laennec's, in stronger terms than that writer ventured to use.

Of the more recent writers of authority on diseases of the heart, none has spoken so confidently with respect to the subject now under consideration as Dr Hope. It is, therefore, necessary to examine a little in detail the grounds of his assured convictions, which are stated in these words:—"Though close adhesion may not, for a time, create *much* inconvenience, its effects are ultimately fatal, especially in the working classes. A tranquil abstemious life, by which, in other forms of organic diseases of the heart, existence may sometimes be prolonged to its natural period, may do much, but cannot be *equally* availing here; for, as the action of the organ itself is a constant struggle, repose is impossible."[2] The opinion of Dr Hope, therefore, amounts to nothing less than that adherent pericardium is the most rapidly and inevitably destructive of all organic diseases of the heart.

The grounds of this inference are stated by Dr Hope as twofold:—Firstly, that he had never seen an individual with completely adherent pericardium who had not "more or less palpitation and

---

[1] Maladies du Cœur, vol. i., p. 363.

[2] Diseases of the Heart, &c. Fourth edit., pp. 181, 182. The italics are the author's.

hurried respiration on exertion;" and secondly, that he had "never examined, after death, a case of complete adhesion of the pericardium, without finding enlargement of the heart, generally hypertrophy with dilatation." The latter of these statements is very greatly at variance with the remark to which I have already adverted, of Laennec, who can scarcely be supposed to have overlooked so manifest a lesion as hypertrophy and dilatation of the heart, when stating that in *many cases* of adherent pericardium he found no evidence of disturbance of the functions of that organ. This discrepancy is so striking as to be quite irreconcilable, without supposing one or both of the statements to proceed from a limited experience, or an erroneous generalisation. Facts to be presently mentioned will tend to show on which side the error lies.

With respect to Dr Hope's other statement of his experience in adhesion of the pericardium, that there existed invariably "*more or less* palpitation and hurried respiration *on exertion*," it might well be objected to on the score of vagueness; as, in all probability, such a criterion could not be applied even to healthy individuals of sedentary habits (much less to hospital patients), without including the great majority of them. But, assuming something distinctly morbid to have been intended by the expression in question, this statement cannot be regarded as singly lending any support to the doctrine of the irresistible and rapid fatality of adherent pericardium, or indeed as meaning more than that persons having this peculiarity are somewhat more short-breathed, and less able for active exertion, than others. The main evidence, therefore, of the invariably serious consequences ascribed to adherent pericardium by Dr Hope, must be regarded as depending upon the frequency of its coincidence with hypertrophy of the heart.

It may serve to indicate the probable source of the discrepancies above noticed to observe here, that *all* the cases of adherent pericardium (five in number) related in Dr Hope's work are instances of complex cardiac disease, in which the hypertrophy and dilatation find ample explanation, independently of the pericardial lesion. Thus, in two of the cases (Harrison, May) there was aortic obstruction with regurgitation; in the other three (Payne, Williams, Jones), extensive disease of both aortic and mitral valves. It may be not unfair to presume that similar cases formed the staple of Dr Hope's experience, if we recollect that in adducing the evidence of his much-vaunted "pathognomonic signs" of adherent pericardium, he would naturally have attached importance to uncomplicated instances of the affection, had they fallen within his sphere of observation. The absence of such cases from his book, considered in connection with the previous experience of Laennec, and with the facts to be hereafter adduced, would appear to render it highly probable that Dr Hope had never looked for adherent pericardium, except in cases where there were obvious cardiac symptoms; and that he found hypertrophy and dilatation to be invariable concomitants of

adherent pericardium, simply because cardiac symptoms are rarely present, except in cases complicated with valvular disease. This view of the case would effectually reconcile all the conflicting evidence on this subject, as a somewhat similar explanation will in all probability apply to the cases and opinions of most of the older authors, and especially of Corvisart. Indeed, all morbid anatomists are well aware of the tendency of post-mortem examinations, when pursued with a view merely to the elucidation of particular symptoms, to lead the observer into a vicious circle of error. The symptoms being predetermined, the problem is to find a morbid appearance which will account for them, and when this has been discovered, or imagined, the inquiry is not pursued further. Thus, the whole class of lesions which are *negative* as regards symptoms, but which more than any others illustrate the healing powers of nature and art, habitually escape observation.

As a further proof that Dr Hope had not given to the evidence on this subject that impartial consideration which the high reputation of his work, and the confident expression of his own opinion, demanded, I may mention that he has fallen into two very grave errors in stating the doctrine of his predecessors in the inquiry. In the first place, he claims the opinion of "Lancisi, Vieussens, Meckel, Senac, Corvisart, and, *more strongly than all*, Morgagni," as favourable to the view of the incompatibility of health with pericardial adhesion; whereas we have seen that Morgagni's very careful induction from the facts before him, though by no means decisive, or expressed with undue confidence, has evidently led to a precisely opposite bias in his own mind. In the second place, Dr Hope takes a most inexcusably erroneous view of the statements of Laennec, when he attacks him as founding his opinion solely on "the absence of complaint on the part of the patient." We have seen that Laennec expressly mentions, and puts prominently forward as one of the grounds of his opinion, the absence of *signs* (*i. e.*, *physical* signs) of disturbed cardiac function in the fatal illness; and as if to guard this observation from being lightly passed over as a random assertion, he adds the only qualification of it which his experience suggested, in the form of a remark on the contraction of the auricles. Yet this very precise and guarded statement is entirely omitted by Dr Hope, to be superseded by his own counter-assertion, equally unsupported by details of evidence, and in all probability founded on far less exact observation.[1]

The opinion of Dr Latham, as to the ultimate consequences of

---

[1] The consideration of these inaccuracies, and of the facts to be hereafter adduced, will tend to diminish the mortification of those who, having attempted to apply Dr Hope's "pathognomonic signs" of adherent pericardium in practice, have had their faith in their own powers of observation shaken, by not finding themselves able to detect the affection "with much ease," "in the great majority of cases."—Op. cit., p. 183.

adhesion of the pericardium, is given in a single passage in his "Lectures on Clinical Medicine."[1] "It is a question with me after all, what are the consequences which naturally result to the functions and structure of the heart from simple adhesion of the pericardium? For I have not facts enough to appeal to of the sort which are required to settle it." This free admission of an "open question," in reference to this subject, will not, in all probability, be regarded by the readers of the preceding pages as unduly guarded, or as anything else but the true and honest expression of the sum of our present information. It is well, however, to remark, that Dr Latham's personal experience has revealed to him "a few cases" in which, as in those of Laennec and Louis, complete adhesion of the pericardium was unattended, during the fatal illness, by symptoms referable to the heart; and in which, morever, there was neither hypertrophy nor dilatation of that organ.[2] He has also not failed to observe the termination of acute pericarditis in apparently perfect recovery, and has seen such cases at the date of several months after the attack, with unimpaired general health and local function; but he admits the doubt as to whether these were actually cases of *adherent* pericardium; and considers that the want of the evidence of dissection in this class of cases, and of a sufficient history in the other, together with the casual character of both, preclude the formation of a decided judgment from them.[3] On the other hand, the evidence, from examination after death, of the dependence of hypertrophy of the heart upon adherent pericardium, is vitiated, according to his experience, by the almost constant concurrence of valvular lesions with pericardial adhesions attended by hypertrophy.[4] In this respect the experience of Dr Latham concurs with that of Bouillaud, and with the *recorded* cases, at least, of Dr Hope.

The great importance of this subject, in reference to the prognosis in cases of recovery from pericarditis, and the manifest deficiency of facts calculated to contribute to the formation of a definite opinion upon it, have led me to believe that a not unacceptable contribution to the natural history of pericarditis might be framed by collecting the *entire* experience as to the permanent results of adherent pericardium, from the records of upwards of 500 *post-mortem* examinations, performed by myself, or by others under my superintendence, during the last two years. With this view, I have prepared the following abstract, in which appear, to the best of my knowledge, *all* the cases of considerably adherent pericardium which have been examined in the Royal Infirmary during the period referred to. Though some of these cases unavoidably, and others through unfortunate neglect on my own part, are deficient in certain particulars bearing on the question, and few of them have any pretensions to completeness as individual histories of disease, they will be found to have a collective

[1] Vol. ii., p. 111, Lecture 23d.
[2] Op. cit., p. 101.
[3] Op. cit., pp. 101, 102.
[4] Op. cit., p. 111.

value, arising from the circumstance of their being drawn *indiscriminately* from the examination of cases of mixed disease, occurring in the wards of a large hospital. And if their actual number be considered too small to warrant the formation of a very decided and definite opinion, they may at least direct the attention of others, similarly situated with myself, to the subject, in such a way as will elicit, ere long, more precise and numerous data.

The cases which I have to adduce are fifteen in number. Many of these are instances of complete and close adhesion, entirely obliterating the cavity; in others, the adhesions were partial, but always so situated as that they may be conceived to have restrained, to a considerable degree, the motion of the heart within its serous cavity. On the other hand, all instances of adhesion near the base, even though firm and close, and of very lax and mobile adhesions, wherever situated, have been omitted as open to objection.

As the cases appear to add comparatively little to our knowledge of the clinical department of the subject, and as the negative evidence which they afford would not be a satisfactory ground of classification, they are here arranged chiefly with a view to bring out prominently the existence or non-existence of hypertrophy of the heart, the presence of which condition may, I think, justly be considered as a sure and delicate test of disturbance or impediment to the function of the organ. In proof of this, I may refer to the almost invariable occurrence of hypertrophy, or hypertrophy with dilatation of the cavities, in conjunction with valvular disease, as well as to the hypertrophy of the right ventricle, which usually supervenes in emphysema of the lungs, and that of the left side in aortic aneurism. Indeed, there is no fact in cardiac pathology more clearly established than the relation of hypertrophy of the heart to mechanical impediment of its function. I shall, therefore, arrange the cases in three groups, the first comprising those in which there was *not* hypertrophy of the heart to any appreciable degree; the remaining two, those in which hypertrophy existed, with or without other collateral lesions.

First Series.—*Cases of Adherent Pericardium, in which the Heart was not enlarged or otherwise diseased.*

Case I.—(P. R., Vol. xii., 1[1]).—A female, æt. 46, died exhausted and much emaciated, with symptoms of scirrhus of the pylorus. There were no symptoms referable to the heart, which does not appear to have been examined.

There was a cancerous ulcer of the pyloric end of the stomach, and cancer of the lumbar glands. The pericardium contained about four ounces of clear serum; and there was an old adhesion between the anterior part of the left ventricle and the mediastinal pericardium. The heart was very small, and all its apertures small, but not diseased. Aorta relatively large. No disease of the other thoracic viscera.

---

[1] The references in brackets are to the Pathological Register of the Royal Infirmary. The figure indicates the number of the case in the volume indicated.

CASE II.—(P. R., Vol. xii., 2).—A male, æt. 37, died of diabetes mellitus. No symptoms referable to the heart, though he was long under observation.

Pericardium adherent in several places over the left ventricle. Several chronic thickenings of the membrane on the surface of the right ventricle. Heart and lungs normal.

CASE III.—(P. R., Vol. xii., 49).—A male, æt. 30, had the ordinary symptoms of incipient pulmonary phthisis, succeeded by obstinate diarrhœa. The chest was physically examined, but no note is made of any cardiac affection. The urine was albuminous, and of specific gravity 1005. He died exhausted.

The dissection revealed miliary and opaque tubercle of the lung, with tubercle of the ileum, and ulceration of the colon. The kidneys were in an advanced state of granular disease. The pericardium was adherent by firm fibrous bands (admitting, however, of some motion) everywhere, except at the apex of the heart, and on the anterior surface of the aorta. No abnormal condition of the heart mentioned.

CASE IV.—(P. R., Vol. xii., 132).—A male, æt. 45, said to have enjoyed good health till the supervention of hepatic symptoms, two months before his admission into hospital. He suffered from ascites. There is in the account of his case a single note, of a murmur with the first sound of the heart, *heard at the apex*. [As there do not appear to have been any cardiac symptoms, and as the post-mortem examination does not bear out this statement, it is very probable that this is an error. The murmur may have been anemic, or possibly a roughness connected with some slight motion in the adhesions.]

On dissection there was cirrhosis of the liver, accompanied by enlarged spleen. The peritoneum full of fluid. The pericardium firmly and closely adherent. The heart not enlarged.

CASE V.—(P. R., vol. xii., 255.)—An adult male, age not stated. Was admitted for pneumonia, from which he recovered; but during the convalescence took cholera, and died.

The body was emaciated and anemic; there were sloughs on the nates. The lungs presented scattered traces of disease. The pericardium universally and firmly adherent. Heart healthy.

CASE VI.—(P. R., vol. xii., 291.)—An adult male, age not stated. No case. The principal disease found, on examination, was in the urinary organs. There were abscesses in the kidneys, with dilatation of the ureters and bladder, from enlargement of the prostate gland. The pericardium was firmly and closely adherent.

CASE VII.—(P. R., vol. xii., 335.)—A female, æt. 63. No history. The lungs were tubercular, and there was also tubercular disease of the peritoneum. The pericardium was firmly adherent.

CASE VIII.—(P. R., vol. xii., 337.)—A male, aged 45. Died exhausted from malignant disease. Dissection showed cancer of the liver and stomach, and also of the cervical and anterior mediastinal lymphatic glands. The pericardium was closely, firmly, and universally adherent.

CASE IX.—(P. R., vol. xii., 376.)—A male, aged 30, affected with chronic phthisis. Heart's sounds stated to be natural. Died exhausted.

On dissection, the lungs were found tuberculous, and the intestines ulcerated. The heart, firmly adherent all round to the pericardium, weighed, together with the adherent membrane, and the great vessels cut outside the pericardium, only eight and a-half ounces.

CASE X.—(P. R., vol. xii., 385.)—A female, aged 19, affected with syphilitic rupia, died in a state of extreme exhaustion and emaciation. Was long under observation, but presented no symptom referable to the heart.

The liver, kidneys, and lungs were more or less diseased. The whole body extremely emaciated. There was a firm, though rather lax, adhesion between the pericardium and the heart on the right side near the apex. Heart healthy.

The critical reader will possibly object to this series of cases, that in some of them the condition of the heart as to hypertrophy is not specially reported; and that details of weight and measurement are not given in the greater number. But to weigh a heart with the pericardium adherent to it, and two inches of the great vessels attached, would have been to furnish a datum of no practical value, as it could not have been compared with any standard; and to separate the heart from the fibrous pericardium and the adherent cellular tissue for the purpose of weighing it, would not only have been an unduly troublesome and uncertain process, but would have destroyed several preparations which it was desirable to preserve. I can state, however, positively with reference to these cases, that they were all rather under than above the usual average of size (considered with reference to the individual), with the exception of one, which I have preserved as the largest of the series, and in reference to which, perhaps, some degree of hypertrophy may be admitted. This heart, with its adherent pericardium (which is somewhat thickened), and the additional length of great vessels, weighs at present (being preserved in chloride of zinc solution) sixteen and a-half ounces. I have found that the healthy pericardium of a heart weighing nine and a-half ounces, when separated along with the additional length of great vessels, amounted to about four and a-half ounces in weight; so that, making due allowance for the thickening of the membrane in the present instance, we can scarcely suppose this heart to weigh much more than eleven or eleven and a-half ounces—an amount which may have been large relatively to the individual, but can scarcely be considered to constitute at all a marked specimen of hypertrophy.[1] The organ appears, however, to the eye increased in size. I regret that, owing to a note being mislaid, I am unable to state which of the above cases furnishes this doubtful example. On the other hand, several of the cases presented decidedly small hearts, particularly Cases I. and IX. In the latter the organ was preserved as the smallest of the series. It weighed, when recent, as stated in the report, only eight and a-half ounces, with the adherent pericardium, &c.; and, allowing not more than two and a-half ounces for these appendages, the reduction of size must be considered, for an adult, as falling within the limits of atrophy.

These cases appear amply sufficient to destroy the force of Dr Hope's assertion, as to the invariable connection of hypertrophy with adherent pericardium; while, on the other hand, in so far as they are accompanied by clinical details, they bear out Laennec's statement of his own experience. I reserve for the sequel the question, whether they justify his view of the innocuous character of this lesion?

---

[1] Since the above was put in type, I find that this statement must be modified, as the loss of weight from preservation in chloride of zinc is more considerable than I had supposed. It is probable, therefore, that this heart was in the early stage of hypertrophy.

SECOND SERIES.—*Cases of Adherent Pericardium, in which the Heart was enlarged, but with considerable valvular or other collateral disease tending to produce Hypertrophy.*

CASE XI.—(P. R., vol. xii., 110.)—A male, aged 55. Admitted February 25, 1849, with marked hemiplegia of right side of face and right limbs, the attack having occurred the preceding day. Had been subject to colds, with cough and palpitation, for a year; previously healthy, but intemperate. The heart was examined, and the following signs found: cardiac dulness not increased; apex pulsates between sixth and seventh ribs, at a point equi-distant from nipple and sternum; action of heart very irregular; sounds loud and flapping, can scarcely be distinguished from each other. Many pulsations of the heart lost at the wrist. Two days before death the action of the heart is noted as "jogging." Sounds confused. He died 15th March, exhausted and comatose.[1]

The pericardium was universally closely and firmly adherent. There was a calcareous concretion imbedded in the adhesions towards the base of the heart. The arch of the aorta was much dilated and scabrous from atheromatous and earthy deposit; at one part the dilatation almost constituted a sacculated aneurism. The aortic valves were slightly incompetent. The heart hypertrophied. The lungs and kidneys were diseased.

CASE XII.—(P. R., vol. xii., 379.)—A male, æt. 30, was admitted into the hospital, July 16, 1850, labouring under general anasarca, of four months' standing. He was very intemperate in his habits, but without serious illness till six months before admission, when he had an inflammatory affection of the chest. There were signs of hydrothorax. The urine albuminous, sp. gravity 1005. The following signs are noted in relation to the heart:—Precordial dulness extends to the right of the mesial line; first sound accompanied by a clear and musical bellows murmur at apex; at base, second sound accompanied by a murmur. He died July 22.[2]

Dissection showed the heart greatly hypertrophied; the mitral orifice extremely contracted; the aortic valves incompetent; the pericardium adherent throughout. There was also hydrothorax.

The chief remark which appears to be demanded by these two cases is, that the amount and kind of the concurrent disease of the heart and great vessels is such as appears to preclude the idea of the adherent pericardium having had the most considerable share in its production. Dilatation of the aorta, and incompetent aortic valves, in the one case; extreme mitral contraction, and aortic regurgitation, in the other, abundantly account for the secondary organic derangement of the heart, as well as for the symptoms. In Case XI., it will be observed that the apex was displaced; instead of beating external to the nipple, as might have been expected in so large a heart, it was nearer the sternum, though lower than usual. As there was no pleuritic effusion, this can probably only be accounted for by the restraint to which the organ was subjected in its increase of size from the closeness of the adhesions; and while the case, therefore, shows that Dr Hope's first sign of adherent pericardium (the apex beating unusually high up in the chest) is not applicable to all cases

[1] Case-book of Ward No. 6.
[2] Case-book of Ward No. 4.

accompanied by hypertrophy, it seems to indicate the propriety of keeping in view the displacement of the apex in any manner not accounted for by ordinary circumstances, as one of the elements of a diagnosis. In this case it is also observable, that the *jogging* motion of the heart, mentioned by Dr Hope as a sign of adherent pericardium, appears to have been present, at least shortly before death.

THIRD SERIES.—*Cases of Adherent Pericardium, in which the Heart was enlarged, without valvular or other collateral disease apparently sufficient to account for the enlargement.*

CASE XIII.—(P. R., vol. xii., 270.)—A man, aged about 30, admitted *in extremis*. "Heart's impulse strong; no morbid sound."

On dissection, the heart hypertrophied, especially the left ventricle. The lungs very emphysematous.

[This case was examined in my absence, and is exceedingly defective in its details. The state of the valves is not mentioned; but the note as to the sounds makes it *probable* that the case is correctly placed in this series.]

CASE XIV.—(P. R., vol. xii., 375.)—A man, æt. 48, admitted into hospital, May 13, 1850, on account of a recent hemiplegia of left side of body. Affected likewise with anasarca, dyspnœa, and palpitations. Urine albuminous, sp. gravity 1009. He had been bled largely seven years before, for an inflammation in the chest. Had afterwards, in the course of two years, two distinct attacks of cerebral symptoms,—viz., fulness in the head, with diminished vision, and deafness; these, however, disappeared after cupping. A year before admission, cough, dyspnœa, pain at epigastrium; repeated hæmoptysis. He was dismissed with the paralysis cured, but the other symptoms only relieved, on the 20th May. Readmitted, July 8, on account of increased dyspnœa. On July 20, seized with the first of a series of convulsive attacks, with tendency to coma, of which he died on the 22d. The cardiac signs were noted at both periods of admission. The apex beat between fifth and sixth ribs, a little to the left of the nipple; dulness of precordial region extended; considerable impulse of the heart when in erect posture; strong arterial pulsation of great vessels; no bruit, but second sound deficient in sharpness (according to second report, accompanied by a peculiar roughness). Pulse 84, natural strength.[1]

The morbid appearances were,—general anasarca; softening of both corpora striata; disease of kidneys; hemorrhagic condensation of both lungs. The heart weighed twenty-nine ounces; its muscular fibre had to some extent undergone both the granular and the fibrous degeneration. The organ was adherent to the pericardium at one point on the left ventricle (anteriorly), about two inches from the apex; and opposite to this point was an isolated adhesion of the pleuræ to the lung. The pulmonary artery was somewhat dilated, and its valves slightly incompetent. The aortic valves were competent; the others were examined, but nothing abnormal reported. Thoracic aorta normal.

CASE XV.—(P. R., vol. xiii., 14.)—A very tall robust woman, aged apparently about 50, not at all emaciated. Died a few hours after her admission into hospital, in a state of great exhaustion from dyspnœa. No history was obtained, nor was the heart carefully examined.

On dissection, the pericardium was firmly and closely adherent at two places,—viz., over the anterior surface of the right ventricle, near the base, and at

[1] Case-book of Ward No. 1.

the posterior surface of the apex of the left ventricle. There was also a firm patch of rough lymph on the anterior side, near the apex. With these exceptions, the greater part of the pericardium was healthy. The heart was greatly hypertrophied, weighing, with the pericardium, which was not thickened, thirty ounces. (The great vessels were cut *within* the pericardium.) All the cavities were dilated, and the auriculo-ventricular orifices appeared slightly enlarged; there was, however, no disease of their valves, which seemed tolerably adequate to close the openings. The aortic and pulmonary valves closed perfectly, so as to prevent regurgitation. The arch of the aorta presented a little opaque deposit in the inner coat, but was not otherwise altered. The lungs presented hemorrhagic condensation; the liver was enlarged and tuberculated; the spleen abnormally firm.

The three cases here related have not equal claims to consideration, as instances of secondary disease of the heart from adherent pericardium. The deficient details of Case XIII. only permit it to be placed in this series on the ground of probability; and in Case XIV. it may not unreasonably be objected, that the degeneration of the fibres of the heart aided and determined, if it did not exclusively cause, the enlargement. I am not, however, disposed to take this view, inasmuch as hypertrophy, in connection solely with degeneration of the muscular fibre, without valvular or any other lesion of the heart or great vessels, is far from common; while, on the other hand, degeneration of the fibre, without hypertrophy, or with hypertrophy determined by valvular lesion, is of extreme frequency. The incompetency of the tricuspid valve was too trifling to have had much influence on the heart, and was probably secondary. On the whole, therefore, I believe that this case may be, without hesitation, placed in the present series, although it is possible that the condition of the fibres of the heart may have been a secondary cause of its dilatation, as seems not unfrequently to be the case in the latter stages of valvular lesions. At all events, in Case XV., there is an unexceptionable instance of adherent pericardium, accompanied by hypertrophy and dilatation of the heart, apart from any other circumstance which appears likely to have caused that affection.

The physical signs in Case XIV. were simply those produced by the hypertrophied and dilated state of the heart. The position of the beat of the apex, however, was higher and less removed from the sternum than was perhaps to be expected in the case of a heart so considerably enlarged; although it is doubtful whether a distinct opinion on this point could have been formed during life.

I shall now sum up, as shortly as possible, the inferences which appear to be deducible from the preceding cases, taking these in the order in which they most naturally present themselves.

1. It seems probable that a *considerable* majority of the cases of adherent pericardium occurring among the patients of a general hospital, and a *very large* majority of such cases, when not primarily connected with endocardial or arterial disease, is unattended by enlargement of the heart, or by any prominent symptom of cardiac

disorder. In the instances under examination, the proportion of cases *with*, to cases *without*, hypertrophy, was 10 to 5, endocardial disease being included; and 10 to 3, endocardial disease being excluded.

2. It is clear, from these facts, that the tendency of adherent pericardium to produce hypertrophy of the heart is very much less than that of valvular or endocardial lesions. In 19 cases which I have lately examined of disease of the orifices sufficient to produce decided obstruction or regurgitation, I can find only 1 in which hypertrophy of the heart is not mentioned; in it the mitral orifice was "slightly contracted;" and it is not certain that a small amount of relative hypertrophy may not have existed. In the 13 cases of uncomplicated adherent pericardium, on the other hand, there were 10, or at least 9, in which not the slightest hypertrophy was present; and in 2 or 3 the heart was remarkably small.

3. It is not, however, a legitimate inference from this statement, that adhesion of the pericardium has little tendency to produce hypertrophy, or that it has as few deleterious consequences as Laennec ascribed to it. The greater number of hospital deaths, apart from those arising from fevers, acute diseases, and organic affections of the heart and great vessels, are among individuals who have long suffered from emaciating diseases, and are accordingly, from bloodlessness and diminished bodily activity, in the most favourable condition for *latency* of a cardiac affection. In fact, the 10 cases of adherent pericardium without hypertrophy afford the following results:—

Chronic tubercular phthisis,	3
Malignant disease,	2
Diabetes mellitus,	1
Prostatic disease, &c., probably in an old person,	1
Syphilis, with extreme cachexia and emaciation,	1
Pneumonia, followed by cholera, probably in a cachectic, and certainly in a highly emaciated, subject,	1
Ascites, from diseased liver and spleen,	1

In all of these cases, with the exception of the last, the attendant circumstances were precisely those which appear calculated to restrain and mask a cardiac affection, even if of such a kind as to produce inevitably deleterious consequences in a healthy individual. In the exceptional case of cirrhosis of the liver with ascites, this symptom had existed only two months; and there is no record of the bodily condition of the patient previously to this period. It would be, therefore, a very imprudent conclusion from these cases, to consider adhesion of the pericardium as consistent with the healthy and unrestrained activity of the body, or even with the due performance of the functions of the heart in all cases of disease.

4. But there is, moreover, strong evidence in the cases recorded, that in some circumstances at least adhesion of the pericardium, originally uncomplicated, may lead to very great hypertrophy and dilatation of the heart. These cases form, it is true, a minority in the hospital

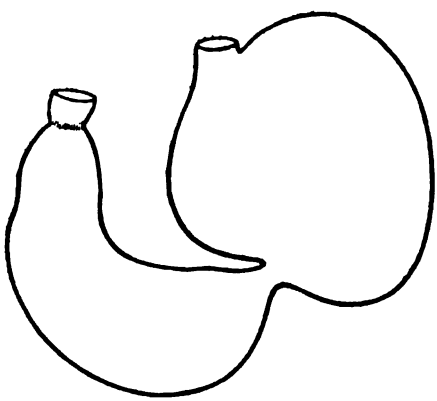
results, but they are not on that account to be disregarded. Of the 3 cases of this kind recorded, there is at least 1 in which no other conceivable cause for hypertrophy existed; and 2 others, in which there was more or less strong probability that the adhesion was the original source of the cardiac disorder. It is important to notice, that in these 3 cases there was no evidence of the co-existence of any other chronic diseases, except those which are common as the secondary effects of organic derangements of the heart.

5. If the preceding inferences be admitted, we have to reconcile two apparently contradictory facts: *firstly*, that adherent pericardium, at first uncomplicated, may, in certain cases, result in extreme hypertrophy of the heart; and, *secondly*, that in other cases it may not only fail to produce this effect, but may appear altogether powerless in opposing the atrophy of the heart that results from chronic disease.—(See especially Case IX., also Case I.) The only view which seems to harmonise these conditions, is the supposition, that the free motion of the heart within the pericardium is required in health, not so much to meet the necessities of the circulation in its tranquil and ordinary condition, as to provide for the contingency of excited action, and to give abundant scope for the smooth and painless motion of the heart under those circumstances in which the habitual equilibrium of the circulation is disturbed. Such circumstances are of daily occurrence; in the healthy and vigorous, from superabundant use of bodily exertion; in the sick and debilitated, from the more sparing use of it; in all, but especially in the nervous and excitable, from mental emotion, and a variety of minor causes. These temporary excitements are, however, to a great extent controllable; and on this fact depends, I believe, the practical application of these principles to the management of adherent pericardium, where it is known or suspected to exist. An individual so affected cannot, in all probability, afford to live according to the measure of his other faculties and functions; he must adopt a reduced scale of existence, and restrain himself from every considerable indulgence, either of his activity, his appetites, or his emotions; but in following out this prescription, he may in all probability be assured, that, unlike the subject of any other organic heart disease, he has his enemy under a control all but absolute.

6. It does not appear, from the preceding cases, that hypertrophy is less apt to take place when the adhesions are partial, than in the case of universal obliteration of the cavity; provided only that, in the former case, the seat of the adhesion be not far removed from the apex—the point of greatest motion. In fact, it will have been observed that, in two of the three cases of hypertrophy without endocardial disease, the adhesions were exceedingly partial, though close and firm; and, on the other hand, in six of the ten cases of adhesion without hypertrophy, the serous cavity was entirely obliterated. It has not, therefore, seemed necessary to make any modification of the conclusions on this account.

ARTICLE IV.—*Cases of Double Stomach, with Observations.* By JOHN STRUTHERS, Fellow of the Royal College of Surgeons, and Lecturer on Anatomy, Edinburgh.

ON opening the abdominal cavity of a middle-aged female subject, at lecture during the winter session 1847-48, my attention was attracted by an unusual appearance of the stomach. It seemed contracted in the middle, and on distension it became evident that the stomach consisted of two sacs, connected together by a narrow communication. The accompanying woodcut will convey a better idea of its form than could be done by detailed description.



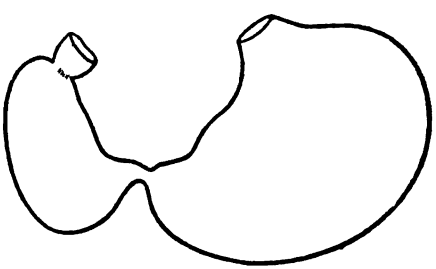
The communicating passage is just large enough to allow the forefinger to pass with ease, and the length of the constricted portion is about half an inch. On turning the stomach inside out, the mucous membrane is seen to be smoothly continuous from one cavity to the other; and, whether in the collapsed or distended state, there is no appearance of a valvular fold or thickening of the coats in the contracted part, or where it opens into either sac. The left division is fully one-third more capacious than the other, and forms a large cul-de-sac projecting in front of the œsophagus, which enters towards the upper and back part. In the sketch, which presents an anterior view, the left division is represented as twisted forwards, so as to show the place of entrance of the œsophagus, the lower part of which is naturally concealed by the prominence there appearing to lie to its left side; and this rounded prominence seems naturally to have been received into the concavity of the lesser curvature of the pyloric sac. The right or pyloric division corresponds very much to the narrow elongated form of stomach which we sometimes see, and is not much less than some adult entire stomachs of

that form. The two sacs are together much more capacious than any large single stomach that I have seen. In the sketch, a small part of the duodenum is represented beyond the pylorus, the valve of which is fully formed. About three inches to the cardiac side of the pylorus will be observed a slight constriction, especially marked on the greater curvature. This corresponds to the slight constriction which usually, or at least frequently, exists at this part of the stomach.

Instead of agreeing with me in regarding this case as one of true or congenital bilocular stomach, some may suppose that this state of parts may possibly have resulted from morbid action. On the one hand, that the second sac was merely a dilated portion of the duodenum, due to a partial constriction beyond, whilst the constricted part between the two sacs corresponded to or was the original pylorus. Or again, that the constriction was of the nature of a morbid contraction or stricture on an originally single sac. These suppositions, however, are incompatible with the facts of the existence at the right extremity of the second sac of neither more nor less than the natural pyloric thickening and projection, and the entire absence of thickening or puckering at or near the narrow part between the two sacs, as well as the absence of the least trace of old or recent disease on any part or surface of the preparation.

Death in this case had resulted from typhus fever, and I was unable to obtain any previous history.

In my museum there is a dried preparation showing a similar peculiarity in the form of the human stomach, of which the following is a sketch:—



This preparation originally belonged to Dr Knox. For the only information I can furnish regarding it, I am indebted to Dr Lonsdale of Carlisle, formerly assistant and successor to Dr Knox, who writes to me that it was simply a dissecting-room preparation, without, so far as he is aware, any previous history.

Here, but for the constricted portion, there is less departure from the usual form of the stomach than in the former case, and in capacity the two sacs do not exceed a good sized simple stomach. The pyloric division is in this case proportionally smaller than that of the one first described, and terminates more abruptly at the pylorus,

which is seen as a well-formed nearly circular projection in the interior. On cutting into the sacs on either side of this constriction, there is no appearance of thickening or valvular projection. The orifices are smooth, and now scarcely of sufficient size to receive the little finger. On the posterior surface of the cardiac division, between two and three inches to the left of the constricted part, there is an irregular patch of thickening larger than a shilling, as if the wall had been thickened and adherent at this part. Although this thickening appears to be of pathological origin, it has no connection with the constricted part, which appears to be quite natural or healthy, as in the case just recorded.

The occurrence of a contraction about the middle of the stomach has been noticed by various anatomists. Morgagni,[1] in relating the post-mortem examination of a lady who had for many years been troubled with frequent vomiting, states that "the stomach was contracted, and near the antrum pylori the contraction was so much greater than in other parts, that the viscus appeared as if it had been divided into two cavities." After ascribing the symptoms in this case to other conditions, he goes on to say, "I might have supposed that the contracted state of the stomach had some influence in producing this affection had I not observed the same structure in other women, who had not been subject to vomiting. Similar contractions, too, have been discovered by other anatomists in persons of both sexes; and sometimes the aperture of communication between the two portions of the stomach was exceedingly small. Doubtless in the preceding case the contraction was congenital." Morgagni again refers in the same work to this condition, as having been noticed in several of his dissections, and observes that "the aperture of communication between the two portions of the stomach was scarcely larger than the natural extent of the pylorus." On this the editor remarks, in a note, "central contraction of the stomach is often met with; but, in general, it appears to be a temporary affection. In all the instances which have occurred to me, it was instantly removed by inflation. Sometimes, however, it is permanent; and the aperture between the two portions has been found by others as small as Morgagni has represented it above. Sir E. Home and Dr Monro have described this morbid appearance. In the Medico-Chirurgical Journal for January 1816, Dr Palmer has related a case in which the contraction was as perfect as if a tight ligature had been applied, and great difficulty was found in passing the little finger through the narrow ring which separated the cardiac and pyloric portions. The inner coat seemed healthy, except at the strictured part where the coats were thickened; and adjacent to it there were some spots resembling minute granulations."

---

[1] The Seats and Causes of Diseases investigated by Anatomy, 1779. Edition by W. Cooke. London, 1822.

Dr Monro[1] states that a contraction in the middle of the stomach has been described by Blasius, Lorry, Walter, Hufeland, and Van der Kolk, and he also notices the cases described by Morgagni. The constrictions here referred to by Dr Monro appear to have been chiefly spasmodic, and to have disappeared soon after death or on distension, since he refers, as a contrast, to one case of permanent contraction in a preparation before him, but the constriction was not greater than to give the appearance as if a cord had been "tied very slightly around the stomach."

Sir E. Home has paid particular attention to this subject, having written several papers on the structure and form of the stomach in various animals and in man. In one of these[2] he expresses the opinion that the human stomach is divided by a muscular contraction into a cardiac and pyloric portion; that this contraction is the result of the natural action of the organ in digestion, and disappears soon after death; so that unless the body be examined within twenty-four hours the contraction will not be seen. In connection with this he gives a representation (Plate XI.) of the figure of the stomach in this condition, in which the contraction is moderate and gradual, leaving between it and the pylorus about a fifth part of the length of the organ. In a subsequent paper[3] the same author relates a case in which, by a morbid contraction near its middle, the stomach was as completely divided as in the two cases I have related. In the drawing illustrating this case, the constricted portion has not a diseased appearance, but the contraction is described as morbid, and it is supposed that the morbid action had ensued in the part at which the physiological contractions had formerly taken place.

In describing the form and curvatures of the stomach, E. Huschke observes:[4]—"Home admits, as in the stomach of the rodentia, a contraction towards the pylorus; this constriction does not always exist, and when it is present, distension with air sometimes removes and sometimes leaves it. In the first case, it arises from spasm of the muscular coat which existed during life; in the second, it is due to a contraction of all the coats of the viscus."

M. Cruveilhier remarks on this subject:[5]—"The examples of double or triple stomachs in the human subject are merely cases of single stomachs having one or more circular constrictions. The essential character of a double stomach is not an accidental or even a congenital contraction, but a difference in structure. Bilocular stomachs, indeed, are very common; but this form (resembling that of some kind of calabash-gourds), though sometimes extremely well-

---

[1] Morbid Anatomy of the Human Gullet, Stomach, and Intestines. 1811.
[2] Philosophical Transactions of the Royal Society of London, 1807, pp. 170, 171.
[3] The same work, 1817, pp. 350, 351.
[4] Encyclopédie Anatomique, 1845, tome v., Splanchnologie, p. 46.
[5] Descriptive Anatomy, 1841, vol. i., p. 467.

marked when the stomach is empty, disappears almost entirely when it is much distended by inflation."

From the consideration of these various statements and opinions regarding constricted or double stomach, it appears that the condition chiefly referred to is that of a moderate constriction, which disappears soon after death or on distension. In a few of the cases, however, the constriction has evidently been permanent, though the want of precision in the descriptions leaves it doubtful whether the constriction was moderate or deep. In Sir E. Home's case the constriction was deep, but was regarded by him as morbid. In Dr Palmer's case, the communicating passage was very narrow, but there seems to have been thickening of the coats, and ulceration of the lining membrane at this part. It may therefore be concluded that there have been very few if indeed any instances placed on record in which the stomach was so deeply and permanently divided, independent of disease, as in the two cases I have had the opportunity of recording.

An analogy may be drawn between this condition of the human stomach, and that more especially of some rodent animals; but only in so far that there are two cavities separated by a constriction, varying in position and extent in different genera; in other respects, as regards form, position of orifices, and structure of the lining membrane, there is no correspondence. In regard to the latter point, I speak only from the appearances presented to the naked eye, the specimen in the case first related having been too old for microscopic examination; but the lining membrane of both cavities presents the same appearance, and the thickness of the walls was the same in both and at the constriction. M. Cruveilhier would regard a difference in structure as essential to constitute the case one of "double stomach." But,—whilst in animals in which the stomach consists of more than one cavity, there is a difference in structure in accordance with their habits and the stages of their digestive process,—I should rather expect, in the case of a division of the human stomach into two parts, not to find any difference in their lining membrane, greater at least than that which normally obtains in the microscopic structure of the cardiac and pyloric portions.

Independent of the condition of true or permanent constriction, the preceding notice of the history of this subject brings out prominently the circumstance of the occasional or frequent existence of a *temporary constriction*, which is worthy of notice in connection with the physiology of the stomach.

Sir E. Home is of opinion that this is the natural condition of the organ during digestion, the constriction serving the purpose of retaining the food in the cardiac division until it be prepared for farther change in the pyloric. He admits, however, that the circular muscular fibres are in no way more developed at any particular part, which we would rather expect to find were this a regularly recurring condition. In support of this view he states that he has

seen a similar temporary constriction on the stomachs of some animals in which the organ is naturally simple, when the examination was made soon after death. In connection with this may be mentioned the observation made in regard to contraction of the stomach in the well-known case of St Martin. It is described as contracted about four inches from the pylorus, so that a tube introduced by the fistulous aperture was first resisted at this part, and, having passed the contraction, was then grasped and drawn on by the action of the parietes beyond. From the description given, however, it does not appear whether there was really an hour-glass contraction, as supposed, at this part, or whether the whole of the pyloric portion was not also in a contracted condition.

The question, as to whether the stomach is naturally and periodically constricted so as partially to divide it into two cavities during digestion, is worthy of being inquired into. In doing so, it must not be forgotten that the constriction will not be apparent under all circumstances;—that it will be noticed especially when death has taken place during or soon after digestion;—that too rapid or forcible distension may at once destroy it;—and that it cannot be noticed unless the body be examined soon after death.

ARTICLE V.—*Case of Spontaneous Evolution.* By FRANCIS ADAMS, LL.D., Surgeon, Banchory.

SEPTEMBER 19, 1850.—I was called to a woman who had just parted with a child, which I found still enveloped in its membranes, with the waters not discharged. Upon examination, the placenta could be felt at the mouth of the womb, but there being no piece of cord to pull by, nothing could be done to forward its expulsion without using undue violence; and as the discharge of blood was very moderate, and I was otherwise particularly engaged, I merely directed that she should be kept cool, with her head low, and that I should be informed of the further progress of the case. About two hours afterwards, I was again sent for, when I found that a placenta had been expelled, and was hanging by its cord; that another child was coming down with the arm protruding, and that the head could be felt pressing down at the symphysis pubis. The violence of the expulsive efforts was so great, that it was clearly impracticable to turn and bring down the inferior extremity; and therefore I rather assisted by pulling at the arm and the shoulder; but the woman was so ungovernable, that I could do little or nothing in this way, and in the course of a few minutes delivery took place by the natural efforts; but, at the same time, a complete evolution of the child occurred, the breech and legs being first expelled, after which the shoulders and head easily followed.

This case, then, is very similar to the case of spontaneous evolution published, by Dr Dyce of Aberdeen, in a late number of this Journal. Dr Dyce's remarks are very interesting, but I cannot agree with him, that spontaneous evolution is of so rare occurrence as he and other obstetrical authorities represent. It is certain, that in many twin cases premature delivery takes place; that in many of these cases there is a cross-presentation; and farther, that in most of them delivery takes place without artificial assistance. Such cases, then, attract little notice, and are seldom reported, as the delivery is accomplished by the natural efforts. When a cross-presentation takes place in the case of a second child, it is frequently impossible by any means to push back the arm and shoulder, owing to the intense contraction of the uterus, which no medicine has much power in relaxing,—for even chloroform has little effect in suspending the tonic contraction of the uterus when fairly established. Under such circumstances, in the course of thirty years' practice, I have been twice compelled to amputate the arm at the shoulder-joint, and then, having succeeded in bringing down the chest so far as to allow of its being opened, I effected the delivery by the breech. Failing this, the only alternative would be to decapitate the child, and then to bring down first the body and afterwards the head.

It will be remarked, that in this case there was but one placenta, and that it was separated at the birth of the first child, and lodged in the mouth of the womb, from which it was discharged some time before the birth of the second child. Yet, as stated above, there was little or no hemorrhage, as there must have been provided there were any vessels, such as the Hunters have described, connecting the placenta to the uterus. While the uterus was distended by a second child, including its membranes, a formidable hemorrhage must have taken place from the ruptured extremities of the utero-placental vessels, provided any such vessels actually existed.

ARTICLE VI.—*On Aloin: the Cathartic Principle of the Aloes.* By Messrs T. and H. SMITH, Chemists, Edinburgh.

DURING last summer, having occasion to prepare a quantity of the aqueous extract of aloes—a preparation in repute with many medical men—we made use of cold rather than hot water as the solvent, from an impression that a more perfect, though less bulky, result would be obtained, by leaving in the marc a large proportion of resinous matter, which must necessarily be contained in the extract prepared from a hot solution. Further, with the view of rendering the preparation as perfect as possible, the filtered liquid was evaporated *in vacuo*,—thereby avoiding an excess of heat, under the in-

fluence of which and exposure to the air, recent researches show the cathartic property of the aloes is seriously impaired.

After the syrupy liquid had cooled, certain appearances arrested our attention, and seemed of sufficient importance to induce a departure from the original intention of forming it into a dry extract. The vessel containing the thin extract was now, therefore, set aside for a few days; and when again examined, the contents were found to have lost their original liquid state, and to have become loaded with a mass of granular crystalline matter. The whole was at once put into a cloth, and freed from the liquid portion by strong pressure. The crude solid substance left in the cloth being very impure, required further treatment before an opinion as to its nature could be formed, and was therefore dissolved in hot water, and filtered while still hot, out of contact with the air. On the cooling of the liquid, the substance separated in a crystalline state, and of a deep yellow colour. After a second pressure, it was subjected to another treatment exactly like the last. The solid cake resulting from the third pressure, after being completely dried, presented the appearance of an opaque straw-yellow mass, breaking short and of a dull fracture. Its taste was intensely bitter and distinctly aloetic, but entirely without smell. It was found to be quite combustible, and to leave no ash on being burned on platinum foil. Its solution acted on test paper neither as an acid nor an alkali; it was, therefore, a neutral body. It dissolved in very small quantity in cold water—not above a grain to the ounce—but very readily when heated. It is very soluble in acetic ether, and also in acetic acid, of about 25 per cent., even in the cold; four grains may be easily dissolved in a fluid drachm of either of these liquids. In lime water it is considerably more soluble than it is in cold distilled water. Oil of turpentine and chloroform do not appear to have any solvent action on it. It is very readily dissolved by aqua potassæ, and other alkaline liquids. In rectified spirit, with the assistance of heat, it dissolves in large quantity; and on very slowly cooling, out of contact with the air, it crystallises in beautiful yellow satiny tufts of rhombic plates. The spirituous solution, however, refuses to give crystals unless above forty grains to the fluid ounce of spirit be used. It is, therefore, very soluble in this menstruum—twelve parts by measure being capable of holding one part in solution. Weak spirit is also a good solvent; for on adding water to a strong alcoholic tincture, there is no separation. Sulphuric ether dissolves it very sparingly.

From a consideration of all these characters, we became satisfied that the substance which had been separated by us from the commercial Barbadoes aloes had never been made known before, and, therefore, was a new substance; but whether the interest attached to it should terminate here remained to be seen. Of course, the first and most important question which suggested itself to the mind, and which remained to be solved, was—Whether the crystalline substance, which we had now convinced ourselves was an educt, and

not a product, from the aloes, had any of the virtues of aloes as a medicine, or whether the virtues of aloes were concentrated in it, and depended on its presence for their action on the living body.

The first trial, with the view of ascertaining its action on the body, was made with half a grain, and it acted twelve hours after being taken, in the manner that so characteristically distinguishes the action of aloes. The same quantity was again given to two healthy young men, with a similar result, except that in one about twenty-four hours elapsed before its operation. In the next two cases, one grain failed to act; but in one of these, the dose having been increased to two grains, a very strong operation was the consequence. In a case where four grains were given, the person, a patient of Dr Robertson, Physician to the Edinburgh Royal Infirmary, had been getting one quarter grain doses of elaterium with little effect. The operation in this case was rather violent. In numerous other cases in which it was tried, it invariably operated in doses of from one to two grains. The result of these trials was the removal from our minds of every doubt of the crystalline body yielded by the aloes being that on which its cathartic action depended, being, in fact, the active principle of the drug.

To the new crystalline substance we have given the name of aloin. It was obtained, as already stated, from Barbadoes aloes; and it became a question, both interesting and important, to ascertain whether it could be procured from other kinds, such as the Socotorine and Cape. With this view, we, therefore, put these two kinds of aloes under treatment, operating in the very same way as with the Barbadoes, but without success. That the aloin is present in both of these sorts there cannot be a doubt.[1] In fact, we have no hesitation in stating our belief, that we shall yet obtain it from these, the result of what little we have done convincing us of its existence in them as well as in the Barbadoes sort, but by the presence of impurities, chiefly, we suspect, of a resinous nature, the aloin is probably so entangled that it cannot get freedom for that arrangement which seems necessary for ready crystallisation. That a crystalline body may be present, and yet may not crystallise, will, we believe, be readily admitted by any one much occupied with the process of crystallisation. Another cause may perhaps be found in changes and decomposition, originating in faulty processes of preparation, the result of ignorance, carelessness, or fraud. That faulty methods of preparing the aloes may be one cause of certain kinds of the drug not giving aloin, is rendered probable by the following circumstance

[1] Since writing the above, a vessel containing a solution from the Cape aloes, and which had been put aside and remained undisturbed for a long period, has been examined, and found to contain a mass of crystalline matter, exactly of the same appearance as that first obtained from similar solutions of the Barbadoes aloes, which, we feel satisfied, will turn out to be aloin.

which came under our observation. If aloin be crystallised, either from water or rectified spirit, and the crystals left in contact with the mother liquid, in the course of a few weeks, but more quickly in the latter than in the former case, the crystals will have then entirely disappeared, thus showing the readiness with which the aloin loses its crystallisable character. Keeping these considerations in view, and taking advantage of what little we now knew of the properties of the aloin, the following steps were taken :—

The watery solution of Cape aloes having been mixed with a very little sulphuric acid, to separate a quantity of colouring matter along with a fatty acid and chlorophyle, was filtered and then evaporated in vacuo to a thin extract. The extract was shaken with a quantity of ether, which was then poured off, and allowed to evaporate very slowly. Aloin at length crystallised, but of course, from its slight solubility in ether, the quantity was very small; but the thick liquid which had been washed with the ether, after standing undisturbed for a week or two, was found to have yielded a crystalline crop of aloin considerably more abundant.

The nature of the process by which the presence of aloin in the Socotorine aloes was tested by us was somewhat different from the preceding. A strong alcoholic tincture of the aloes having been prepared, sulphuric ether was added as long as any precipitate was produced. A dark coloured compound of lime being thus taken out of the way, the ethero-spirituous solution, after becoming quite clear, was mixed with a small quantity of water. The result was a separation of the mixed liquids into two distinct portions: the one heavier, composed of most of the water and spirit and a little ether, forming the under portion; and the lighter, consisting principally of ether, but containing a little spirit and water, floating above it. The heavy liquid, having been separated, was exposed to a heat of about 130° Fahr. till the spirit and ether had completely evaporated. The watery solution now left on cooling gave crystals having the characteristic appearance of aloin, and a trial strengthened this belief of their nature. The lighter liquid, on slow evaporation, likewise produced similar crystals.

The solutions of aloin are wonderfully altered by the action of the air. One striking experiment made by us will perhaps place this character of aloin in as strong a light as any that could be stated. Two grains of aloin having been dissolved in two ounces of distilled water, the solution, which was of a very pale yellow colour, was corked up in a phial that was nearly filled with it, and then set aside in a heat of about 120° Fahr. The colour of the solution gradually passed into a fine red, deepening continually in intensity, till in a few weeks the colour had become so deep as almost entirely to obstruct the passage of light.

It may not, also, be without some interest to mention here, the singularly striking effect of nitric acid upon the aloin. Half a drachm of strong commercial nitric acid being put into a test tube, and fif-

teen grains of aloin gradually added, great heat and violent action are produced, with the escape of red fumes, and a perfect solution is the result. The syrupy liquid being now added to cold water, a yellow precipitate is formed, which, however, dissolves on adding more water; and, on neutralising the liquid with aqua potassæ, a liquid of a splendid red colour is produced. The intensity of the colour is so great that, on comparing it with a liquid of equal bulk, and containing the colouring matter of two ounces of cochineal, that coloured by the aloin is of a deeper red, and even in the richness of its shade rivals the other. When the liquid is slowly evaporated, tufts of long needle-shaped crystals of chrysammate of potash are produced.

For an account of the chemical characters of aloin, we beg to refer to an article by our friend, Dr Stenhouse, in the "London, Edinburgh, and Dublin Philosophical Magazine" for December last, from a consideration of which it will be quite obvious, that the substance we have obtained from the aloes is quite different from that described by Mr Edmund Robiquet.

21, Duke Street, Edinburgh, January 1851.

Article VII.—*On the Use of Cod-Liver Oil in Pulmonary Consumption.* By James P. Bramwell, M.D., Perth.

It is now generally allowed that the influence of cod-liver oil over pulmonary consumption is greater than that of any known therapeutic agent, and the constantly increasing quantity of this substance consumed goes far to show, that, in the treatment of this dreadful malady, it bids fair, sooner or later, to supplant the other remedial means on which we were compelled formerly to rely.

There can be little doubt that the results arrived at from the use of this remedy, will bear a very favourable comparison with those formerly obtained before this medicine was introduced into practice; and this will be found to hold good, as regards the mean duration of life in those cases which have terminated fatally, as well as in the number of recoveries.

From the combined results of Louis' and Bayle's tables, Dr Gregory's experience, and Dr Home's pathological tables in the "Edinburgh Medical and Surgical Journal" for 1838, we find that the ordinary duration of pulmonary consumption in fatal cases is not more than six months. Out of 297 cases treated in the Edinburgh Infirmary, 170, or one in every 1·7, died. But we are not to suppose that this was the true mortality, as no surveillance appears to have been kept over those that were discharged; and from what we know of the fatality of this complaint, which has so long been the opprobrium of the profession, we are warranted in regarding the ratio of the deaths as much higher.

"No hope" was formerly the sentence pronounced by the physician on his consumptive patient, and time, with few exceptions, too surely confirmed his prognosis. When we find, however, "forms and degrees of this disease that former experience had taught us to be utterly hopeless and speedily fatal, retarded, arrested, nay, sometimes even removed and almost obliterated, by various processes of restored health,"[1] we are called upon to remodel our opinion, at least to some extent, and to hold out prospects to our consumptive patients, that would formerly not have been consistent with truth, or ever likely to be realised.

"Pulmonary phthisis," says Professor Bennett,[2] "is now a very curable disease;" and from the results of cases published by him, he is certainly warranted in making a statement which many may be inclined to question. Be that as it may, experience has shown, that the "cure of this disease may be undertaken by the use of the cod-liver oil, with much greater hope of success than by any other agent, medicinal, dietetic, or regimenal, that has ever yet been employed."[3]

There are many cases now on record where cures have been effected by the use of cod-liver oil, in all stages of pulmonary consumption; and when the result obtained has not been so favourable, life has unquestionably been prolonged, and the demon of destruction for a time arrested.

The following cases, extracted from my note books, certainly tend to confirm the truth of the foregoing remarks.

CASE I.—*Incipient Consumption in Left Lung; Fistula in Ano; Cod-Liver Oil; Recovery.*

Mr A. Mac———, æt. 18, of a strumous family, consulted me in October 1849, on account of a troublesome cough which he caught from exposure to cold some months before. Says he has been losing flesh rapidly for some time past, and complains of night fever, followed by copious perspiration. Cough very troublesome in the morning, accompanied with muco-purulent expectoration. Says he has consulted a physician of much experience in the town where he has lately resided, who regarded his case as all but hopeless.

Physical Signs.—There is dulness on percussion under the left clavicle, with persistent muco-crepitation in the same region; respiratory murmur feeble over all the left side; no morbid sounds on right side; natural breath-sound distinctly audible; pulse 115. Abscess forming in close contact with the rectum.

Treatment.—To take cod-liver oil in dessert-spoonfuls three times a-day. To rub in, morning and evening, croton oil liniment over the chest. To be confined to his room.

June 1850.—Has continued the treatment, and can now take the oil in table-spoonful doses. There is a remarkable improvement. The night hectic and perspirations are quite gone; cough much less frequent. He can now walk several miles without much fatigue. The respiratory murmur is, however,

[1] Williams—London Medical Journal for January 1849.
[2] Bennett—Edinburgh Monthly Medical Journal, May 1850.
[3] Williams—London Journal of Medicine.

still deficient in the left side, and there is still slight dulness on percussion under the left clavicle, but no muco-crepitation.

October.—He is now quite free from all phthisical symptoms, and very desirous that the fistula should be operated on. To this I consented.

January 12th, 1851.—I have seen my patient only a few days ago. He has passed the winter hitherto without any return of his former symptoms, and feels strong and well.

Case II.—A. Cross, æt. 20, of fair complexion, bookbinder by trade, was admitted into the Perth Infirmary under Dr Macfarlane's care, in the beginning of March 1849, with well-marked symptoms of pulmonary consumption. States that for several months past he has suffered from troublesome cough, with loss of flesh, fever in the evening, with copious morning sweats, and that, during a severe fit of coughing, a considerable discharge of blood from the lungs took place, which alarmed him, and forced him to seek for advice.

Physical Signs.—The expansion of the chest is only very partial, and performed with difficulty. There is distinct dulness on percussion under the left clavicle. The only morbid sound appreciable is a muco-crepitant râle in the apex of the left lung. Breath-sounds feeble on both sides.

Treatment.—Ol. jecoris aselli three times a-day in dessert-spoonful doses, with the free use of the croton oil liniment to the chest.

June.—There is now a marked improvement in all his symptoms; he has gained flesh considerably, and coughs but little; there are now no moist râles audible; respiratory murmurs still very feeble. He left the hospital soon after, and went to Dunkeld, where he remained all winter and part of the following spring. I had several letters from him, in which he stated that, with the exception of some cough, he was enjoying tolerable health, though very weak.

January 12th, 1851.—For several months past our old patient has been labouring as a congregational missionary in town; and, although not very strong, he can spend four or five hours a-day in his district. I have to-day examined his chest, on account of a slight cold which he has contracted, but can detect little or no trace of his former complaint. The natural breath-sounds are distinctly audible on both sides, and the muco-crepitation is quite gone. There is still, however, a very slight degree of dulness under the left clavicle, and the respiratory murmur is not quite so loud here as in other portions of the chest.

Case III.—*Pulmonary Phthisis of Left Lung; Hectic; Great Marasmus; Cod-Liver Oil; Recovery.*

Mrs ———, æt. 25, of dark complexion, slender configuration of chest, and curved spine. I was called to see this lady three months after the birth of her first child; she was much emaciated, and had been suffering from cough more or less constant for several months past. She has regular night-fever, with copious morning sweats. Character of expectoration muco-purulent, with now and then a little blood. Right lung expands pretty freely; respiratory murmurs somewhat feeble, but no ronchi audible. There is distinct dulness on percussion over the spine of the left scapula, with muco-crepitation in the same region.

Treatment.—To take the cod-liver oil in dessert-spoonfuls three times a-day. Chest to be rubbed night and morning with a liniment of strong acetic acid and turpentine. Bacon, fresh or salt, to dinner or breakfast.

February.—It is now nearly four months since the oil was first administered, and for some time past she has taken it in tablespoonful doses. The change that has taken place has certainly astonished me. She has now no hectic; her pulse is 80; has gained much flesh; coughs and expectorates very little.

July.—Continues to improve; still uses the oil regularly. The chest has been frequently examined during her convalescence, and repeated applications

of the croton oil liniment have succeeded in driving off the muco-crepitation, which more than once returned to the apex of the left lung. Catamenia have of late regularly appeared.

January 12th, 1851.—It is now about two years and a-half since this lady began to take the oil. She is now quite free from all her former symptoms, and enjoys a fair proportion of good health, being in the fourth month of her pregnancy.

CASE IV.—*Incipient Phthisis in the Apex of the Right Lung; Hæmoptysis; Cod-Liver Oil; Recovery.*

Mr R., teacher, æt. 18, dark complexion, consulted me, in October 1849, upon what appeared to him to be a threatening of pulmonary consumption. States that he has suffered for some time past from a troublesome cough with little expectoration. About eight days before, during a severe fit of coughing, he was alarmed to find his mouth suddenly filled with blood, of which he lost about one teacupful; since that, mucus tinged with blood has been expectorated; pulse 96; little or no emaciation. The chest expands well, better than most chests do, but not equally; a distinct difference is appreciable by standing behind and placing a hand over each clavicle; while the left lung expands fully, the apex of the right does not dilate to anything like its normal extent. There is also dulness under the right clavicle when percussed, and deficiency of breath-sounds in the same region.

Treatment.—Blood to be taken by cupping from the right infra-clavicular region; a mixture of digitalis and antimony to be used for a few days, and then to begin the use of the cod-liver oil in dessert-spoonful doses.

March 1850.—He has passed most of the winter in the country, and is very much improved. There has been no return of the hæmoptysis; the natural breath-sounds in right lung are now much more distinct. He now takes the oil in tablespoonful doses, and is remarkably fat.

January 12th, 1851.—He is now quite recovered, and has resumed his teaching some months ago.

CASE V.—*Incipient Phthisis of Left Lung; Hæmoptysis; Hectic, &c.; Ol. Jecoris Aselli; Recovery.*

Mrs B., æt. 35, consulted me on account of a cough which has troubled her more or less for the last three months. Says she has been very prostrate for many months past, and ascribes this in great measure to two abortions which she has had during the last twelve months. Her cough has been, for the most part, attended with little expectoration; but about a week before she applied for advice, about a tablespoonful of blood escaped from the lungs. She is much emaciated, and suffers from hectic and copious morning sweat; pulse 100; breath-sound weak in both lungs; there is distinct dulness under the left clavicle when tapped, and on applying the ear to the same region, there is distinct muco-crepitation audible.

Treatment.—To take the ol. jecoris aselli in dessert-spoonful doses three times a-day, and to increase the dose as soon as possible to as many tablespoonfuls. Liniment ol. tiglii to be freely applied to the chest morning and evening.

Under this treatment she improved daily, gained flesh, and ceased to suffer from hectic and perspiration; there has been no return of the hæmoptysis.

It is now more than three years since this person began the oil. She has suffered no relapse, and is at present in good health.

CASE VI.—Elizabeth Macfarlane, æt. 26, of a dark complexion and fragile configuration of body, was admitted into the Perth Infirmary in January 1850, under Dr Macfarlane's care, with considerable reluctance, owing to the unpromising state of her case. Says that for several months past she has felt much

oppression about the chest, and has become very weak and much emaciated; has regular night hectic, followed by drenching perspiration.

Physical Signs.—There is distinct dulness on percussion under the left clavicle, with muco-crepitant râles in the same region. Natural breath-sounds deficient on this side; no morbid sounds appreciable on the right side.

Treatment.—Cod-liver oil three times a-day in dessert-spoonful doses. Croton oil liniment to chest.

April.—She has borne the oil well for some time past in table-spoonful doses, She is remarkably improved, and anxious to be discharged from the house.

January 1851.—I have called upon this patient lately for the purpose of examining her chest. She says she is quite well, and is free from all her former symptoms. On examining her chest, I can detect nothing farther than a very slight want of resonance on percussing the infra-clavicular region of the left side, and also a want of natural breath-sounds in the apex of the same lung.[1]

CASE VII.—*Pulmonary Phthisis in Left Lung; Tubercular Softening; Great Marasmus; Ol. Jecoris Aselli; Retardation of the Disease.*

Janet Laing, æt. 17, applied at the Perth Dispensary in September 1849, on account of a bad cough, which troubled her more or less ever since she was exposed to cold and wet several months before. States that she has been losing flesh very rapidly, and complains much of night fever, with drenching morning sweats; expectorates copiously; character of expectorated matter highly purulent; catamenia gone. There are distinct indications of tubercular deposit and softening in the apex of the right lung, as shown by the want of resonance on percussion, and loud muco-crepitation. There is no morbid sound appreciable in the left side, but the respiratory murmur is very feeble.

Treatment.—To take the cod-liver oil in dessert-spoonfuls three times a-day, and the croton oil liniment to be applied to the chest.

December.—There is as yet very little improvement; expectoration very copious; great debility. To increase the dose of the oil to a table-spoonful three times a-day.

May 1850.—This poor girl has had a hard struggle for life; she is now, however, very much improved; expectoration much less in quantity; no hectic or night sweats.

January 12, 1851.—As long as the summer weather lasted she continued to improve, and was able to take exercise in the open air; but as the winter came on, she suffered to a certain extent from a return of her former symptoms.

Physical Signs.—There is still dulness on percussion under the right clavicle, and some muco-crepitation in the apices of both lungs.

To resume the use of the oil, which she has given up for several months, and also the counter-irritation to the chest.

CASE VIII.—*Pulmonary Phthisis in its third stage; large cavity in the Apex of Left Lung; Hectic; Cod-Liver Oil; Marked improvement.*

Mary Morris, æt. 25, domestic servant, fair complexion, and stout configuration of body, was admitted a patient of the Perth Dispensary under the care of Mr R. Christie in February 1849, on account of a bad cough, which she contracted while washing clothes some four months before. She is now much emaciated; perspires copiously during the night, and expectorates large quantities of muco-pus.

Physical Signs.—The respiration on the right side is free and unembarrassed,

[1] We have good reason to believe, that a recovery, more or less perfect, has taken place under the use of the oil in these six cases, as all of them are now free from both local and general symptoms of pulmonary phthisis.

and there is no morbid sound appreciable. There is distinct mucous gurgling and pectoriloquy heard over the apex of the left lung, which also sounds dull on percussion.

Treatment.—To take cod-liver oil in dessert-spoonful doses three time a-day. Croton oil liniment to the chest.

July.—Her general health is now much improved; perspires much less at night; the expectoration very much diminished in quantity. She can now take exercise in the open air, and is in the habit of walking to church, which is about a mile distant. There are still, however, distinct indications of an existing cavity.

The same treatment to be continued.

January 12, 1851.—I have examined this patient, along with Mr Christie, a few days ago. She has kept her ground remarkably well, but has still some cough and slight expectoration, but no hectic, and has gained flesh considerably. The right lung seems still free from all disease, and the respiration in it is almost puerile. In the apex of left lung there is distinct cavernous respiration, but no garguillement; respiratory murmur very feeble in this side; dulness on percussion under left clavicle still appreciable.

Whether the oil may eventually, in this and the preceding case, bring about a cure is a matter of uncertainty; but there is the strongest reason for believing that it has retarded the onward march of a disease which would otherwise long ere now have proved fatal.[1]

CASE IX.—*Tubercular Phthisis in its second stage; Copious Purulent Expectoration; Hectic, &c.; Cod-Liver Oil; Retardation of the Disease; Death from Hæmoptysis.*

E. Monro, weaver, æt. 33, presented himself at the Dispensary in October 1848. States that he has been suffering from cough, with copious expectoration, for some months past, and that he has of late become very thin and weak, and quite unfit for work; perspires copiously during the night, and complains of flushing of the face and fever in the evening; pulse 110. There is distinct dulness under the left clavicle when percussed, and mucous and sub-crepitant râles in the same region. The right lung appears free from disease.

Treatment.—Ol. jecoris aselli in dessert-spoonfuls three times a-day. Croton oil to the chest morning and evening.

September 1849.—Monro called at my house a few days ago. Says that after using the oil for three months, he found himself remarkably improved; his cough and expectoration were almost gone, and he had gained much strength. Some four months after this he thought himself sufficiently strong to resume his work, and returned to his damp workshop. From this day he began to relapse, and his former symptoms slowly returned; he has had a sharp hæmoptysis a few days ago, which has agitated him a good deal. To resume the oil as formerly, and for some time to come to give over all work.

For more than a year after he kept remarkably well, but owing to pecuniary difficulties he was obliged again to take to his cold damp workshop, and again his phthisical symptoms returned. Some six months after, this poor man died suddenly from severe hæmoptysis.

Had the circumstances in which this patient was placed been more favourable, and no necessity laid upon him to provide for a starving family, the result of his case might have been very different; but even with every disadvantage, his life was most unquestionably prolonged by the use of the cod-liver oil.

---

[1] In proof of the not unfrequent cicatrization, or fistulous transformation, of pulmonary caverns, see Andral's Clin. Med., tom. iii.; also Lænnec on Diseases of the Chest, page 311.

Part Second.

REVIEWS.

ARNAULD'S *Port-Royal Logic.* Translated by T. S. BAYNES.

THOMSON'S *Outlines of the Necessary Laws of Thought.*

DESCARTES *on the Method of Rightly Conducting the Reason, and Seeking Truth in the Sciences.*

COLERIDGE'S *Essay on Method.*

WHATELY'S *Logic and Rhetoric.* New and cheap edition.

MILL'S *Logic.* New and cheap edition.

DUGALD STEWART'S *Outlines.*

SIR JOHN HERSCHEL'S *Preliminary Dissertation.*

Quarterly Review, vol. lxviii.—Article upon Whewell's Philosophy of Inductive Sciences.

"PRAY, Mr Opie, may I ask what you mix your colours with?" said a brisk dilettante student to the great painter. "With *Brains*, Sir," was the gruff reply—and the right one. It did not give much of what we call information; it did not expound the principles and rules of the art; but, if the inquirer had the commodity referred to, it would awaken him—it would set him a-going, a-thinking, and a-painting to good purpose. If he had not, as was likely enough, the less he had to do with colours and their mixture the better. Many other artists, when asked such a question, would have either set about detailing the mechanical composition of such and such colours, in such and such proportions, rubbed up so and so; or perhaps they would (and so much the better, but not the best) have shewn him how they laid them on; but even this would leave him at the critical point. Opie preferred going to the quick, and the heart of the matter—"*Brains, Sir.*"

Sir Joshua Reynolds was taken by a friend to see a picture. He was anxious to admire it, and he looked it over with a keen and careful eye. "Capital composition—correct drawing—the colour, tone, chiaroscuro excellent; but—but—it wants, hang it, it wants—*That*," snapping his fingers—and, wanting "that," though it had everything else, it was worth nothing.

Again, Etty was appointed teacher of the students of the Royal Academy, having been preceded by a clever, talkative, scientific expounder of æsthetics, who delighted to tell the young men *how* everything was done, how to copy this, and how to express that. One came up to the new master, "How should I do this, sir?"

"Suppose you try." Another, "What does this mean, Mr Etty?" "Suppose you look." "But I have looked." "Suppose you look again." And they did try, and they did look; and they saw and achieved what they never could have done, had the how or the what (supposing this possible, which is not likely in its full and highest meaning) been told them, or done for them; in the one case sight and action were immediate, exact, intense, and secure; in the other, mediate, feeble, and lost as soon as gained. But what are "*Brains*;" what did Opie mean? and what is Sir Joshua's "*That*"? What is included in it? and what is the use, or the need of trying and trying, of missing often before you hit, when you can be told at once and be done with it; or of looking when you may be shown? Everything in medicine and in painting—practical arts—as means to ends, let their scientific enlargement be ever so rapid and immense, depends upon the right answers to these questions.

First of all, "brains," in the painter, are not diligence, knowledge, skill, sensibility, a strong will, or a high aim,—he may have all these, and never paint anything so truly good or effective as the rugged woodcut we must all remember of Apollyon bestriding the whole breadth of the way, and Christian girding at him like a man, in the old sixpenny "Pilgrim's Progress;" and a young medical student may have zeal, knowledge, ingenuity, attention, a good eye and a steady hand—he may be an accomplished anatomist, stethoscopist, histologist, and analyst; and yet, with all this, and all the lectures, and all the books, and all the sayings, and all the preparations, drawings, tables, and other helps of his teachers, crowded into his memory or his note-books, he may be beaten in treating a whitlow or a colic, by the nurse in the wards where he was clerk, or by the old country doctor who brought him into the world, and who listens with such humble wonder to his young friend's account, on his coming home after each session, of all he had seen and done, and of all the last astonishing discoveries and revolutions of the day. What the painter wants, in addition to, and as the complement of, all the other elements, is *genius and sense;* what the doctor needs to crown, and give worth and safety to his accomplishments, is *sense and genius:* in the first case, more of this, than of that; in the second, more of that, than of this. These are the "*Brains*" and the "*That*."

And what is genius? and what is sense? Genius is a peculiar native aptitude, or tendency, to any one calling or pursuit over all others. A man may have a genius for governing, for killing, or for curing the greatest number of men, and in the best possible manner; a man may have a genius for the fiddle, or his mission may be for the tight-rope, or the Jew's harp, or it may be a natural turn for seeking, and finding, and teaching truth, and for doing the greatest possible good to mankind—or it may be a turn equally natural for seeking, and finding, and teaching a lie, and doing the *maximum* of mischief. It was as natural, as inevit-

able, for Wilkie to develop himself into a painter, and such a painter, as for an acorn when planted to grow up into an oak, a specific *quercus robur*. But *genius* is not enough, even for a painter, he must likewise have *sense;* and what is sense? *Sense* drives, or ought to drive, the coach; sense regulates, combines, restrains, commands, all the rest—even the genius; and sense implies exactness, soundness, power, and promptitude of mind.

Then for the young doctor, he must have as his main, his master faculty, SENSE—νους, justness of mind, because his subject-matter is one in which principle works, rather than impulse, as in painting; the understanding has first to do with it, however much it is worthy of the full exercise of the feelings, and the affections. But all will not do, if GENIUS is not there,—a real turn for the profession. It may not be a liking for it—some of the best of its practitioners never really liked it, at least, liked other things better; but there must be a fitness of faculty of body and mind for its full, constant, exact pursuit. This sense and this genius, such a special therapeutic gift, had Hippocrates, Sydenham, Pott, Pinel, John Hunter, Delpech, Dupuytren, Kellie, Cheyne, Baillie, and Abercrombie. We might, to pursue the subject, pick out painters who had great genius and little or no sense, and *vice versa;* and physicians and surgeons, who had sense without genius, and genius without sense, and some perhaps who had neither, and yet were noticeable, and, in their own sideways, useful men.

But one great object will be gained if we have given our young readers (and these remarks are addressed exclusively to students) any idea of what we mean, if we have made them think, and look inwards. The noble and sacred science you have entered on is large, difficult, and deep, beyond most others—it is every day getting more immense, more deep, and in many senses more difficult, more complicated and involved. It requires *more than the average* intellect, energy, attention, patience, and courage, and that singular but imperial quality, at once a gift and an acquirement, *presence of mind*—αγχινοια, or nearness of the νους, as the subtile Greeks called it, than almost any other department of human thought and action, except perhaps that of ruling men. Therefore it is, that we hold it to be of paramount importance that the parents, teachers, and friends of youths intended for medicine, and above all that those who examine them on their entering on their studies, should at least (we might safely go much further) satisfy themselves as far as they can, that they are not below the average in intelligence; they may be deficient *quâ medici*, and yet, if taken in time, may make excellent men in many other useful and honourable callings.

But suppose we have got the requisite amount and specific kind of capacity, how are we to fill it with its means: how are we to make it effectual for its end? On this point we say nothing, except that the fear now a-days, is rather that the mind gets too much and too many things, than too little or too few. But this means of

turning knowledge to action, making it what Bacon meant when he said it was power, invigorating the thinking substance, of giving tone, and you may call it muscle and nerve, blood and bone, to the mind—a firm gripe and a keen and sure eye: *that*, we think, is far too little considered or cared for at present, as if the mere act of filling in everything for ever into a poor lad's brain, would give him the ability to make anything of it, and above all the power to appropriate the small portions of true nutriment, and reject the dregs.

One comfort we have, that in the main, and in the last resort, there is really very little that *can* be done for any man by another. Begin with the sense and the genius—the keen appetite and the good digestion—and, amid all obstacles and hardships, the work goes on merrily and well; without these, we all know what a laborious affair, and a dismal, it is to make an incapable youth apply. Did any of you ever set yourselves to keep up artificial respiration, or to trudge about for a whole night with a soporised victim of opium, or transfuse blood (your own perhaps) into a poor, fainting, exanimate wretch? If so, you will have some idea of the heartless labour, and its generally vain and miserable result, to make a dull student apprehend—a debauched one interested, knowing, or active in anything beyond the base of his brain—a weak, etiolated intellect hearty, and worth anything; and yet how many such are dragged through their dreary *curricula*, and by some miraculous process of cramming, and equally miraculous power of turning their insides out, get through their examinations: and then—what then? providentially, in most cases, they find their level; the broad daylight of the world,—its shrewd and keen eye, its strong instinct of what can, and what cannot serve *its* purpose,—puts all, except the poor object himself, to rights; happy is it for him if he turns to some new and more congenial pursuit in time.

But it may be asked, how are the brains to be strengthened, the sense quickened, the genius awakened, the affections raised?—the whole man turned to the best account for the cure of his fellow-men? How are you, when physics and physiology are increasing so marvellously, and when the burden of knowledge, the quantity of transferable information, of registered facts, of current names—and such names!—is so infinite: how are you to enable a student to take all in, bear up under all, and use it as not abusing it, or being abused by it? You must invigorate the containing and sustaining mind, you must strengthen him from within, as well as fill him from without; you must discipline, nourish, edify, relieve, and refresh his entire nature; and how? We have no time to go at large into this, but we will indicate what we mean:—encourage languages, especially French and German, at the early part of their studies; encourage, not merely the book knowledge, but the personal pursuit of natural history, of field botany, of geology, of zoology; give the young, fresh, unforgetting eye, exercise upon the infinite diversity and combination of natural colours, forms, substances, surfaces, weights, and sizes—

everything, in a word, that will educate their eye or ear, their touch, taste, and smell, their sense of muscular resistance; encourage them by prizes to make skeletons, preparations, and collections of any natural objects; and, above all, try and get hold of their affections, and make them put their hearts into their work. Let them, if possible, have the advantage of a regulated *tutorial*, as well as the ordinary professorial system. Let there be no excess in the number of classes and frequency of lectures. Let them be drilled in composition—by this we mean the writing and spelling of correct, plain English (a matter not of every-day occurrence, and not on the increase),—let them be directed to the best books of the old masters in medicine, and *examined in them*,—let them be encouraged to a wholesome and manly literature. We do not mean popular, or even modern literature—such as Emerson, or Alison, or the trash of inferior periodicals or novels—fashion, vanity, and the spirit of the age, will attract them readily enough to all these; we refer to the treasures of our elder authors. If our young medical student would take our advice, and for an hour or two twice a-week take up a volume of Shakspeare, Don Quixote, Milton, Dryden, Pope, Cowper, Montaigne, Addison, Defoe, Goldsmith, Fielding, Scott, Charles Lamb, Macaulay, Jeffrey, Sydney Smith, Thackeray, &c., not to mention authors on deeper and more sacred subjects, they would have happier and healthier minds, and make none the worse doctors. If they, by good fortune—for the tide has set in strong against the *literæ humaniores*—have come off with some Greek or Latin, we would supplicate for an ode of Horace once a month, and a page of Xenophon. French and German should be mastered either before or during the first years of study. They will never afterwards be acquired so easily or so thoroughly, and the want of them may be felt when too late.

But one main help, we are persuaded, is to be found in studying, and by this we do not mean the mere reading, but the digging into and through, the energizing upon, and mastering, such books as we have mentioned above. These are not, of course, the only works we would recommend to those who wish to understand thoroughly, and to make up their minds, on these great subjects as wholes; but we all know too well that our art is long, broad, and deep, and time, opportunity, and our little hour, brief and uncertain, therefore, we would recommend those books as a sort of game of the mind, like cricket—a mental exercise, a gymnastic, a clearing of their eyes of the mind as with euphrasy, a strengthening their power over particulars, a getting fresh, new views of worn out, old things, and, above all, a learning the right use of their reason, and by knowing their own ignorance and weakness, finding true knowledge and strength. Taking up a book like Arnauld, and reading a chapter of his lively, manly sense, is like throwing your manuals, and scalpels, and microscopes, and natural (most unnatural) orders out of your hand and mind, and taking a game

at Sparks's, or a smart run to the top of Arthur's Seat. Exertion quickens your pulse, expands your lungs, makes your blood warmer and redder, fills your mouth with the pure waters of appetite, strengthens and supples your legs; and though at the top you may encounter rocks, and baffling debris, and gusts of fierce winds rushing out upon you from behind corners, just as in Arnauld, and all truly serious and honest books of the kind, you will find difficulties and puzzles, winds of doctrine, and deceitful mists; still you are rewarded at the top by the wide view. You see, as from a tower, the end of all. You look into the perfections and relations of things. You see the clouds, the bright lights, and the everlasting hills on the far horizon. You come down the hill a happier, a better, and a hungrier man. But, as we said, you must eat the book, you must crush it, and cut it with your teeth and swallow it; just as you must go up, and not be carried up the hill, much less merely imagine you are there, or look upon a picture of what you would see were you up, however accurately or artistically done: no—you your ownself must *do* both.

Philosophy, the love and the possession of wisdom, is divided into two things—science or knowledge; and a habit, or power of mind. He who has got the first is not truly wise unless his mind has transfigured it, unless he appropriates and can use it for his need.

The prime qualifications of a physician may be summed up in the words *Capax*, *Perspicax*, *Sagax*, *Efficax*. *Capax*—there must be room to receive, and arrange, and keep knowledge; *Perspicax*—senses and perceptions, keen, accurate, and immediate, to bring in materials from all sensible things; *Sagax*—a central power of knowing what is what, and what it is worth, of choosing and rejecting, of judging; and finally, *Efficax*—the will and the way—the power to turn all the other three—capacity, perspicacity, sagacity, to account to the performance of the thing in hand, and thus rendering back to the outer world, in a new and useful form, what you had received from it. These are the intellectual qualities which make up the physician, without any one of which he would be *mancus*, and would not deserve the name of a complete artsman, any more than proteine, would be proteine if any one of its four elements were amissing.

We have left ourselves no room to speak of the books we have named at the head of this paper. We recommend them all to our young readers. Arnauld's famous and most excellent and entertaining "Art of Thinking" is, if only one be taken, probably the best. Thomson's little book is admirable, and is specially suited for a medical student, as its illustrations are drawn with great intelligence and exactness from chemistry and physiology. We know nothing more perfect than the analysis, at page 348, of Sir H. Davy's beautiful experiments, to account for the traces of an alkali he found when decomposing water by galvanism. It is

quite exquisite, the hunt after and the unearthing of "*the residual cause.*" This book has the great advantage of a clear, lively, and strong style. We can only give some short extracts.

INDUCTION AND DEDUCTION.

"We may define the inductive method as the process of discovering laws and rules from facts, and causes from effects; and the deductive, as the method of deriving facts from laws, and effects from their causes."

There is a valuable paragraph on anticipation and its uses—there is a power and desire of the mind to project itself from the known to the unknown, in the hope of finding what it is in search of.

"This power of divination, this sagacity, which is the mother of all science, we may call anticipation. The intellect, with a dog-like instinct, will not hunt until it has found the scent. It must have some presage of the result before it will turn its energies to its attainment. The system of anatomy which has immortalised the name of Oken, is the consequence of a *flash of anticipation*, which glanced through his mind when he picked up in a chance walk, the skull of a deer, bleached by the weather, and exclaimed—'*It is a vertebral column!*'"

"The man of science possesses principles—the man of art, not the less nobly gifted, is possessed and carried away by them. The principles which art *involves*, science *evolves*. The truths on which the success of art depend lurk in the artist's mind in an undeveloped state, guiding his hand, stimulating his invention, balancing his judgment, but not appearing in regular propositions." "An art (that of medicine for instance) will of course admit into its limits, everything (*and nothing else*) which can conduce to the performance of *its own proper work*; it recognises no other principles of selection."

"He who reads a book on logic, probably thinks no better when he rises up than when he sat down, but if any of the principles there unfolded, cleave to his memory, and he afterwards, perhaps unconsciously, shapes and corrects his thoughts by them, no doubt the whole powers of his reasoning receive benefit. In a word, every art, from reasoning to riding and rowing, is learnt by assiduous practice, and if principles do any good, it is proportioned to the readiness with which they can be converted into rules, and the patient constancy with which they are applied in all our attempts at excellence."

"A *man can teach names to another man, but he cannot plant in another's mind that far higher gift—the power of naming.*"

"*Language is not only the vehicle of thought, it is a great and efficient instrument in thinking.*"

"The whole of every *science* may be made the subject of teaching. Not so with *art*; much of it is not teachable."

Coleridge's profound and brilliant, but unequal, and often somewhat nebulous "Essay on Method," is worth reading over, were it only as an exercitation, and to impress on the mind the meaning and value of *method*. Method is the road by which you attain, or hope to attain, a certain end; it is a process. It is the best direction for the search after truth. System, again, which is often confounded with it, is a mapping out of knowledge, either already gained, or theoretically laid down as probable. Aristotle had a system which did much good, but also much mischief. Bacon was chiefly occupied in preparing and pointing out the way—the only way—of procuring knowledge. He left to others to systematise the knowledge after it was got; but the pride and indolence of the human spirit leads it constantly to build a system on imperfect knowledge.

It has the trick of filling up out of its own fancy what it has not the diligence, the humility, and the honesty, to seek in nature; whose servant, and articulate voice, it ought to be.

Descartes' little tract on Method, is like everything the lively and deep-souled Breton did, full of original and bright thought.

Sir John Herschel's volume needs no praise. We know no work of the sort, fuller of the best moral worth, as well as the highest philosophy. We fear it is more talked of than read.

We would recommend the article in the "Quarterly Review" as first-rate, and written with great eloquence and grace.

SYDNEY SMITH'S *Sketches of Lectures on Moral Philosophy.* Second Edition.

SEDGWICK'S *Discourse on the Studies at Cambridge, with a Preface and Appendix.* Sixth Edition.

We have put these two worthies here, not because we had forgotten them,—much less, because we think less of them than the others, especially Sydney. But because we bring them in at the end of our entertainment, on the same principle as a liqueur—be it Curaçoa, Kimmel, or old Glenlivet—is introduced after dinner, and followed by the plum-pudding—that most English of realised ideas. Sydney Smith's book is one of rare excellence, and well worthy of the study of men and women, though perhaps not profound enough for our modern philosophers, male and female. It is really astonishing how much of the best of everything, from patriotism to nonsense, is to be found in this volume of sketches. You may read it through, if your sides can bear such an amount of cumulative laughter, with great benefit; and if you open it anywhere, you can't read three sentences without coming across some, it may be common thought, and often original enough, better expressed and *put* than you ever before saw it. The lectures on the Affections, the Passions, and the Desires, and on Study, we would have everybody to read and enjoy.

Sedgwick is a different, and, as a whole, an inferior man; but a *man* every inch of him, and an Englishman too, in his thoughts, and in his fine mother wit and tongue. He has, in the midst of all his confusion and passionateness, the true instinct of philosophy—the true venatic sense of objective truth. We know nothing better in the main, than his demolition of what is untrue, and his reduction of what is absurd, and his taking the wind out of what is flatulent, in the famous "Vestiges;" we don't say he does justice to what is good in it; his mission is to do fell justice *upon it*, and he fulfils that. His remarks on Oken and Owen, and his quotations from Dr Clarke's admirable paper on the "Development of the Fœtus," in the "Cambridge Philosophical Transactions," we would recommend to our medical friends. The very confusion of Sedgwick, is the free outcome of a deep and racy mind; it puts us in mind of what happened, when an Englishman was looking with astonish-

ment and disgust at a Scotchman eating a boiled sheep's head, and was asked by the eater what he thought of that dish? "Dish! sir, do you call that a dish?" "Dish or no dish," rejoined the Caledonian, "there's a deal o' fine confused feedin' aboot it, let me tell you," swallowing a finely confused eye, humours and all.

We conclude these rambling remarks, with a quotation from Arnauld, the friend of Pascal, and the intrepid antagonist of the Vatican and the Grand Monarque. One of the noblest, freest, most untiring and honest intellects, our world has ever seen. "Why don't you rest sometimes?" said his friend Nicole to him. "Rest! why should I rest? haven't I an eternity before me to rest in?" The following sentence from his "Port Royal Logic," so well introduced and translated by Mr Baynes, contains the gist of all we have been trying to say. It should be engraven on the tablets of every young student's heart—for the heart has to do with study as well as the head.

"There is nothing more desirable than *good sense and justness of mind*,—all other qualities of mind are of limited use, but exactness of judgment is of general utility in every part and in all employments of life.

"*We are too apt to employ reason merely as an instrument for acquiring the sciences, whereas we ought to avail ourselves of the sciences, as an instrument for perfecting our reason.* Justness of mind being infinitely more important than all the speculative knowledge which we can obtain by means of sciences the most solid. This ought to lead wise men to make their sciences *the exercise and not the occupation of their mental powers.* Men are not born to employ all their time in measuring lines, in considering the various movements of matter; their minds are too great, and their life too short, their time too precious, to be so engrossed; but they are born to be just, equitable, and prudent, in all their thoughts, their actions, their business, to these things they ought especially to train and discipline themselves."

So, young friends, bring *Brains* to your work, and mix everything with them, and them with everything. *Arma virumque*, tools and a man to use them. Stir up, direct, and give free scope to Sir Joshua's "*that*," and try again, and again; and look, *oculo intento, acie acerrimâ*. Looking is a voluntary act,—it is the man within coming to the window; seeing is a state,—passive and receptive, and, at the best, little more than registrative.

Since writing the above, we have read with great satisfaction Dr Forbes' Lecture delivered before the Chichester Literary Society and Mechanics' Institute, and published at their request. Its subject is, Happiness in its relation to Work and Knowledge. It is worthy of its author, and is, we think, more largely and finely imbued with his personal character, than any one other of his works that we have met with. We could not wish a fitter present for a young man starting on the game of life. It is a wise, cheerful, manly, and warm-hearted discourse on the words of Bacon,—"He that is wise, let him pursue some desire or other: for he that doeth not affect some one thing in chief, unto him all things are distaste-

ful and tedious." We will not spoil this little volume by giving any account of it. Let our readers get it, and read it. The extracts from his Thesis, "*De Mentis Exercitatione et Felicitate exinde derivanda*," are very curious—showing the native vigour and bent of his mind, and showing also, at once the identity and the growth of his thoughts during the lapse of thirty-three years.

We give the last paragraph, the sense and the filial affection of which are alike admirable. Having mentioned to his hearers that they saw in himself, a living illustration of the truth of his position, that happiness is a necessary result of knowledge and work, he thus concludes:—

"If you would further desire to know to what besides I am chiefly indebted for so enviable a lot, I would say:—1st. Because I had the good fortune to come into the world with a healthful frame, and with a sanguine temperament. 2d. Because I had no patrimony, and was therefore obliged to trust to my own exertions for a livelihood. 3. Because I was born in a land where instruction is greatly prized and readily accessible. 4th. Because I was brought up to a profession which not only compelled mental exercise, but supplied for its use materials of the most delightful and varied kind. *And lastly and principally, because the good man to whom I owe my existence, had the foresight to know what would be best for his children. He had the wisdom, and the courage, and the exceeding love, to bestow all that could be spared of his worldly means, to purchase for his sons, that which is beyond price,* EDUCATION; well-judging that the means so expended, if hoarded for future use, would be, if not valueless, certainly evanescent, while the precious treasure for which they were exchanged, a cultivated and instructed mind, would not only last through life, but might be the fruitful source of treasures far more precious than itself. So equipped, he sent them forth into the world to fight Life's battle, leaving the issue in the hand of God; confident, however, that though they might fail to achieve renown or to conquer Fortune, they possessed *that* which, if rightly used, could win for them the yet higher prize of HAPPINESS."

J. B.

On a New and Successful Treatment for Febrile and other Diseases, through the Medium of the Cutaneous Surface. By WILLIAM TAYLOR, M.R.C.S.E., &c. 8vo. London: 1850.

MR TAYLOR'S new and successful treatment consists in rubbing grease into the skin till it is saturated with it. He was led to discover this method of cure in the following way:—

"As far back as 1829, I had repeatedly observed that persons admitted into the receiving ward at the workhouse, for the purpose of being cured of itch, were sometimes also suffering from fever, in its different forms and stages. The treatment usually adopted was the warm bath, and the compound sulphur ointment; and when the patients were cured of the itch, I generally found, where the rubbing had not produced excoriation on the skin, that the fever also had frequently disappeared. An alterative, with saline medicine, which was usually administered, had the credit of curing the fever, although in my own mind I was soon convinced that it was removed by other measures.

"Much time passed away; numerous cases came under observation; but I was still unable to find out the agent to which the benefit might truly be

ascribed. At length, however, a case was admitted, where recovery took place without any medicine being administered, and then the frictions fixed my attention. I at once proceeded to make trials with a variety of greasy substances in the hot stage of fever, each substance being used separately. From these experiments I concluded that lard was the ingredient chiefly worthy of notice."—Pp. 3, 4.

Mr Taylor, however, does not seem to have been satisfied with the lard, and accordingly "set to work to ascertain what form of ointment was most likely to be efficacious."

"A variety of further experiments were accordingly made; proceeding from oil at the one extremity to wax at the other, and attempting to 'imitate the secretions of the skin,' I by turns resorted to the ceratum cetacei, to prepared lard, to suet (both mutton and beef), but I eventually mixed lard and suet, in equal proportions, melted them carefully over a slow fire (or a water-bath), and this combination, having acquired about the consistence of common tallow, was, on extensive trial, found to answer every requisite, no mixed ointment that liquifies at a temperature below that of the blood being equal to it."—Pp. 6, 7.

This application is designated throughout the book as the "hard ointment."

The rules which our author lays down for its application do not appear to us to be very accurately established,—at all events they are not very precisely expressed; for, at page 9, we are told—

"With regard to the mode of using the ointment in public institutions, I should state that the friction ought to be made with vigour and energy; and here I met with serious obstacles; for the nurses are often old people, for the most part unfit for, or unequal to, the rubbing process."

Whilst, at page 12, it appears that—

"It should be done freely but gently, during from half an hour to an hour at a time,—indeed, until the skin is saturated; when, from being harsh, hot, and dry, like washed leather, it becomes soft, and, for the most part, after a few applications, yields the feeling of velvet."

There certainly is some discrepancy here; perhaps our author may be fairly entitled to reply, that vigour and gentleness are not incompatible in a process of friction; but, from his constantly insisting on the importance of patients being "well rubbed," we are led to infer that the vigorous is, in his estimation, a more important element in the manipulation than the gentle.

Our readers will easily believe the statement made by Mr Taylor in his introduction, that he had very great difficulties to encounter in establishing it in practice:—

"The treatment to some was disagreeable; patients objected to rubbing—they disliked the greasy applications—it was something new."—P. 11.

That all this, however, was mere prejudice, and that when fairly tried it is anything but disagreeable, will appear from the following passage from page 117:—

"The sense of comfort imparted by the hard ointment is remarkable. The expression of this patient, in his illness, was,—'It feels like a coat of mail on you.'"

De gustibus non disputandum est. We should have thought a

coat of mail one of the very last articles of dress to be quoted as a type of remarkable comfort; but we must inform our readers that the patient who uses this comparison is a drayman of Barclay and Perkins,—one of those warlike gentry whose deeds of prowess are now a subject of correspondence between the Foreign Office and the Court of Vienna.

It is to communicate the results of this practice, the nature of which will be gathered from the foregoing extracts, that Mr Taylor has published this book. He tells us that he would willingly have done so long ago, but for his aversion to the responsibilities of authorship, and his disinclination to thrust himself on the notice of the public. He has, however, now been led to do so partly by the solicitations of his friends, and especially by the appearance of a work by Dr Schneeman, of Hanover, on the treatment of scarlatina by a similar practice, and which made him anxious to show that Dr Schneeman's plan had been pursued by him for twelve years previously in this country. We willingly concede to Mr Taylor his "*réclamation de priorité*;" we also concede to him, with equal readiness, our belief that he is actuated by no other motive than a sincere desire to communicate what he conceives to be useful knowledge to his professional brethren, and that his book is not of that advertising character which so often distinguishes little works of this kind. But here our commendations end. As a contribution to medical science the book is worth nothing; the author not yet having attained to that most rudimentary point in therapeutics, the faculty of distinguishing the *post* from the *propter*. He continues to treat cases of disease with remedies of established activity at least, if not of established utility; to these he superadds this greasy process (which, by the way, from some strange whim, he calls "cutaneous induction"), and ascribes the whole benefit derived in the case to the "hard ointment." It never seems to have occurred to him, that either his cures must not all be due to his "induction," as he would have us suppose, or if they are, that he has all the while been needlessly cramming his patients with physic which was of no use to them.

A brief analysis of Mr Taylor's cases will show how completely his activity as a prescriber of physic has put it out of the power of him, or any one else, to judge as to what share his grease could have in his cures.

We have nine cases of inflammatory fever, in every one of which repeated doses of calomel and saline antimonial mixture are given.

Fourteen cases of typhus (?), all treated with calomel and saline febrifuges, with a single exception where the patient refused to take the medicines, and got well under the ointment alone. This is the only case approximating towards evidence in favour of his plan of treatment; and even if we did believe that the recovery was *propter* the "induction," and not "*post*" it, we could not forget that one swallow does not make a summer.

Six cases of scarlatina, all treated also with the invariable calomel and antimonial mixture.

Measles, stated generally to be successfully treated by it, without any detail of cases, a statement true of most kinds of treatment of measles, which in general needs little treatment at all.

Nine cases of dropsy, all of which got at least squill and blue pill, some digitalis, some pyrola, and some a grain of elaterium every second night. A tolerably heroic practice this last, if the elaterium be of good quality; and yet all these cases are quoted to show the good effects of rubbing the patients with the hard ointment.

We have said enough, we think, to show that Mr Taylor has mounted a hobby, and has been run away with by his charger. We need not, therefore, follow him through his chapters, in which he narrates cures of phthisis, hydrocephalus, and insanity by his anointing process.

We have not seen Dr Schneeman's original work, which was the exciting cause of Mr Taylor going into print, but we saw an abstract of it in a contemporary. We did not find in this any evidence of the greasing plan being superior to the warm bath, by which we have been so long accustomed in this country to act on the skin in scarlatina, and we avow that our confidence in the Hanoverian doctor's statements was very much shaken by his declaration, that it "materially shortens the duration of the disease, and checks all infection at the end of the third or fourth day," inasmuch as we affirm that no man living can pretend to determine within one or two days when scarlatina, or any similar disease, begins to be contagious, and when it ceases to be so.

Part Third.

CLINICAL REPORTS, LECTURES, ETC.

CLINICAL MEDICINE.—PROFESSOR BENNETT.

THE MICROSCOPE AS A MEANS OF DIAGNOSIS.

(*Continued from last Vol., p. 552.*)

We have next to speak of the optical parts of microscopes, which are certainly much more important than the mechanical ones,—everything depending upon obtaining a clear and distinct image of the object examined. Under this head we may describe the objective, the eye-piece, and methods of illumination.

1. *The Objective, or series of Achromatic Lenses*, is that part of the optical portion of a microscope which is placed at the bottom of the tube or body, and is near the object to be examined. This may be considered the most important part of the instrument, and the greatest pains have been taken by all opticians in the manufacture of good lenses. It is here I consider that the

London opticians are pre-eminent, for I am not aware that in any part of the world such perfect objectives have been manufactured as the eighth of an inch by Smith, the twelfth of an inch by Ross, and the sixteenth of an inch by Powell. But when we come down to the one-fourth of an inch, which is by far the most useful objective for anatomical and medical purposes, the superiority of the London opticians is very slight, if any. At this magnifying power the compound lenses of C. Chevalier, Oberhaeuser, Brunner, and Nachet of Paris, Schiek and Pistor of Berlin, Frauenhofer of Munich, and Ploesl of Vienna, may be employed with the greatest confidence, and it may be said that by far the largest number of important discoveries in science have been made through their employment. The Parisian lenses, in addition, have one great advantage, namely, their cheapness.

The London opticians have succeeded in combining the lenses of their objectives, so as to obtain a large field of vision, with as little loss of light as possible. These qualities are valuable in the lower magnifying lenses during the examination of opaque objects, and in the higher objectives when observing transparent objects by transmitted light. But in the lenses of medium power, such as the one-fourth of an inch, the amount of light is so great as to be almost a defect. Notwithstanding careful management of the mirror and diaphragm, the field of vision is often dazzling, and always presents a glare most detrimental to the eyes of the observer. I cannot employ Ross's fourth of an inch for fifteen minutes without feeling intense headach, and I know of more than one excellent observer in whom the sight has so much suffered from this cause as to incapacitate them from continuing their researches. In the same manner, the lenses of Brunner and Nachet give rise to a yellow light highly disagreeable; while those of Oberhaeuser, Schiek and Pistor, and Frauenhofer, (with Ploesol's I am not familiar) present a pale blue light, most pleasant to work with, and which may be gazed at for hours without fatiguing the eye.

For the above reasons, as well as from considerable experience in the use of many kinds of microscopes by different manufacturers, I am satisfied that the best lens you can employ for ordinary purposes is Oberhaeuser's No. 7, which corresponds to what is called in England the quarter of an inch. For low powers you may have Oberhaeuser's No. 3, or the one inch lens of the London opticians. For all the purposes of the medical man these will be sufficient. The anatomist may occasionally require a higher lens, as during the examination of the ultimate fibrillæ of muscle, when the eighth, twelfth, or sixteenth of an inch of the London opticians may be procured. All these lenses may be attached to the model we have recommended by means of a brass screw made on purpose.

2. *The Eye-Piece.*—This is that portion of the optical apparatus which is placed at the upper end of the tube or body, and is near the eye of the observer. While the objective magnifies the object itself, the eye-piece only magnifies the image transmitted from below. Hence, as a source of magnifying power, it is inferior to the lens; and when this possesses any defects, these are enlarged by the eye-piece. Two eye-pieces are all that is necessary with the model I have recommended, and those of Oberhaeuser, called Nos. 3 and 4, are the most useful for the medical man.

3. *Methods of Illumination.*—There are few things of more importance to the practical histologist than the mode of illumination. This is accomplished, —1st, by transmitted light; 2d, by reflected light; and, 3d, by achromatic light.

Transmitted light is obtained by means of a mirror placed below the object, which, to be seen, must therefore be transparent. In large microscopes mirrors are provided with universal joints, so that they may easily be directed in any direction. Below the stage every microscope should possess a diaphragm pierced with several sized holes, whereby the amount of light furnished by the mirror may be moderated. It is also useful in the examination of many objects that the light should be directed upon them sideways; this

may be done by the diaphragm, or by the mirror, and, in the small model formerly figured, is admirably attained by simply turning the whole microscope. The best light for microscopic purposes is that obtained by catching the rays which are reflected from a white cloud. The conjoined use of the mirror and diaphragm can only be learned from actual experience.

Reflected light is employed in the examination of opaque objects, and the lenses of low power, manufactured by the principal London opticians, enable us to do so without assistance. Occasionally, however, the light of the sun is useful; and when this cannot be obtained, the rays of a lamp or gas light, concentrated by a bull's-eye lens, may be employed. Hence every microscope should be possessed of such a lens, and it is most convenient to have it attached to the body of the instrument by a moveable ring, and stem with two joints, as in the model figured. Achromatic light is only useful in the examination of very delicate objects, with high powers. The apparatus necessary for obtaining it is occasionally useful in ascertaining the ultimate structure of muscle, or the nature of the markings on minute scales or fossils, but is useless for the purposes of the medical man. In the same way I know of no benefit to be obtained by a polarising apparatus.

In addition to the mechanical and optical parts constituting the microscope itself, the box which contains it should possess a convenient place for holding a few slips of glass, a pair of small forceps, a knife, and two needles firmly set in handles. A micrometer to measure objects with is also necessary to those who are making observations with a view to their exact description. No other accessories are necessary.

An excellent microscope of the model previously figured, with two objectives (Nos. 3 and 7), two eye-pieces (Nos. 3 and 4), a neat box with all the accessories necessary (with the exception of a micrometer, which had better be English), may be obtained in Paris for the sum of about 125 francs (L.5), and will cost in Edinburgh, after payment of carriage and duty, about six guineas. And for all the purposes of the medical man, this is amply sufficient.

The defining power of a microscope is generally tested by examining with it a transparent object, with certain fine markings, which can only be rendered clearly visible when the glasses are good. In all such cases it is of course necessary to be familiar with the structure of the test-object in the first instance. If you are not confident on this point, it is better to trust to the judgment of a friend, whose knowledge of histology is ascertained, or place your dependence entirely on a respectable optician. One of the best test-objects for a quarter of an inch lens is a drop of saliva from the mouth. For, if the microscope shows with clearness the epithelial scales, the structure of the salivary globules, their nucleus, and contained molecules, you may be satisfied that the instrument will exhibit all the parts with which, as medical men, you have to do.

On the Employment of the Microscope.

Having then obtained a good instrument, and tested its qualities in the manner described, you should next determine the number of diameters linear the various combinations of glasses magnify. This you may do for yourself with the aid of a micrometer, a pair of compasses, and a measure. The mode of doing this must be taught practically. I cannot explain it here. If it be not in your power to learn for yourself, the optician will give you a table, setting forth the various degrees of magnifying power possessed by the lenses, and different eye-pieces, with the tube up or down. This table should always be referred to during the description of objects, and the amount of magnifying power invariably stated.

The art of demonstrating under the microscope is only to be acquired by long practice, and, like everything requiring practical skill, cannot be learnt from books or systematic lectures. I can only, therefore, give you very general directions on this head.

All that is necessary in examining fluid substances, is to place a drop in the centre of a slip of glass, and letting a smaller and thinner piece of glass fall gently upon it, place it upon the stage under the objective. In this way the fluid substance will be diffused equally over a flat surface, and evaporation is prevented, which would dim the objective. The illumination must now be carefully arranged, and the focus obtained, first by means of the coarse, and then by means of the fine adjustment. It will save much time, in examining structures, to employ always, at one sitting, the same slips of glass, as it is easier to clean these with a towel, after dipping them in water, than to be perpetually shifting the coarse adjustment.

The action of water, acetic acid, and other re-agents on the particles contained in the fluid, may be observed by adding them to another drop before covering with the upper glass; or when this is done, a drop of the re-agent may be placed at the edge of the upper glass, when it will be diffused through the fluid under examination by imbibition.

The mode of demonstrating solid substances will vary according as they are soft or hard, cellular or fibrous, &c. &c. The structure of a soft tissue such as the kidney, skin, cartilage, &c., is determined by making very minute, thin, and transparent slices of it in various directions, by means of a sharp knife or razor. These sections should be laid upon a slip of glass, then covered over, and slightly pressed flat, by means of an upper one. The addition of a drop of water renders the parts more clear, and facilitates the examination, although it should never be forgotten that most cell-structures are thereby enlarged or altered in shape from endosmosis. Acid and other re-agents may be applied in like manner. The double bladed knife of Valentin will enable you to obtain large, thin, and equable sections of such tissues, and thus enable you to see the manner in which the various elements they contain are arranged with regard to each other. Harder tissues, such as wood, horn, indurated cuticle, &c., may be examined by small thin sections, made in the same way. Very dense tissues, such as bone, teeth, shell, &c., require to be cut into thin sections, and afterwards ground down to the necessary thinness. Preparations of this kind are now manufactured on a large scale, and may be obtained at a trifling cost. A cellular parenchymatous structure, such as the liver, may be examined by crushing a minute portion between two glasses. If it be membranous, as the cuticle of plants, epithelial layers, &c., the membrane should be carefully laid flat upon the lower glass, and covered with an upper one. A fibrous structure, such as the areolar, elastic, muscular, and nervous tissues, must be separated by means of needles, and then spread out into a thin layer before examination, with or without water, &c.

The commencing observer should not be discouraged by the difficulties he will have to encounter in dissecting and displaying many tissues. He must remember that the figures he sees published in books are generally either fortunate or very carefully prepared specimens. Practice will soon enable him to obtain the necessary dexterity, and to convince himself of the importance of this mode of inquiry. He should early learn to draw the various objects he sees, before and after the action of re-agents, not only because such copies constitute the best notes he can keep, but because drawing necessitates a more careful and accurate examination of the objects themselves. A note-book and pencil for the purpose should be the invariable accompaniments of every microscope.

How to Observe with a Microscope.

The art of observation is at all times difficult, but is especially so with a microscope, which presents us with forms and structures concerning which we had no previous idea. Rigid and exact observation, therefore, should be methodically cultivated from the first, in order to avoid those errors into which the tyro, when using a microscope, is particularly liable to fall. Thus you should carefully examine the physical properties of the particles and ultimate structures you may see, and not hastily conclude, that you have under observation

so-called pus, tubercle, or cancer-corpuscles, because they were obtained from what was, *a priori*, believed to be pus, tubercle, or cancer. Nothing has been more clearly demonstrated by the progress of histology, than the fact, that the naked sight has confounded different structures together, from a similarity of external appearance, and that the greatest caution is required at all times, but especially in learners, in forming opinions as to the nature of different tissues.

The physical characters which distinguish microscopic objects consist of,—1st, size; 2d, colour; 3d, shape; 4th, edge or border; 5th, transparency; 6th, surface; 7th, contents; and, 8th, effects of re-agents. These we may notice in succession.

1. *Size.*—The size of the minute bodies, fibres, or tubes which are found in the various textures of animals, can only be determined with exactitude by actual measurement. The most ready way of doing this is demonstrating them on a slip of glass, possessing lines ruled on its surface $\frac{1}{100}$th of an inch apart. This is a micrometer. It will be observed, for the most part, that these minute structures vary in diameter, so that when this medium size cannot be determined, the variations in size from the smaller to the larger should be stated. Human blood-globules in a state of health have a pretty general medium size, and these may consequently be taken as a standard with advantage, and bodies may be described as being two, three, or more times larger than this structure.

2. *Colour.*—The colour of various structures varies greatly, and often differs, under the microscope, from what was previously conceived regarding them. Thus the coloured corpuscles of the blood, though commonly called red, are, in point of fact, yellow. Many objects present different colours, according to the mode of illumination,—that is, as the light is reflected from, or transmitted through their substance, as in the case of certain scales of insects, feathers of birds, &c. Colour is often produced, modified, or lost, by re-agents, as when iodine comes in contact with starch corpuscles, when nitric acid is added to the granules of chlorophyle, or chlorine water affects the pigment cells of the choroid, and so on.

3. *Shape.*—Accurate observation of the shape of bodies is very necessary, as many of these are distinguished by this physical property. Thus the human blood globules, presenting a biconcave round disk, are in this respect different from the oval corpuscles of the camelidæ, of birds, reptiles, and fishes. The distinction between round and globular is very necessary to be attended to. Human blood corpuscles are round and flat, but they become globular on the addition of water. Minute structures seen under the microscope may also be likened to the shape of well known objects, such as that of a pear, balloon, kidney, heart, spade, &c. &c.

4. *Edge or Border.*—The edge or border may present peculiarities which are worthy of notice. Thus it may be dark and abrupt on the field of the microscope, or so fine as to be scarcely visible. It may be smooth, irregular, serrated, beaded, &c. &c.

5. *Transparency.*—This physical property varies greatly in the ultimate elements of numerous textures. Some corpuscles are quite diaphanous, others are more or less opaque. The opacity may depend upon corrugation or irregularities on the external surface, or upon contents of different kinds. Some bodies are so opaque as to prevent the transmission of the rays of light, —when they look black by transmitted light, although they be white, seen by reflected light. Others, such as fatty particles and oil globules, refract the rays of light strongly, and present a peculiar luminous appearance.

6. *Surface.*—Many textures, especially laminated ones, present a different structure on the surface from that which exists below. If, then, in the demonstration, these have not been separated, the focal point must be changed by means of the fine adjustment. In this way the capillaries in the web of the frog's foot

may be seen to be covered with an epidermic layer, and the cuticle of certain minute fungi or infusoria to possess peculiar markings. Not unfrequently the fracture of such structures enables us, on examining the broken edge, to distinguish the difference in structure between the surface and the deeper tissues of the tissue under examination.

7. *Contents.*—The contents of those structures which consist of envelopes, as cells, or of various kinds of tubes, are very important. These may consist of included cells or nuclei, granules of different kinds, pigment matter, or crystals. Occasionally their contents present definite moving currents, as in the cells of some vegetables, or trembling rotatory molecular movements, as in the ordinary globules of saliva in the mouth.

8. *Effects of Re-agents.*—These are most important in determining the structure and chemical composition of numerous tissues. Indeed, in the same manner that the anatomist with his knife separates the various layers of a texture he is examining, so the histologist, by the use of re-agents, determines the exact nature and composition of the minute bodies that fall under his inspection. Thus water generally causes cell formations to swell out from endosmose, whilst syrup, gum water and concentrated saline solutions, cause them to collapse from endosmosis. Acetic acid possesses the valuable property of dissolving coagulated albumen, and, in consequence, renders the whole class of albuminous tissues more transparent. Thus, it operates on cell walls, causing them either to dissolve or become so thin as to display their contents more clearly. Æther, on the other hand, and the alkalies, operate on the fatty compounds, causing their solution and disappearance. The mineral acids dissolve most of the mineral constituents that are met with, so that in this way we are enabled to tell with tolerable certainty, at all events, the group of chemical compounds to which any particular structure may be referred.

Any farther instruction I could give you, with regard to the microscope as a means of diagnosis, could only consist of a description of the different tissues in their healthy and diseased conditions. With this view, it is necessary to become acquainted with the structure both of plants and animals, first in their healthy and then in their diseased conditions. This constitutes, as you are aware, a distinct portion of my systematic lectures on the institutes of medicine,—a necessary preliminary branch of education to that of clinical medicine. The subject is also taught *practically* in this university during the summer months; and certainly there are few studies where, owing to the sources of error inherent in the best microscopes, the manual dexterity required, and the difficulties to be overcome, a teacher is more necessary. A phenomenon, which may have puzzled a solitary observer for months, concerning which, even at the termination of that time, he may have arrived at an erroneous conclusion, is, when properly explained, at once understood. Hence why histology has made such progress in Germany, where practical courses of lectures have been given upon it for many years, and where, in consequence, the young anatomist commences his career with a knowledge which would have astonished the older observers. The researches of Remak on the nerves, of Emmert, of the younger Burdach, and some of the greatest discoveries made in anatomy and physiology, have constituted the subject of the theses published by these observers, on taking their degree of doctor in medicine. Several of our own graduates have recently distinguished themselves in a like manner; and it is satisfactory to know that there is an extensive field for original research open to our alumni, the cultivation of which will actually bind them more closely and intimately with their practical studies.

In conclusion, let me repeat what I have formerly published and frequently stated, namely, that you should regard the microscope only as a means to an end; that in itself it is nothing, and can no more confer the power of observing, reflecting, or of advancing knowledge, than a stethoscope can *per se* enable a physician to discover a disease, or a cutting instrument give the judgment and

skill necessary for performing a great operation. We should learn to distinguish between the mechanical means necessary for arriving at truths, and those powers of observation and mental processes which enable us to recognise, compare, and arrange the truths themselves. In short, rather endeavour to observe carefully and reason correctly on the facts presented to you, than waste your time in altering the fashion and improving the physical properties of the means by which facts are ascertained. At the same time, these are absolutely necessary, and perhaps no kind of knowledge has been so much advanced in modern times by the introduction of instruments and physical means of investigation, as that of medicine. These enable the practitioner to extend the limits to which otherwise his senses would be limited, and I claim for the microscope a place beside that of the stethoscope, pleximeter, speculum, probe, &c. I do not say employ one to the exclusion of the other, but be equally dexterous in the use of all. Do not endeavour to gain a miserable reputation as a microscopist, or as a stethoscopist; but by the appropriate application of *every* instrument and means of research, seek to arrive at the most exact diagnosis and knowledge of disease, as to earn for yourselves the title of enlightened medical practitioners.

CLINICAL MIDWIFERY—PROFESSOR SIMPSON.

ON THE DIAGNOSIS OF THE DISEASES OF THE UTERUS AND INTERNAL FEMALE ORGANS.

Among the new arrangements made in the Hospital last year, the Managers, as you are aware, set aside a special ward for the treatment of the Diseases peculiar to Females, and placed that ward under my care. At the request of my Colleagues, I have promised to give, from time to time, a clinical lecture upon the more interesting and important cases of disease which are there submitted to our notice. And I begin these lectures with the consideration of the

DIAGNOSIS OF THE DISEASES OF THE UTERUS AND ITS APPENDAGES.

I commence our studies with this subject, at the request of some of yourselves; and it is the more necessary in this respect, that, in studying these affections, you enter upon a field of practice in which the means of diagnosis are peculiar in various respects, in consequence of the anatomical and other peculiarities of the organs themselves, the diseases of which it is our object to investigate.

But you must permit me to make one or two preliminary remarks. And first of all, I would earnestly beg you to hold this important fact in view—namely, that the diseases of the uterus do not essentially differ, in their principles of pathology and treatment, from the diseases of other individual organs of the body. The uterus is liable, for example, to be the seat of congestion, inflammation, carcinoma, &c., like any other organ of the body which you can name. It may be, and certainly is, more liable than other organs to some morbid states and actions; but what I wish to impress upon you is, that its diseases are generically the same as the diseases of other organs; and the general principles of their treatment are also the same, the special modifications of management which they require not being greater than the special modifications that must be attended to in applying any general principles of cure to any other individual organ, or set of organs. You treat and cure, for example, an attack of acute inflammation of the uterus, or a chronic inflammatory ulcer of the cervix of the organ, as you would treat an acute inflammation or a chronic inflammatory ulcer of the cornea, or of the pharynx, or larynx, or of any other part of the body,—with this difference, that you can apply local antiphlogistic and other curative measures much more readily and easily to the uterus than to many other more internal organs. In fact, you may, if you deemed it necessary, apply lotions and dressings to an ulcer, for example, of the cervix uteri, with as much certainty and precision as if you applied them to an ulcer

of the external surface of the body; you can thus almost reduce the organ, in relation to treatment, to the facilities presented in treatment by an external as compared with an internal organ.

Modern pathology has made great advances in the management of uterine diseases. These advances, however, have not, let me repeat, consisted so much in finding out that the uterus is the seat of any new, or any peculiar, affections, as by finding out means and methods by which we can detect and diagnosticate the affections that are actually present in it. Here, as elsewhere, after being enabled to discover what its diseases really are, the chief difficulty of the physician is removed; for, as I have stated to you, the same principles of treatment apply to the treatment of specific morbid actions in the uterus as apply to the treatment of the same specific morbid actions in other organs. Indeed, if you will attempt to analyse what the great progress in the medicine of modern times has actually consisted of, you will, I believe, find that it has consisted, not so much in the detection of any new principles of therapeutic treatment, as in the detection of appropriate means for enabling the practitioner to detect and discover in different individual organs what the actual diseased actions are in those organs for which his aid may be required. The treatment, for instance, of inflammation, ulceration, and most other morbid states, was perfectly well known and established hundreds of years ago; but (to take our example from the uterus itself) it was not till of late years that we had certain means and measures of knowing when inflammation and when ulceration, &c. &c., were present in this organ.

The study and practice of medicine has confessedly made, within the last fifty years, enormous advances; but these advances have been advances in pathology and diagnosis, much more than advances in therapeutics. And yet what we still wish and desiderate in actual practice are still greater advances in our diagnostic means; or (to state the same circumstance in other words) at the bedsides of our patients we find that it is in diagnostic means, and not in therapeutic resources, that our existing knowledge is most frequently deficient.

Every medical man is constantly meeting with difficulties in practice, and would often gladly avail himself of an opportunity of resolving these difficulties by appealing to the judgment of some older and more experienced practitioner. Ask the medical practitioner what the nature of the difficulties is which he most frequently encounters in practice. If he will properly analyse the subject, he will confess to you that his difficulties most usually consist in exactly determining what the special disease, or diseased action, in his patient may be; not in determining what he should employ for its cure, provided he were once perfectly certain of its precise nature, and precise seat and extent. The real difficulties of the practitioner do not commonly consist of doubts about the nature and details of the therapeutic measures which he ought to use; but they consist of primary doubts and uncertainties as to what the diseased action is, against which he is to turn his diversified therapeutic resources. They are doubts, not about therapeutics, but about diagnosis.

Since the diseases of the uterus and its appendages have of late years attracted so much more the attention of the profession than they formerly did, one grievous error has been committed in their study and investigation. It is an error which has been committed often in the study and investigation of the diseases of other organs, before we arrived at a sufficiently full and sufficiently comprehensive view of the morbid affections of these organs. The error I allude to is the error of exclusiveness. Formerly many practitioners seemed to look upon all diseases of the uterus as diseases indicative of debility; and they treated almost every one of them with muriate of iron and other chalybeate preparations; sometimes adding, where there was any discharge, the local employment of astringent or tonic injections. I fear that even yet you will find some old practitioners treating their uterine cases upon this sole principle. Then a second set of pathologists would (if we may judge from their

writings) seem to suppose that all diseases of the uterus are marked by, if they do not consist of, congestion and engorgement of blood; and that they are to be remedied by the remedies applicable to this state. A third set, again, look upon the general run of uterine cases as almost invariably inflammatory in their nature, and imagine that we are sure, or almost sure, to find in every case inflammation, or some of its results, as ulceration, purulent discharge, &c. A fourth set would seem to fancy all uterine ailments to be produced by some mechanical displacement or dislocation of the uterus, and to consist of prolapsus, versions, and flexions of this organ upon itself, and upon the neighbouring parts. Again, there are some practitioners—one, in particular, in immense practice in America—who would appear to believe that the affections of the uterus were fundamentally nervous or neuralgic disorders, and that they were always to be treated by the local inunction upon the cervix uteri, of morphia, aconite, and similar sedatives. Another section of pathologists imagine the so-called uterine diseases are, after all, not uterine, but ovarian; and that ovarian irritation and inflammation is actually the source and origin of much of the suffering that is imagined to be uterine in its seat. Lastly (for it is needless to extend this tedious enumeration), you will find another set enjoying the belief, that these supposed uterine or ovarian diseases are not at all uterine or ovarian in their origin, but, in reality, diseases of the general system; they may not deny that local affections do occur in the uterus and ovaries, but these local affections are, in their opinion, results and effects of some more general constitutional disease of the nervous system, or of the economy at large.

All of these views are correct in one respect, that they are all founded partially (though only very partially) upon truth. For, no doubt, the uterus is in practice seen to be liable to one and to all of these morbid states. Thus it is liable to congestion, to inflammation and its consequences, to displacement, to neuralgia, &c.; and the ovary is liable, also, to ovaritis, &c. But all these opinions are excessively unsound and excessively wrong in another point of view,—viz., from their having been in practice pushed, by those who have adopted them, to such an extreme and exclusive length, as to amount, in some instances, to a practical fallacy and a practical error of the greatest possible magnitude. It shall be my earnest endeavour to prove to you that, in relation to the uterine as in relation to other organs, all such extreme and exclusive views are ever erroneous views. The diseases of the uterus are not always of one kind and of one character; they are of many different kinds and different characters, and require, in consequence, many different modes of treatment; and we would place ourselves under the most foolish and fatal fallacy, if we supposed that there was only one series of morbid actions liable to go on in the uterus and its appendages, and only one method of treatment applicable to all their diseased states.

No doubt all such errors in pathology and practice, as I have just now adverted to, will gradually die away as medical men become more intimately acquainted with uterine affections; with their pathology and treatment; and, above all, as they become more intimate with the proper means of making a diagnosis of the affections of the internal reproductive organs in the human female. Certainly such errors will be best corrected in each of you, by each of you studying earnestly and carefully, the means of diagnosticating the different morbid states and morbid actions to which these organs are subject. And, as I have already mentioned, it is my main object at present to point out to you by what different means the diagnosis of uterine and ovarian diseases may be effected, and to state to you the general principles of diagnosis which we can apply to their detection and discrimination. In doing so, I shall enter into no historical details; the knowledge I wish to impart to you is that kind of practical knowledge which we constantly require to use and apply at the bed-side, and which we must be able to employ in the daily and hourly exercise of our profession.

In attempting to make a diagnosis of the diseases of the uterus and its appendages, as in attempting to make a diagnosis of the diseases of most internal organs, the physician calls in to his aid two sets of symptoms,—viz., *first*, the Rational, Dynamic, or Functional Symptoms, as they have been termed; and, *secondly*, the Physical or Anatomical Signs and Symptoms. Let us first note the very marked and important difference which exists between these two sets of diagnostic phenomena—since we thus may have the

SYMPTOMS OF UTERINE DISEASE EITHER, 1. DYNAMIC, OR, 2. PHYSICAL.

Under the Dynamic, Rational, or Functional Symptoms are included all those lesions, deviations, or derangements in the functions of the affected organ, in the functions of other organs that sympathise with it, and in the functions and state of the system generally, that can possibly be excited by the disease or diseases that we may happen to be investigating.

On the other hand, the Physical Signs or Symptoms consist of those means of diagnosis, and particularly of those exercises of the senses of touch, sight, hearing, &c., by which we can attain a knowledge of the anatomical structure, form, density, colour, position, &c. &c., of the organ which is diseased. By this latter set of symptoms or signs—the Physical—we attempt, in fact, to read and discover within the living body the actual morbid anatomy of the organ or organs that are affected. We endeavour by them to study and to ascertain, as exactly as possible, upon the living patient, those anatomical lesions and changes which we would expect to detect on the post-mortem examination of that patient, provided the disease proved fatal.

In detecting and diagnosticating the diseases of the different organs and parts of the body, the practical medical man has recourse sometimes more to one of these sets of symptoms, sometimes more to the other. A diagnosis always becomes more and more accurate the more we can conjoin both means of investigation, and draw our deductions from both sources of evidence. Of the two sets, the rational or functional symptoms are the least to be depended upon; or, at all events, our diagnosis is always the less accurate, the less we can take advantage of the physical or anatomical signs.

Surgical diagnosis is, as a whole, more accurate than medical diagnosis. But why is the surgeon more accurate in the discrimination of the affections which he treats than the physician is? Merely because he can, and does, found his diagnostic deductions far more upon physical symptoms; while the physician is of necessity driven to depend more upon the rational or dynamic symptoms. In fact, in making a diagnosis of the diseases submitted to his care,—viz., those placed upon the external surface of the body, the surgeon relies almost entirely upon physical diagnosis alone, because he can handle and examine the diseased parts by direct tactile examination, and by direct visual inspection. Deprive a surgeon of these means of physical diagnosis, and throw him back merely upon the functional or dynamic symptoms that may be present, without allowing him to exercise his sense of touch, or sight, or hearing, in the discrimination of the cases submitted to him, and you will find him as much or more at fault than the physician sometimes is. Take, for example, a case of any disease of the eye, leading on to imperfection and dimness of sight. No surgeon would venture to make a decided diagnosis of what that dimness or blindness was produced by, provided he were allowed to appeal to the dynamic or functional symptoms only. He might be told fully as to the mode and manner in which the blindness came on, as to its varieties under different circumstances, as to its being accompanied or not with pain or lachrymation, &c. &c.; but these and other circumstances would often be inadequate to enable him to decide whether the imperfection of vision arose from an affection of the cornea, or of the iris, or of the lens, or of the retina, or of some other parts; or by what particular affection of these parts it was produced; while an inspection of the diseased eye itself by the surgeon,—that is to say, a glance at the physical symptoms or physical diagnosis—would probably enable him to resolve at once all these

difficulties, and to ascertain determinately and unequivocally what the diseased state of the organ really was.

It is very different with the physician, in reference at least to some classes of affections. For in some he cannot avail himself of any of these measures of physical diagnosis which the surgeon finds to be so absolutely necessary to enable him to come to a correct decision in surgical cases. In affections, for example, of the brain and spinal cord, and of their membranes and component parts, the physician can, with a few rare exceptions, make use only, in his diagnosis, of the dynamic or functional symptoms. It is only these symptoms that he can reach in this class of diseases. For we have no means as yet, and, in all probability, will never have any, in this special class of affections, of ascertaining, in the yet living subject, what the special anatomical lesions are, that may be present in the encephalon or in the spinal canal. We cannot reach and search these cavities and their contents by any modification of hearing, or touch, or sight. We can ascertain the morbid derangements that may take place in them only by studying the changes and derangement of function to which their presence gives rise in the nervous system generally, in the different parts of this system, and in the functions of those organs which are liable to be more directly or indirectly influenced by that system.

In other classes of disease, the physician, however, has fortunately the power of appealing to physical signs as well as to rational symptoms. He can derive his deductions of the diseased states of the organs themselves from both classes of proof; and you will always find that in medicine our practical knowledge of these diseases has progressed proportionally with the amount and extent to which physical diagnosis has been applied in their discrimination. There is no set of organs, for example, the pathology and practical knowledge of which has advanced more in modern times than that of the organs contained within the chest. If you look into any work on the affections of the heart or lungs, written thirty or forty years ago, you will find a strange contrast between its contents and those of any modern work written on the same subject. Formerly, physicians were often in error in regard to their cases, as to what this or that thoracic affection might be; and their true nature was often only accurately detected after death. The modern physician has almost always the power of discovering what the affections of the heart and lungs are, even during the life of the patient; and can trace, usually with great accuracy, their morbid anatomy while the patient is still alive. Mark what this difference between thoracic medicine, thirty or forty years ago, and that department of medicine now, is to be attributed to. It is entirely attributable to the fact, that while in former times physicians were obliged and necessitated to attempt the diagnosis of these diseases by functional symptoms only, they have now the power of aiding and correcting that method of diagnosis by ascertaining a series of physical signs appertaining to almost every disease of the heart and lungs, through the instrumentality of auscultation and percussion. No careful or judicious physician depends now for the diagnosis of these chest affections upon functional symptoms only; but he appeals constantly, in aid of his diagnosis, to the more certain signs which may be derived from the pleximeter and the stethoscope.

I might safely repeat the same remark, and with nearly exactly the same degree of truth, with regard to the affections of the uterus and internal female organs. Formerly (and even in our own days still far too frequently) the physician has been led to suppose that he could make a diagnosis of diseases of the uterus by depending and relying upon functional symptoms only. A little practice will, however, I believe, readily convince you, that in most cases we are necessitated to have recourse to a physical diagnosis in order to obtain anything like an accurate knowledge of the diseases under which our patients are labouring. Do not suppose that, in making this remark, I mean to imply that the functional symptoms are of no value. Nothing could be farther from my belief and conviction. I by no means wish to state that they are of no value; all I mean to state is, that they are not of sufficient value to make a sufficient

distinction between the different individual or special diseases of the uterus and uterine appendages.

And now, having made these too diffuse introductory remarks, let me call your attention to what the *functional* and what the *physical* symptoms of the diseases of the uterus and ovaries really consist of. And first, let us consider

THE RATIONAL OR DYNAMIC SYMPTOMS OF UTERINE DISEASE.

The uterus performs at different times different functions. During menstruation its lining membrane forms and exudes the so-called menstrual fluid. In the intervals between menstruation we have merely a mucous secretion flowing from the same tissue. And again, it performs from time to time a variety of important functions connected with conception and pregnancy. Under different states of disease, one or more or all of these functions may become perverted. In other words, the first division of dynamical symptoms liable to occur in uterine disease consist of—

I. DERANGEMENTS IN THE FUNCTIONS AND VITAL CONDITION OF THE UTERUS ITSELF.—Thus the function of menstruation may become irregular in regard to the time of its occurrence, or the duration of its appearance;—or the quantity of the menstrual fluid which is thrown off;—or the nature of that fluid may vary; and it may or it may not be accompanied by pain and suffering;—and pain, if present, may be constant, or it may be spasmodic or paroxysmal. The mucous secretion may be increased or diminished in its quantity or quality, or it may become occasionally or constantly mixed with blood or pus. The function of conception may be interfered with, so that sterility is the result; or the uterus may not have the power of carrying the fœtus beyond the second, fourth, or sixth month, the patient being subjected to a series of abortions or premature labours.

The natural states of sensation of the uterus and its appendages may also be altered and perverted. They may be decreased, but this is rare. Far more frequently they are increased to a degree amounting to actual pain; and the pain may appear under very different modifications. It may be continuous or intermittent; it may be dull and aching, or pulsative, or lancinating, and extremely severe. Often, when it is present, it is accompanied with a feeling of heat and tension; and far more frequently still, with a feeling of weight and bearing down. Almost all these pains, resulting from diseases of the uterus, or its appendages, are diminished by the reclining position, and increased by two circumstances,—by a long continuance in the erect posture, and by the active and congested state of the organs, which accompanies the secretion and elimination of the catamenial discharge. Most of the so-called local pains of the uterus are not situated in the position of the uterus itself, or at least in the central part of the pelvis; but the patient more frequently complains of them as located in one or other of the sides of the pelvis; most generally in the left side, in consequence, no doubt, of the left side of the uterus and left broad ligament and ovary being more liable to be irritated than the right, by the varying condition of the rectum, which enters the pelvis towards the left; and the distensions and accumulations in this portion of the intestinal canal are certainly often the more immediate exciting causes of uterine and pelvic pains, if the uterus and its appendages are tender and predisposed to suffer pain, in consequence of any diseased condition existing in them.

At present it would be out of place for me to dwell upon the irregularities and deviations which we meet with in practice in these functional symptoms of the uterus in uterine diseases generally, and in different cases of the same disease. But let me make this one remark as a caution to you (I shall insist upon it more afterwards), viz.—that identically the same pathological affection does not always affect identically in the same way the functions of the uterus. You may have, for instance, fibrous tumours in the walls of the uterus (one of the most frequent organic diseases in this organ), producing, as local functional

symptoms, increased menstruation, or menorrhagia;—increased mucous secretion, or leucorrhœa;—pain during menstruation, or dysmenorrhœa; and, in the intervals between the menstruations, constant uterine pains and bearing down, and irritation of the bladder and surrounding parts. But in other cases of exactly the same organic disease, more or fewer, or indeed all of these symptoms, may be entirely wanting.

Anatomists tell us that the cerebro-spinal nerves which come to the uterus are principally distributed to the cervix uteri;—and hence it has been considered by some pathologists, that the diseases of this part of the organ ought to be accompanied with pain, although the maladies of the body and fundus of the organ may not necessarily give rise to this symptom. But the results of practice by no means bear out this theory. The two most common affections of the cervix uteri are,—1. Chronic inflammation and its consequences—hypertrophy, ulceration, &c.; and, 2. Carcinoma of this part. In practice, cases of both of these diseases from time to time occur, in which, though the disease has progressed far, yet the patient has not been made aware of its presence by any increased tenderness or pain in the part sufficient, at least, to attract her attention; and it is frequently very difficult to persuade the patient—nay, sometimes difficult to persuade the medical attendant—that such serious affections are present, because they incorrectly believe and argue that such affections could not exist without the accompaniments of local pain and suffering.

II. Dynamic Symptoms in other neighbouring Pelvic Organs.—Dynamic symptoms often exist, in connection with uterine disease, not in the uterus or its appendages, but in the parts and organs in their more immediate vicinity. There may be pains, for example, about the bladder or rectum; about the coccyx or sacrum; in the groins; or along the crest of the ilium, and (what is exceedingly frequent) down the limbs, along the course of the crural or sciatic nerves. Pains running from the lower lumbar region, or from the groins, down along the limbs, are felt, and often felt severely, by females even during common menstruation; and intermittent pains in the lower part of the abdomen, or uterine colics (as they have been termed), are common under the same circumstances, as well as in different varieties of morbid uterine irritation. The functions of the rectum or bladder may be interfered with. Defecation may be difficult or painful. Often in uterine diseases there is constipation; and sometimes a want of power to expel the feculent matter, as if there was a kind of paralysis of the rectum; this last occurs not unfrequently in cases of retroversion of the unimpregnated uterus. In many cases the functions of the bladder are more or less affected. Micturition may be too frequent,—a symptom that is common to many uterine diseases; or there may be great dysuria, as is often seen in cases of pelvic cellulitis, &c.; or there may be complete retention, as from the pressure of uterine tumours; or the result may be the incontinence and impossibility of retaining the urine.

These various pelvic symptoms are, in some instances, mechanical in their pathology; in other instances they are sympathetic results. In other words, they are occasionally the direct result of the physical pressure of the enlarged or displaced uterus upon the rectum, or bladder, or trunks of the affected nerves. But, again, they may be present, and equally strongly marked, although the uterus is not unnatural in size or unnatural in position; and, consequently, when the origin of these symptoms cannot be mechanical. In these last instances (and perhaps they are the more numerous class), we speak of the resulting disturbances of function and increased sensations, as sympathetic or reflex,—that is, as originating in an irritation primarily seated in the uterus and uterine nerves, but terminating and felt, secondarily, in the sensitive nerves of those other neighbouring parts which are affected.

This last remark leads me next to show you, that, in diseases of the uterus and its appendages, we have often the principal pains and sufferings of the

patient not located in the uterus or in the pelvis itself, but located in other parts of the body; or we have a third series of dynamic symptoms, consisting of,—

III. SYMPATHETIC PAINS IN DIFFERENT AND DISTANT PARTS OF THE BODY.—This important class of reflex sympathetic pains or neuralgias is often so marked and severe as to draw away the attention of the patient, and even of the practitioner, from the real nature and real seat of the original and primary malady.

Of late years, much important physiological and pathological information has been laid before the profession regarding what are termed reflex motions. You are all well acquainted with the nature and mechanism of the motions that are designated under that term. You all know that they are muscular movements created by excitation upon distant mucous and cutaneous surfaces, the impression made upon these surfaces being conveyed by a nervous arc (which arc always passes through the spinal cord), to those muscles that are ultimately set into contraction by it. The surface or part primarily irritated and excited is often, in this way, far removed from the muscles or sets of muscles secondarily called into action. I believe that often morbid sensations originate in the same way; and that you can speak as truly of reflex sensations as of reflex motions. In your surgical studies you have all studied one form of reflex sensations,—viz., the severe morbid pain in the knee-joint, which is so frequently found to accompany the early stage of morbus coxarius. It is impossible to explain, upon any mechanical principle, why a patient, labouring under disease of the hip-joint, should have the principal pain in the knee-joint. It is not explicable by reference to any amount of swelling in or around the diseased hip-joint, compressing the trunk of those nerves that supply the knee. In fact, the sympathetic or reflex pain in the knee occurs most generally in the very commencement of the disease, when the lining membrane merely of the hip-joint is as yet affected, and when there is as yet little or no swelling or effusion. An impression, or excitation, is conveyed from this affected membrane by the nerves which are distributed to it, and this impression is probably conveyed by them upwards, as high as the spinal cord; and then another impression would seem to be reflected or conveyed downwards from the cord along another set of nerves,—viz., along those the branches of which are ultimately distributed to the knee-joint, the immediate seat of the so-called sympathetic pain. Or, perhaps, we may state the matter more correctly, if we say, that the trunk of the nerve going to the knee, has a morbid sensitive impression made upon it somewhere during its course, as it passes through one of its higher plexuses, or at its origin in the cord itself, producing a change in it as if its own branches in the knee were painfully affected. Irritation of the mucous membrane of the bladder (to take another example) often gives rise to sympathetic or reflex pains in the orifice of the urethra, and along the course of the lower extremities. I have seen several cases where the small, red, granular, and very painful sensitive tumour which sometimes grows at the orifice of the female urethra, was accompanied with distressing pain in the heel and sole of the foot. But it is needless to go to other organs in search of such examples; the uterus itself affords us many marked instances, even when it is not diseased. Thus, when the uterus is distended and irritated in pregnancy, the patient often complains of reflex or sympathetic pains in the lumbar region, in the mammæ, in the nerves of the teeth, &c. When the uterus is the seat of actual disease, such reflex pains are, as I have just now stated, often very marked; and, at the same time, they are very apt to deceive and mask the original and primary disease. Some years ago, I saw a case, with Dr Johnston, of Berwick, which made a strong impression upon my mind, in relation to this question. The lady had been under the care of an eminent London accoucheur, and complained to him principally of pain in the mammæ, for which he had ordered, at different times, leeches, and a sufficiently varied course of sedative local applications, and numerous internal remedies. But he had not once examined

into the condition of the uterus, which was the source of the sympathetic pain in the mammæ. She was suffering, in fact, not from any diseased state of the mammæ (the organ which the physician was so assiduously treating), but she was suffering under a slow corroding carcinoma of the cervix uteri. At the time I saw her, the disease had already destroyed almost the whole tissue of the cervix, leaving the remainder of its excavated walls hard, rough, and indurated, like the interior of a dice-box. The dependence of the reflex or sympathetic pains seen in uterine cases upon the uterine irritation itself, as their primary source, is best seen in cases of displacement. By restoring a displaced uterus to its natural position, even though only temporarily, you generally at once moderate or entirely remove the attendant sympathetic effects. We cannot have the same precise proof in other uterine diseases, as ulcer, cancer, &c., because we cannot make the experiment of at once removing the primary irritation.

The reflex or sympathetic pains under which patients suffering from uterine diseases are liable to complain, may be situated in very different parts of the body. I shall enumerate the principal of them, and that, as far as possible, in the order of their frequency.

Pain in one or both mammæ (such as occurred in the case I have just now alluded to) is very common in many forms of uterine irritation or disease; but is seldom so very severe as it was in that instance. It is often accompanied by some degree of swelling or tension in the gland; and occasionally the areola becomes somewhat darker and changed, as in pregnancy. These mammary symptoms are particularly liable to be increased at the period of menstruation. If a patient complains to you of pain in this part of the body, without your being able to trace any local affection of the mamma itself capable of creating it, always make the necessary inquiries and investigations, in order to know whether some congestive, or inflammatory, or other diseased action, is not going on in the uterus.

Pain under the left mamma, and upon the edges of the ribs of that side, is also very frequent, perhaps even more so than pain in the mamma itself. It has sometimes appeared to me to be more common, in cases of uterine or ovarian disease, occurring in the unmarried than in the married. It is probably as frequent in cases of uterine affections, as pain in the shoulder is in cases of hepatic affections. This pain under the left mamma is sometimes diffused along the side; but more usually it is limited, and the patient will tell you that she thinks she could cover the whole of the pained spot with a crown piece. When it is not severe, it is not affected by the act of respiration; but in more severe cases, it is increased by deep inspiration; and many a poor patient has been bled and leeched, &c., over and over again, under the idea that this pain was an indication of pleuritic inflammation, when, in fact, it had nothing to do with pleurisy, or any disease of the chest; but was merely a sympathetic or reflex pain, and one entirely resulting from uterine irritation.

Pains in the right side, or right hypochondriac region, are much more rare, but are still sometimes also to be met with.

Pain in some of the vertebræ of the back, imitating spinal irritation, is often also an accompaniment of uterine irritation, and is not unfrequently combined with the existence of the pain under the left mamma. The patient principally complains of it when the part is pressed. Many a patient suffering from these sympathetic vertebral pains, in consequence of uterine derangement or disease, has been laid up and counter-irritated with the cautery, moxa, &c., under the idea that there was actual spinal disease present.

Pain in the lumbar region, or in the sacral or lumbar region combined, is especially common, as a sympathetic morbid sensation in uterine diseases, but particularly in those which consist of any displacement or enlargement of the organ. The great sacro-lumbar pain which the woman in labour suffers in the same part, is itself entirely a reflex or sympathetic pain of the character that we are speaking of.

Pains in the abdominal parietes also occasionally occur. Sometimes they are limited to one or other side, occupying the site of the ascending or of the descending colon. Occasionally they are confined to a limited spot. But in other instances we find the whole surface of the abdominal walls tender and neuralgic. And when this is the case, there is often co-existing a greater or lesser degree also of tympanitis—a kind of combination which is very apt to deceive the young practitioner into the idea, that the disease of his patient is really more formidable than it actually is. For the combination may lead him to suppose, that he has to do with a case of acute peritonitis, or (if it is of longer standing) with a case of serious enlargement and organic disease of the uterus, or ovary, or some abdominal organ. In some cases, the apparent tenderness is so great that the patient timidly shrinks under the least touch, and is even terrified at the approach and pressure of the hand of the practitioner.

Pains in the extremities and joints are sometimes secondary morbid results when the primary morbid irritation or disease is seated in the uterus. I have already stated to you that pain, stretching from the loins or pelvis downwards, along one or both of the lower extremities, is a very common symptom indeed in uterine diseases. In ovarian enlargement there is sometimes (particularly in the earlier stages of the disease) a morbid sensation of numbness and prickling in the corresponding limb; but this effect is probably mechanical, the consequence, that is to say, of pressure upon the nerves of the limb, and not true reflex or sympathetic results. I had occasion also to mention, the occasional excitation of reflex morbid pain in the heel and foot, by the small sensitive vascular tumour that occasionally grows in and around the orifice of the female urethra. Other forms of irritation in the urinary canals may excite morbid pains in the same distant part; and I have met with them also, but more rarely, in uterine cases, in which I could not trace any primary pathological disease in the kidneys, bladder, or urethra. Under the name of hysterical knee-joint, surgical authors describe a painful affection of that joint, occasionally accompanied by swelling and apparent effusion, and which has often been mistaken for the common white swelling, or scrofulous disease, of the same part of the body. The pain and other symptoms are so chronic and so marked, as sometimes to have led the surgeon to amputate the limb for supposed incurable disease of the joint. I feel well assured that in these cases the pain in the knee is almost always, or, indeed, always, a secondary or reflex pain, and that the primary source of irritation will be found in some other distant organ, and very often, indeed, in the uterus or ovary. Similar pains in the other joints of the extremities seem rarer; but are not altogether unknown. At present I have a patient suffering under chronic inflammatory ulceration of the cervix uteri, and profuse purulent discharge from that part, with much pain and tenderness in the canal of the vagina. But she is suffering still more from severe pain and tenderness of the skin and tissues lying over the head of the right radius. This pain in the arm has sometimes been so excessive as to deprive her of rest; and she herself maintains that it decreases and increases in correspondence with the changing state of the uterine irritation and discharge.

Pains in the face and head may also occur. I have already stated to you, that toothach, as a sympathetic pain, occurs occasionally in patients in the state of pregnancy. I have not had occasion to trace it distinctly, as a mere reflex and sympathetic pain excited by disease of the uterus. Local and limited pains, as over one or other eyebrow in the forehead, in the occiput, in the temple (clavus hystericus), &c., have appeared to me sometimes to be the secondary consequences of uterine irritation. The headachs, however, under which uterine patients so often suffer, are not generally direct secondary sympathetic effects of the uterine affection. The cephalalgia in these cases is almost always a kind of indirect result either of the anemia, or of the deranged digestion and assimilation, which so often come to attend upon chronic uterine affections. For uterine diseases frequently produce other deviations in the functions of the body, than mere painful sensations. They often derange the functions performed by dif-

ferent organs. Let me state to you what phenomena, as rational or dynamic symptoms of uterine disease, we find most frequently referable to such

IV. Derangements of Functions in distant Organs.—We have already considered the urinary bladder as liable to be irritated and deranged in its function in uterine disease—sometimes mechanically, but very frequently sympathetically. Let me merely add on this point, that occasionally the urinary secretion is altered and very various in its character. The irritation of pregnancy is well known to produce changes in the urinary secretion, which have been considered as quite characteristic of pregnancy; sometimes the morbid secretion of albuminous urine occurs; and one or two rare cases have been recorded, in which women have suffered from diabetes when pregnant, and have recovered their usual health completely after delivery. It may yet be found that uterine diseases are also liable occasionally to derange and alter the urinary secretion itself; but the subject, as a source of any rational symptoms of uterine disease, has hitherto attracted no degree of attention. Uterine patients, when questioned on the matter, tell you generally, in relation to it, that the urinary secretion is most variable, and especially that it alters, from time to time, from being very profuse and watery, to being small in quantity and loaded with sediment. This sediment is usually composed of the urates; but I have seen the triple phosphate, oxalates, and other morbid ingredients in the urine, in uterine cases. I am not acquainted, however, with a sufficient number of facts to be able to state to you, whether these occurrences are mere coincidences, or whether, in any case, the derangement in the urinary secretion, and the derangement in the morbid state of the uterus, stand in relation to each other in any respect as cause and effect.

The intestinal canal is an organ more frequently deranged in its functions in uterine disease than, perhaps, any other organ of the body. Usually the functions of it are performed sluggishly and inertly. Constipation is very often present; and the lower portions of the canal especially appear to have lost their healthy tone and power. At the second or third month of pregnancy the abdomen of the human female is often larger than it is when the uterus is actually still more enlarged at the fourth or fifth month. This early enlargement is the result of the derangement of the functions of the bowels, and, more particularly, of the accumulation of gases within them. In the same way a partial tympanitis often accompanies other morbid irritations of the uterus, as well as the irritation of it from pregnancy. Pregnancy is, as you are all aware, not unfrequently accompanied with dyspepsia in the form of gastrodynia, pyrosis, and other morbid symptoms; and nausea and vomiting are very common sympathetic phenomena in the pregnant state. All these symptoms occur also in connection with diseases of the uterus; though more rarely than with pregnancy.

Further, in uterine disease we find another abdominal organ not unfrequently sympathetically affected—I mean the liver. In very many women the biliary secretion becomes disordered at the return of each menstruation,—in some a state of constipation, in others a state of diarrhœa recurring during each menstrual period. The biliary and catamenial secretions seem almost vicarious of each other; and as in other cases in which such physiological relations exist, the two functions are not unfrequently simultaneously deranged in their pathological actions also,—both being occasionally increased or decreased together; or, what oftener happens, one being increased in extent and activity when the other is diminished. In some cases the cure of a uterine disease seems also to rectify the co-existent, and perhaps resultant, hepatic derangement; while, no doubt, also, in other cases we find ourselves altogether unable to amend and arrest uterine diseases and discharges, till we have, in the first instance, used appropriate means to modify and correct the attendant hepatic disorder.

The organs within the chest are more rarely affected. Often, indeed, there is palpitation present, but it is principally in cases where there has been a loss

of blood from uterine disease, or a constant and profuse leucorrheal discharge, weakening and debilitating the patient; or when a deranged state of the digestive functions has led on to the same result. Besides, in prolonged uterine disease, we very often find the mobility of the nervous system greatly increased; and palpitation in all such patients occurs readily under any excitement, and sometimes forms a sufficiently distressing, and, in their own opinion, an alarming symptom.

At first thought, one would scarcely expect to find the respiratory function ever affected sympathetically in uterine disease. I have, however, seen a sufficient number of cases fully to convince me, that there is in reality a true kind of *asthma uterium*, as mentioned by the older nosologists. I saw, for example, within the last few days, a lady whose history is briefly this:—She never menstruated till she was twenty-eight years of age, but every month, or at the period of the menstrual molimen, there was, instead of menstruation, a distinct attack of asthma, lasting generally for two or three days, and sadly shattering and breaking down her constitution. Her state greatly alarmed her brother, who was himself a medical man. I attempted, during several successive months, to induce menstruation, by applying, at the expected periods, nitrate of silver freely to the interior of the uterus, by the usual instrument adapted to that purpose. Some sanguineous discharge was the result, with marked and evident alleviation of the asthmatic symptoms. At last, I introduced a small galvanic pessary into the interior of the uterine cavity, and left it there for several months. After wearing it for a time, the patient menstruated regularly, the asthma entirely disappeared, her health and strength became restored, and, for the first time for many long years, she was able again to join in general society. Menstruation continued regular for some months after the instrument was removed, and the asthma was kept in perfect abeyance; but, on the recurrence of cold weather, she again failed to menstruate, and, instead of it, had one of her old attacks of asthma. At her own earnest suggestion and solicitation, I have, in consequence, again replaced the pessary, to which she herself correctly attributes the new life, as she describes it, which she has of late years gained. Perhaps such a result may appear not so paradoxical as at first sight it might look, when we remember that several German physiologists have insisted, that menstruation is a kind of vicarious respiration, intended to rid the female system of a cumulative amount of carbon. And you know that Andral and Gavarret have proved experimentally, a few years ago, that in the human female there is a kind of direct relation and interchange between the amount of carbon discharged in expiration, and the amount of menstruation present; more carbon being always exhaled from the lungs when the patient was suffering under amenorrhœa, or when menstruation was arrested by pregnancy, than when she is not pregnant, and when she is menstruating regularly.

The nervous system, as I have just now had occasion to state to you, becomes weakened or mobile and supersensitive in most patients suffering under any protracted, and especially under any weakening, form of uterine disease. There is languor, and at the same time excitability of mind observable to a distressing degree in many such patients. There is not the usual capacity for mental exertion. Not a few are morbidly despondent of themselves and of their state. We see, sometimes, sad evidence of the effects of uterine irritation upon the mind and nervous system, in the production of puerperal insanity. In all large asylums you will find cases of insanity combined with, if not resulting from, uterine irritation and disease; and in such institutions the paroxysms of mental excitement are very often seen to be regulated by the monthly recurring irritation in the uterus which exists during the days of menstruation. Occasionally, in cases of uterine disease, there are lesions of the senses, more particularly of the sense of sight, sometimes the vision being impaired, and motes floating before the eyes. The whole muscular system, under such a condition, sometimes becomes debilitated and reduced in tone, the patient being unable to take anything like her usual amount of exercise. You have the same class of patients,

in whom the nervous system is thus weakened and exhausted, occasionally complaining of prickling and numbness of the extremities, as if these parts were losing their power of motion and sensation. And local paralyses seem sometimes, though very rarely, to occur from the irritation of uterine disease, in the same way as they are seen to occur from the irritation of pregnancy. The power of standing and walking is sometimes weakened and abrogated from another cause in uterine ailments,—viz., from the uterus being so much enlarged or displaced, as to produce compression upon the nerves, &c., in the interior of the pelvis. I have seen several cases where this power was restored to the lower extremities, by restoring to its position a uterus that was retroverted.

In pregnancy, the cutaneous surface is sometimes affected sympathetically, ephelis, and other eruptions appearing upon it. In uterine disease the skin is often dry and parched, and in an unhealthy state; and the cutaneous surface of the hands and feet, is particularly liable to be easily chilled, perhaps from imperfect capillary circulation, or from imperfect innervation. I have under my care at present, in private practice, two cases of amenorrhœa, in both of which the face is thickly covered with *acne rosacea*. In one of these the cutaneous eruption has become more than once greatly diminished when the menstrual discharge was restored, by the application of nitrate of silver and dry cupping of the interior of the uterus,—so much diminished, as to impress my mind with the belief, that the eruption itself was a derangement in some way or other the direct or indirect result of the uterine disorder. Some women have, at each menstrual period, a slight erythematic appearance upon the skin of the face.

Some time ago I showed Dr Christison an interesting case, in which there was amenorrhœa co-existing with such a profuse degree of chronic salivation and ulceration of the lips and gums, as at first to have impressed me, as it had impressed other practitioners, with the idea, that she was suffering under the effects of mercurial salivation. My inquiries, however, regarding her quite satisfied me that she could not possibly have been taking any mercury. In what relation the amenorrhœa and salivation stood to each other, in this case, as in other cases, I cannot venture to determine. But you all know that in some women salivation is a very common dynamical symptom in pregnancy. The pregnant patient sometimes loses, in this way, large quantities of saliva daily, up to the very period of delivery.

The thyroid gland, in occasional and rare cases, enlarges in pregnancy. I have seen it enlarged also in uterine cases; but in this, as in others of these complications that I have named, we yet certainly want sufficient evidence to determine the actual relation between these functional derangements and the co-existing uterine disease and irritation. We have yet to study and learn, how far they stand related to each other as simple coincidences, or as regular sequences. And a more careful and complete investigation of them by pathologists, would probably add considerably to the amount and value, of the rational or dynamic symptoms of uterine disease.

In reference to all these morbid pains and morbid derangements of function, which may thus in different cases accompany and be produced by derangement or disease of the uterus or ovaries, let me make one more general remark, viz., that they are all liable to be more or less increased in character and in intensity at the menstrual period, when the uterus and ovaries become congested during the days of the usual menstrual molimen.

Lastly, among the dynamic symptoms produced by uterine disease, I have yet to state to you that these affections sometimes produce, and are accompanied by,

V. States of General Constitutional Derangement.—The states of general constitutional derangement that may be found attendant upon uterine disease vary with the disease itself.

The inflammatory diseases of the uterus are seldom so acute (except in the puerperal state) as to lead to much secondary fever; but inflammation in the

cellular tissue of the pelvis, or Pelvic Cellulitis, frequently leads to much constitutional disturbance; and if the disease is prolonged, and ends, as it often does, in suppuration, then you have very well marked hectic symptoms usually supervening. I have more than once seen the hectic accompanying a pelvic abscess, mistaken for the hectic fever of phthisis.

Where there is much and continued loss of blood from polypus, or cancer, or other causes, you have the results and general effects of anemia and chlorosis superinduced, with their usual accompaniments of headach, palpitation, nervousness, &c.

But you have often chlorosis supervening, apparently paradoxically, under the very opposite set of circumstances, viz., where there is amenorrhœa, and partial or total suppression of the catamenial discharge.

In carcinoma of the uterus, the patient, after the disease begins to break up, usually exhibits the sallow, yellow complexion of organic disease; but it is generally mixed up, also, with pallid lips, gums, and other symptoms of anemia, in consequence of the frequent violence of the accompanying hemorrhages. In most such instances, and in the advanced stages of ovarian dropsy, the general atrophy and emaciation of the patient become at last extremely striking.

But in a large proportion of uterine ailments, especially in those of the slighter description, little, or, indeed, no appreciable amount of constitutional disturbance is produced. Let me merely further add, that when the disease is attended with exhaustion, or if the irritation of it has excited and weakened the nervous system, so as to render it super-sensitive and impressible, hysteria in one or other of its multifarious forms is always apt to supervene.

We come, now, to another question,—one I have already slightly alluded to,—viz.,

WHAT IS THE DIAGNOSTIC VALUE OF THESE VARIOUS FORMS OF DYNAMIC OR RATIONAL SYMPTOMS, IN THE DETECTION AND DISCRIMINATION OF UTERINE DISEASES?

This question is in itself a very important one, and I think we can answer it satisfactorily and decidedly. For I feel assured that no man, who is practically acquainted with the diseases of the uterus, can have any hesitation in declaring, that the dynamic symptoms of which I have spoken, taken either individually or conjointly, are, in the general run of such cases, altogether incompetent and inadequate for the purpose of a perfectly correct and practical diagnosis of the different or specific affections, to which the uterus and uterine appendages are liable. But do not mistake me in what I wish to state to you upon this point.

There are two objects of diagnosis which the medical man is to consider, in relation to every case applying to him for relief,—viz., first, What organ of the body is affected? and secondly, How is that organ affected? In other words, his first duty is, to trace out the seat of the disease, or the organ or organs which are diseased; and his second object is, to determine what special or particular affection of that organ or organs is present in his patient. Now, I do believe that the dynamic symptoms of uterine disease, such as I have described them to you, may enable us in most cases to determine that the uterus is affected. They are, in many cases at least, sufficient to decide us in relation to the first point that I have named; but they are perfectly insufficient, by their own unassisted evidence, to enable us to come to a decision in regard to the second point,—viz., What is the special disease of the uterus, or uterine appendages, under which our patient is suffering? They generally afford us proof enough of the probability of the uterus being affected, without proving to us how it is affected. They may enable us to decide that it is the seat of some morbid state, or some morbid action; without enabling us to determine at all exactly what that morbid state, or that morbid action, may be; and, consequently, without enabling us to decide what should be the proper line of treatment requisite for the relief of our patient.

In treating of this important point,—in endeavouring to show that the

dynamic symptoms are not sufficient to enable a practitioner to make a differential diagnosis among the different diseases of the uterus,—I have often found it difficult to impress deeply enough upon the mind of the student the fact, that there is no practical guiding relation between the kind or amount of uterine disease that may be present, and the character of the secondary dynamic symptoms to which it gives rise; and hence, that he cannot, in practice, depend for the discrimination of the different diseases of the uterus from each other upon the dynamical or functional symptoms. But I think I can adduce, in evidence, a case, with the peculiarities of which you are all sufficiently familiar to enable you to understand the argument, in relation to this question, that may be derivable from it.

The rapidly dilating and developing condition of the uterus during pregnancy, and the state of irritation produced by the ovum enlarging within it, may be regarded as the same condition in all women. Pregnancy is the same disease (if I may so term it) in all. In all cases the vital changes and actions going on in the walls and in the cavity of the uterus are identically the same. Yet mark how very different are the dynamic symptoms accompanying this same condition in different patients. Some patients during pregnancy are long unaware of the condition in which they actually are; they suffer from no kind of complaint,—from no form of distress—no functional derangement, or dynamic symptom. A second set of patients during pregnancy are afflicted with severe local suffering, from irritation, induced by the enlarged organ, in the bladder and the rectum; or they have a feeling of prolapsus and bearing down of the uterus, with pains in the loins, &c. You will find another and a third series, not complaining of any local feeling or distress, but suffering under sickness and nausea,—under general derangement of the stomach and intestinal canal,—under headach and nervous affections,—under toothach or salivation, and other derangements of the sensations and secretions of the body. But, again, in another and in a fourth set of cases, we occasionally find all the most frequent dynamic symptoms, local and constitutional, of pregnancy present, and so well marked, as to impress both the patient, and sometimes even the practitioner, with the certainty of the existence of that state; and yet, after the lapse of a little time, it may be found that the patient is really not pregnant at all; the symptoms being sympathetic merely of some morbid state of the uterus or ovaries, but not sympathetic of the state of pregnancy, though very exactly similar to the symptoms which most commonly accompany it.

Observe, for a moment, what important evidence these well known facts afford. They afford us evidence to this extent,—first, that in pregnancy we have the same identical condition of the uterus, not by any means always accompanied by the same identical dynamical symptoms, these symptoms varying greatly in different patients; and, secondly, they show us that the most usual dynamic symptoms—local, sympathetic, and constitutional—which we see produced by pregnancy, may be present in other conditions of the uterus than in the pregnant condition. But exactly the same two conclusions are true in regard to almost every one of the diseases of the uterus, as well as to its state of pregnancy. In different diseased conditions of the uterus, as in the condition of it in pregnancy, we constantly find the same specific affection of the organ exciting very different phenomena in different women. And, further, we find that apparently phenomena, or dynamic symptoms, which are nearly if not entirely identical, may be excited by two or more diseases of the uterus that are entirely different from each other in their pathology and in their treatment.

I have already alluded to the frequency of fibrous tumours in the walls of the uterus. Let me take them as an example of the remark—though, in fact, uterine pathology is full of similar instances. Fibrous tumours do not, in some patients, produce any very decided dynamic symptoms; and even when

of large size, occasionally they may pass for years altogether undiscovered either by the patient or by the practitioner. Menstruation may be perfectly regular; and pregnancy even may take place. In other patients, however, these fibrous tumours, even when still small in size, sometimes (as I have already stated to you) produce distressing irritation among the pelvic viscera, and derange in various ways the physiological functions of the organ, leading on to menorrhagia, leucorrhœa, dysmenorrhœa, &c. Here, then, we have a disease identical in character in different patients, but producing in these different patients very different kinds of symptoms. And, if it were necessary, I might repeat the same kind of deduction, as derivable from the consideration of the different symptoms attending all other organic diseases of the uterus in different patients, as chronic inflammation, hypertrophy, polypus, cauliflower excrescences, carcinoma, &c. &c. In many cases carcinoma of the uterus produces, from a very early stage of its progress, distressing dynamic symptoms. But in others it marches onward in its fatal course for a long time, without occasioning almost a single dynamic symptom calculated to excite the attention or rouse the alarm of the patient; and when marked dynamic symptoms do at last supervene, how commonly are they mistaken for the symptoms of other and milder diseases of the uterus. I have often, on the other hand, seen cases which have been supposed to be cases of incurable carcinoma, when the disease was some more benign and perfectly curable form of uterine malady, such as inflammatory induration of the cervix, or polypus. In most, if not in all such cases, the error committed has consisted in the practitioner supposing he could make out the differential diagnosis of the disease by dynamic symptoms alone, and without calling in to his aid the evidence to be derived from physical diagnosis.

The most malignant organic diseases of the uterus may, as I have just remarked in relation to carcinoma, long remain occult and latent in their character; they may have marched far on to a fatal termination, without a single dynamic symptom being present calculated to warn the patient to any knowledge of her state of danger. But, on the contrary, we have often severe local and constitutional dynamic symptoms of uterine disease developed, and developed early, in instances of slight and remedial organic affections of the organs, as in simple chronic ulceration, and inflammatory eruptions upon the cervix. And again,—mark this other perplexing fact,—in other instances all, or almost all, these dynamic symptoms may be present, in their most aggravated forms, for months, and even for years, in instances of the so-called irritable uterus, or in neuralgia of the organ,—that is to say, in a set of cases where there is actually no organic disease at all, and where there may be much real distress, but not much real danger.

To sum up these remarks in one more general deduction, let me observe, that we sometimes have the same apparent train of local and secondary dynamic symptoms in neuralgic, in inflammatory, and in benign, and malignant organic diseases of the uterus. And, in whatever way we may explain the fact, it is still an undoubted important pathological and practical fact that, in uterine diseases, there is generally no very direct or fixed relation observable between the intensity and character of the local pathological disease which is present, and the intensity and character of the dynamic symptoms to which that disease gives rise. The accompanying dynamic symptoms may be severe, where the local disease of the uterus is slight and simple; and, on the other hand, it too frequently happens that the local disease of the uterus is severe and serious in its nature, when the dynamic symptoms that accompany it are apparently trivial and transient in their character. It is in consequence of this want of relation between uterine diseases and their dynamic symptoms, that it becomes so requisite in this class of affections to have recourse to physical diagnosis, in order to determine what ascertainable changes have taken place in the structure and organisation of the uterus or its appendages. It is only by instituting, in this way, a physical diagnosis that you can hope, in most instances,

to decide upon the *specific* nature of the uterine disease that may be present. In many cases we may make, as I have already repeatedly stated to you, the general diagnosis of the existence of uterine disease, by the consideration alone of the rational symptoms, or functional derangements, to which such disease gives rise. We can only make *a differential* diagnosis of what the existing specific disease is by physical diagnosis; or, in other words, by the physical examination of the anatomical conditions of the organ itself.

The necessity of physical diagnosis will, I believe, be willingly confessed by all practitioners in relation to the diagnosis of one uterine state, that is not a state of disease. I specially allude to the illustration which I have already brought before you, viz., the state of pregnancy. In deciding upon the existence or non-existence of pregnancy in any cases of doubt or importance, no medical man, who valued his professional reputation, would deem himself justified in offering a final and decided opinion upon the subject, by considering the dynamical symptoms alone,—symptoms, which I have already described to you to be sufficiently varying and equivocal in different patients. No medical man would venture to form a definite deduction and judgment upon the matter until he was allowed to make some physical diagnosis of the condition of the uterus,—that is, until he had ascertained by sight or touch, or both, that the abdomen was really enlarged; until, probably, by a careful external examination of the abdomen by the hand and stethoscope, he had ascertained that the existing tumour was really uterine, and really contained a fœtus; or, in the earlier months, until, perhaps, he was permitted to make a vaginal examination, in order to make out the state of development, size, &c., of the uterus. Exactly in the same way, in deciding upon the pathological nature of any marked uterine disease, and consequently upon the line of treatment which it may require, we believe precisely the same caution to be necessary, and the same local examination to be requisite, where there exists any doubt, and where such a local examination is not otherwise counter-indicated. And in a practical point of view, the examination in a case of uterine disease is far more necessary, because far more important, than in the case of pregnancy. In pregnancy the local examination is had recourse to, in order to determine a point which time itself would soon alone determine. In uterine diseases the same physical examination is had recourse to, for an object of much more immediate and practical moment, viz., to obtain information sufficient to enable us to form a correct and precise judgment as to the specific pathological nature of the case before us, and, consequently, to enable us to select the proper remedial measures for its treatment and cure.

Let us proceed, then, next to inquire,

What are the Means of Physical Diagnosis, that we may Use in Diseases of the Uterus, Ovaries, &c.?

Now, in the diagnosis of these diseases, we require to use the sense of touch, and occasionally of sight, hearing, and smell. We require to use the sense of touch in the external and internal examination of the region of the uterus and of the surrounding parts. We require to use the sense of hearing in the employment of auscultation or percussion. We require to use sight in the use of the speculum. And the nature of the fœtor emitted by the vaginal discharge is sometimes almost enough in itself to decide for or against the existence of extensive carcinomatous ulceration and disintegration.

The os uteri is, as you are aware, an opening so narrow as not to allow of the passage of the finger through it for the examination of the interior of the cavity or walls of the viscus. But it is most important, in some cases, to be able to make an examination of this cavity and of these walls, for the purpose of ascertaining various points with regard to their condition; and in order to do so, we may pass through the os a slender metallic finger, or, in other words, use the uterine sound. Or, if necessary, we may enlarge the size of the opening of the os, so as even to admit the finger itself, by introducing a series of dilating

sponge-tents into the cavity of the os and cervix uteri. Occasionally matters are discharged from the cavity of the uterus, of which it is necessary to ascertain the character and source; and, in some instances, we are aided in coming to a decision upon this matter by the use of the microscope and by chemical tests. Sometimes there are fluid collections in the cellular tissue of the pelvis, or in the ovary; and it may become a matter of life or death to the patient that we should be able to trace what the character of these effusions really is, and, in particular, whether they are inflammatory and purulent or not. We cannot reach them with the finger, but we may reach them with the exploring needle, and in this way trace their true character. Lastly, in cases of neuralgic tenderness of the abdomen and passages, and more especially when these are combined, as they often are, with tympanitic distension and enlargement of the abdomen, giving an appearance of formidable enlargement and very serious disease, it is sometimes impossible to make an accurate examination of the abdominal and pelvic viscera till the patient is fully anæsthetised; and you have seen in the Ward more than one case where the employment of chloroform has, in this way, at once dispelled what previously seemed to be an undoubted and enormous ovarian or uterine tumour.

We have thus, you will observe, a variety of means of physical diagnosis that we may resort to in the detection and discrimination of the diseases of the uterus, and ovaries, and neighbouring parts; and, if we attempt to classify them, we may reduce them to the following order:—

1st, The external or abdominal examination of the patient by touch, auscultation, and percussion.

2dly, The tactile examination of the uterus, ovaries, &c., by the vagina or by the rectum.

3dly, That most important mode of diagnosis,—viz., the simultaneous combination of the external and internal modes of tactile examination.

4thly, The use of the speculum.

5thly, The use of the uterine sound.

6thly, The use of sponge-tents, with the view of dilating the os uteri, so that the finger can be introduced into the cavity of the cervix or cavity of the body of the organ.

7thly, The microscopic or chemical examination of the discharges from the uterus and vagina.

8thly, The employment of the exploring needle in cases of fluid collections, in order to ascertain the contents of such collections; and

9thly, The adoption of anæsthetic agents, to relax the abdominal parietes, and enable us to practice the different modes of examination, in cases of excessive or neuralgic tenderness of the abdominal surface or vagina, &c.

In my next lecture I will attempt to describe how these different means of physical diagnosis are to be employed; in what cases they ought to be adopted, and what special advantages and diagnostic indications can be obtained from the use of them individually and conjointly.

CLINICAL SURGERY.—PROFESSOR SYME.

(*Delivered on the 9th of January* 1851.)

GENTLEMEN,—I now place before you the tumour which was removed yesterday from the female patient whose case I mentioned at our last meeting. It had been growing for fifteen years, as you saw, in the neighbourhood of the right labium, and is found to weigh four pounds and a-half. The texture, when exposed by a section, presents the usual characters of adipose sarcoma; and the ulceration which existed at the most dependent part of the swelling is entirely superficial, probably resulting from the effect of attrition, and not indicative of any alteration in the nature of the morbid growth. The edges

of the wound, you would remark, were stitched together, and compressed by a sponge of sufficiently large size.

You have also now before you the foot which was amputated yesterday at the ankle. There is extensive disease of the tarsal and metatarsal bones, with scrofulous deposition in the soft textures, so extensive as to present somewhat the appearance of a malignant tumour. You would remark that the operation was completed without the slightest difficulty or delay, although the chief obstacle to its completion, which is separation of the integuments of the heel from the os calcis, is of course greatest when the patient is, as in this case, an adult male, where the bone is most fully developed. I trust, therefore, that you will not permit any exaggerated statements, as to the mechanical difficulty of this procedure, to prevent you from undertaking it, especially as the advantages attending its performance are so great, when compared with the effects of amputating the leg, whether we regard the diminution of danger or the patient's comfort. You had a few days ago an opportunity of seeing a young woman from the southern part of Scotland, who, having suffered this operation here twelve months since, had returned in quest of an artificial foot. You must have noticed the convenient cushion which the integuments of the heel provide for the extremity of the stump; and, I hope, heard the patient's statement, that she was accustomed to stand, and walk upon it through the house without any covering whatever. I may now show you the apparatus which seems to me best suited for adapting a shoe or boot in such cases. It was constructed by a bootmaker of this city for his son, whose foot I had amputated.

The girl whom you lately saw labouring under Hæmatocele of the arm-pit, being now about to return home, I beg you to remark how completely she has been relieved by the means employed. The tumour, when punctured, was found to contain, as you may recollect, twenty-four ounces of bloody serum, and four days afterwards, having been again emptied, was injected with two drachms of the tincture of iodine. A moderate degree of excitement followed, and was soon succeeded by absorption of all remaining trace of the disease.

You may recollect that, at one of our meetings a few weeks ago, I called your attention to the case of a patient who had been sent here for advice by my friend Mr Nasmyth, on account of disease in the alveolar process of his upper jaw. I stated that this case seemed to me similar to others which I had met with, presenting peculiar characters not described in books. I related the particulars of some of these, which all agreed in presenting several openings leading to the bone, which felt porous and permeable to the probe for a limited extent, but perfectly fixed, and showing no disposition to separate, notwithstanding the lapse of months or years. Having, upon all such occasions, found that a free incision through the whole extent of gum lying over the affected bone was quickly followed by exfoliation of the diseased part, I anticipated that this would be the result in the present instance. I now remove the piece of lint which the patient has been wearing in the wound; and next, you observe, extract, without the slightest difficulty, a large irregular exfoliation, so that a speedy and sound cicatrisation may be expected. From two to three weeks are usually required for detachment of the diseased bone; but in one case, of a fine young seaman, about nineteen years of age, who had been dismissed from her Majesty's service in the most formal manner, on account, as it was alleged, of diseased *antrum*, recovery was completed in ten days. He then wished to re-enter the service, but was rejected at Leith, on the ground of his previous discharge, and entered on board a merchant ship.

This other man has been supposed to labour under a similar disease to the one last mentioned; but I find, as you see, that the aperture is in the palate, instead of being on the outer side of the gum, as it always has been in the cases that have fallen under my observation; and I also observe that the canine tooth is wanting on the side affected. It is probable, therefore, that the hard substance felt through the aperture is this tooth; and upon examination, by obtaining sufficient space for the purpose, I find that it is so. I therefore desire that the

patient may be sent to Mr Nasmyth, for the benefit of his assistance, as the difficulty of extraction threatens to be considerable.

The next patient presents a case of Fistula in ano, which you must not suppose to be endemic in Edinburgh, from the frequency with which it is brought before you, since most of the patients come from distant parts of the country,—this man, for instance, nearly a hundred miles. We undo his loose hospital dress, place his back upon the table, and elevate his limbs, so as to bring the part affected fairly into view. I then introduce my left forefinger into the anus, and a probe into the sinus at the small orifice, which is found on the summit of a small warty-looking eminence, about two inches from the orifice of the bowel. Directing the probe towards my finger, I feel the internal aperture very little beyond the junction of the skin and mucous membrane. Pushing on the probe, I bring its point outwards into view, and holding it there, with one stroke of the knife divide the textures which it includes, so as to set it free, and effectually accomplish the object of the operation, which is to cut that part of the septum lying between the external and internal openings of the fistula. A piece of lint is then placed in the wound, and the patient sent to bed, requiring no further dressing or care, except what is requisite for the removal of impurities, and with the prospect of being quite well in a few days.

It may seem surprising that this man should have left his distant home in the Highlands, passed through the great western city of Scotland, and pursued his course hither, in search of the relief which you have seen so simply and instantaneously administered. The only explanation I can offer, is the extreme slowness with which the members of our profession have opened their eyes to an improvement that rendered the operation for fistula easy, safe, and effectual, instead of being painful, dangerous, and uncertain. This was the proper limitation of incision, for which we are indebted to M. Ribes, with reference to which I may quote the following passage from a "Treatise on Diseases of the Rectum," which I published in 1838:—

"It had been noticed by Sabatier and other good surgeons, that the internal opening of a complete fistula was generally seated near the orifice of the anus. But in 1820, M. Ribes had the merit of showing[1] that it was always so situated, never exceeding the distance of an inch and a-quarter, and often lying considerably nearer the skin. The importance of this observation will appear when it is recollected that the operation requires division of the parts intervening between the two openings of the fistula; since, unless the internal one be sought for in the proper place, it may escape detection, and thus not only occasion an unnecessarily high section of the septum, but, from not being included in the incision, lead to a continuance of the disease. When the internal opening is sought for at the summit of the sinus it cannot be found, so that the fistula is apt to be supposed incomplete or blind external; and M. Ribes, avoiding this error, ascertained that an internal aperture existed much more frequently than had formerly been supposed. He went, indeed, into the opposite extreme, contending that it was present in every case requiring the operation, and accounting for its constancy by attributing the origin of the disease to ulceration of the mucous coat of the gut. But I have already stated that the abscess which gives rise to fistula is very generally discharged outwards in the first instance; and every attentive practitioner must have remarked that an internal orifice is very seldom met with in recently formed fistulas; which facts are quite inconsistent with this theory of M. Ribes.

"I have ascertained farther, that, in those cases where an internal aperture does not exist, the mucous membrane at the part in which it would be situated if present is not only denuded, but rendered so thin that the perception of a probe through it is hardly less distinct than if it had entered the rectum; and

[1] "Quarterly Journal of Foreign Medicine and Surgery. 1820."

that, if the incision extends to this point, the cure will be no less certain than if an opening into the gut had existed.

"In regard to the importance of the principles thus established, I may, in the first place, remark, that, limiting the incision within the narrow bounds that have now been mentioned, lessens not only the difficulty of its performance, and the suffering of the patient, but also the risk of hemorrhage, and the trouble of after-treatment; since, instead of having to keep separate the edges of a deep and not easily accessible wound, the surgeon has merely to prevent adhesion between the lips of a superficial cut. But the operation, while thus simplified in its performance, is also rendered more certain in its effect, since in cases of complete fistula the most extensive incisions will fail to afford permanent relief, unless they include the internal opening. I have very frequently operated on complete fistulas that had been looked upon as blind external, from the internal orifices having escaped detection through unacquaintance with its position; and the repetition of operations for the disease, which are so frequently heard of in practice, are no doubt referable to this mistake.

"In the reports of surgical cases which I have published from time to time since the year 1829, and also in the systematic work on Surgery, of which the first edition appeared in 1831, I have endeavoured to explain and impress these principles which have been uniformly acted on in my own practice. They are still, however, far from being generally adopted, and many writers of the highest authority continue to inculcate the practice of Mr Pott. Sir A. Cooper says,[1] 'If the fistula does not open into the intestine, you must pass the instrument (a bistoury) up the sinus till it reaches the extremity.'—'A very copious hemorrhage generally follows the division of the septum,' &c. Mr Copeland says, 'In this operation, though there are no vessels of very considerable size in danger of being wounded, yet, when the sinus extends far up the side of the gut, a hemorrhage now and then takes place, either at the time of the operation, but more usually a few hours after it, which, if it be not important from the magnitude of the divided artery, becomes often so from the difficulty, perhaps impossibility, of securing it by a needle and ligature.'—'I will venture to say, that it (the hemorrhage) has occurred to almost every surgeon who is in the habit of performing the operation.'—'After many unsuccessful attempts to secure a bleeding vessel under such circumstances, I once accomplished it by introducing a blunt gorget into the rectum; and, by keeping the gut thus dilated, I was enabled to see the orifice of the bleeding artery and to secure it.'[2] Mr Liston says, 'Some contend that fistulæ are always complete, that they commence from within, and that the internal opening is always at one particular point; but such, according to my experience, is very far from being the case.'—'Having reached the extreme depth of the canal, the direction of the instrument's point is changed, so as to apply its cutting surface to the coats of the bowel at that part.'"[3]

In the second edition of my treatise I added the following passage:—

"Having so long contended in favour of the almost constant presence and fixed position of the internal orifice, I am happy to observe that Sir B. Brodie now advocates the same doctrine.

"'The first thing to be done is to find the inner opening. I do not say that you will always succeed in finding it,—certainly not the first time, but you will rarely fail if you look for it in the right place. Formerly, I often failed, and for this reason,—I did not know where to look for it. I used to think that it was to be found in the upper part of the sinus, but it is never found there if the sinus runs high up. You must search for it immediately above the sphincter muscle.'"[4]

---

[1] "Surgical Lectures, p. 425. 1837."
[2] "On Diseases of the Rectum and Anus, pp. 159-161. 3d edition."
[3] "Elements of Surgery, vol. iii. pp. 70-82.
[4] "Lancet, 26th January 1844, p. 533."

Having thus long and earnestly contended for the proper treatment of fistula, founded upon its true pathology, and endeavoured to bestow the credit of establishing this where it was justly due, I read yesterday, with what feelings of surprise you may imagine, the following statement of a clinical lecture, delivered by Dr Hughes, surgeon to the Jervis' Street Hospital, and published in the "Dublin Medical Press" of 20th November last.

"Till within a comparatively recent date, writers who were received as the best authorities on the subject of fistula in ano, have recorded their opinion, that the external blind fistula was the most common form of the disease; however, one of the highest modern pathologists—I mean Sir B. Brodie—has, from extensive investigations on both the living and dead, arrived at the conclusion, that the complete fistula in ano is the most frequent form of the disease; indeed, Sir Benjamin Brodie is so impressed with the fact, that he states that there are very few cases, if any, in which the fistula does not communicate with the gut internally; and he insists, that if the surgeon will only look for the internal opening in the proper place, that he shall surely find it; and he asserts, that although the action of the sphincter and levator ani muscle may interfere with the healing in of an abscess in connection with the rectum, yet he principally ascribes its non-healing to the constant presence of this internal opening, and to the continued irritation kept up by the infiltration of the contents of the bowel, whether feculent or gaseous, into the cavity of the abscess.

"When this announcement was first made, it startled the profession not a little, emanating, as it did, from one of the highest authorities in Europe, and which, if established, would throw considerable light, not only on the pathology, but also on the treatment of fistula in ano; however, there are many excellent surgeons who still question the invariable presence of the internal opening in fistula in ano. The subject is one of deep interest, and in my mind can only be settled by an extensive series of post-mortem examinations. I, therefore, wish to speak cautiously on the subject; still I hope I may, without committing myself, say, that I suspect cases have been considered as blind external ones which were really of the complete form, and I am convinced that such mistakes can satisfactorily account for the occasional failure of operations for fistula in ano. I, therefore, am of opinion, that the profession is much indebted to Sir Benjamin Brodie for having directed their attention to the frequency of the internal opening in fistula in ano."

I now place before you the patient who had his stricture divided this day week. He tells us that he has suffered nothing from the operation; while, on the contrary, he never had a bougie passed without severe and often protracted rigors, with their febrile consequents. You will recollect that he had suffered from the disease for eighteen years, during which the stricture was repeatedly dilated to the full extent, but never with the effect of rendering micturition easy and comfortable. Latterly, he had been unable to pursue his employment in Glasgow, and came here soliciting the operation, from the good effects of it which had come to his knowledge. No water has passed through the wound since the fourth day after the stricture was divided; and you see I can now introduce a bougie of the largest size into the bladder, without the slightest difficulty or uneasiness. This being the fifth stricture which you have seen me divide since the commencement of the present session, with the same satisfactory result, I trust your testimony will tend to contradict the erroneous impressions so generally abroad with regard to this procedure.

Part Fourth.

PERISCOPE.

MEDICINE.

CASE OF CANCER OF THE LEFT LUNG, WITH CLINICAL REMARKS. BY DR ROBERT L. MACDONNELL, MONTREAL.

March 2, 1850.—I was called to attend Miss ———, aged seventeen, daughter of Captain ———, the history of whose illness has been furnished to me at great length by her father, from whose notes I have condensed the following account:—Two years before, she caught cold, having sat in damp clothes for a whole day; the catamenial discharge, which had been just established, was suddenly arrested, and did not appear for five months; she was attacked with pain in her left side, back, and top of the left shoulder. These pains continued, and were followed by difficulty of breathing, and inability to lie upon the right side; but without cough or expectoration. After some months, a small tumour appeared above the left clavicle, somewhat painful to the touch, and to which tincture of iodine was applied by her medical attendant. In the month of July she was sent to Upper Canada, for change of air; but derived no benefit from it, and returned to Montreal in September much worse. The tumour noticed in the neck had become enlarged, although not yet conspicuous; but it was painful, and this sensation extended up along the side of the neck. At this period her father noticed slight ptosis of the left eyelid, and contraction of the pupil of that eye, "and the iris did not expand and contract equal to the other, in the transition from light to shade." "During the winter of 1848-9, she complained much of pains in her arm and shoulder, particularly at night, and often groaned in her sleep; yet she went out, and her appearance, occupations, and amusements, were as usual." "In the spring of last year," continues her father, "I first observed the left arm to have shrunk or withered conspicuously, yet the tumour in the neck had not much increased in size." The following summer was spent in Upper Canada, during the most part of which she suffered greatly from pains in the left side of the chest, in the back and shoulder, and from debility and dyspnœa. She returned to Montreal last September, so altered in appearance that her father hardly recognised her; she staggered into the hall, "a poor emaciated creature, with a ghastly countenance of a blueish green colour. She had upon her a constant hacking cough, great shortness of breath, had lost all appetite, was reduced to a skeleton, and so weak that she sank upon the bed, whence she did not rise for three weeks."

She was now seen by a physician of this city, who prescribed cod-liver oil, and the local application of iodine to the tumour. "From this time her health fluctuated a good deal; and," adds her father, "Jan. 9.—Favourable symptoms of change in my daughter's health; was enabled to use her left arm, of which she had lost the use for two months." Jan. 10.—A violent fit of dyspnœa set in, accompanied by hysteria and delirium. The journal from which the above quotations have been taken goes on to say, that, "up to March 20, many serious attacks of spasm have been staved off by your prescriptions, and all have ultimately yielded to them; but, with her increasing weakness, the spasms are more frequent, and apparently more dangerous."

Present State.—The attention is immediately attracted by a large tumour on the left side of the neck, which protrudes upwards from the thorax, through

the space bounded by the clavicle in front, and the spine towards the middle. This tumour is of an irregular shape, somewhat globular, uneven on its surface, everywhere resisting, with the integument tense, shining, and œdematous. It is quite dull on percussion, and no pulsation, bruit, or thrill, are perceptible. It is not painful to the touch, nor is it the seat of any constant pain, though shooting pains occasionally proceed from it downwards to the fingers. The surface of the tumour is traversed by large tortuous veins, which anastomose freely with similar veins on the left side of the thorax, the left arm, left side of the neck, cheek, and left side of the forehead. The tumour has by its pressure produced paralysis of motion and sensation of the left arm, and the pulsation of the ulnar, radial, and brachial arteries is completely obliterated. The whole of this arm, and corresponding part of the chest, are extremely œdematous.

Physical Signs.—The anterior portion of the left side of the chest is full and prominent, and is continuous with the tumour, there being no depression to mark the supra and infra clavicular spaces. The clavicle is dislocated forwards; its sternal end projects nearly an inch in front of the sternum. The left side of the chest does not move in inspiration; its intercostal spaces are obliterated, and an extremely dull sound with resistance is elicited by percussion from the clavicle to its lowest part, and the same dulness extends all over the side behind and laterally. The respiration is bronchial before and behind, and there is also bronchophony; but not the least râle of any kind. The upper portion of the right lung in front is clear upon percussion, but from the mammary region downwards it is quite dull. Behind, the respiration is loud and puerile, and without râle. All over the right mammary region the impulse of the heart can be seen and felt; its impulse is extremely abrupt and violent; and both sounds are accompanied by a loud sharp ringing souffle, of a peculiarly musical and metallic character, quite unlike anything I have ever heard. The apex of the heart strikes towards the right axilla. The right hypochondrium yields the usual dulness. There is no increase of hepatic dulness below the ribs. The left side of the chest appears to be increased in size; but I have not yet had an opportunity of determining this point. The respiration is short, about 40. The inspiration is accompanied by great action of the intercostals of the right side; the expiration is accompanied by a short stridulous grunting noise. During the performance of inspiration, the larynx (which is pushed towards the right side) is drawn across the mesial line to the left side. The voice has never been affected, and, except during the severe attacks of dyspnœa, there is no stridor. The voice is naturally œgophonic. She has no cough, nor does she expectorate anything. She has never had hæmoptysis, nor has she at any time had red-currant jelly-like expectoration; but she is subject to frequent attacks of epistaxis, which invariably proceeds from the left nostril. At times this is rather profuse; at other times it is only sufficient to cause a blocking up of the nostril. The beating of the heart is frequently very troublesome; the pulse is small, about 120, sometimes more frequent, and sometimes less so; it is not intermitting nor irregular. There is partial ptosis of the left eyelid, which sometimes proceeds so far as almost to conceal the eyeball, and there is also contraction of this pupil, though this eye is quite as sensible to light as the other, and she can read with it quite as well. No matter to what amount of light this eye be exposed, the pupil is never dilated to more than half the extent of the other. She never suffers from headaches, flashes of light before her eyes, noise in the ears, or frightful dreams. Occasionally her symptoms assume a hysterical character. Her tongue is clean, appetite pretty good, no dysphagia, stomach seldom sick, bowels regular, urine secreted in natural quantity, skin moist, body greatly emaciated. No pains in the chest. Catamenia absent. During the prevalence of easterly winds, and before a fall of snow, her friends remark that her countenance brightens up, she appears quite healthy, and her spirits improve;—these are but the precursors of an extremely severe attack of dysp-

nœa, which is sometimes, however, warded off by the administration of an antispasmodic.

April 24.—Since the last account the tumour has been gradually extending across the neck; it has pushed the larynx, trachea, and thyroid gland completely over to the right side, and now occupies the median line, and extends beyond it. The right arm is now partially paralysed, and the pulse at the wrist is perceptibly smaller than it used to be; œdema and varicose veins occupy the right side of the chest, and the right arm is also becoming œdematous. Since the last report, several severe attacks of dyspnœa and palpitations have occurred,—sometimes almost threatening to terminate her existence. It is noticed that these attacks are invariably preceded by a temporary amendment,—her spirits become cheerful, her strength increases, and the countenance becomes animated; the colour of the face, which is usually sallow and livid, changes to a bright rosy hue; but at the same time it is quite apparent that the tumour undergoes marked augmentation, and that the tortuous and varicose veins become more enlarged and turgid. Her father is positive that he has noticed a connection between these phenomena and the state of the atmosphere, and that they occur invariably in thick damp weather, or previous to a snow-storm. On two or three occasions she became delirious, and her stools and urine were voided unconsciously. The appetite remained pretty good, and the bowels were generally regular. She slept well when free from spasm, and had latterly suffered but little from the pains in the chest and shoulder. During the whole period of my attendance, I never heard her cough, nor had she ever any expectoration; she always lay upon her back. The degree of ptosis varied, but no change occurred in the state of the pupil. The bleeding from the nose occurred almost daily. Enlargement of the liver was noticed towards the close of the disease, and though the left side of the chest had become enlarged, and the intercostal spaces were raised even above the level of the ribs, yet her extreme debility, and the increase of her sufferings induced by a change of position, prevented my measuring the chest accurately.

After a succession of severe attacks of dyspnœa, she expired.

The treatment consisted of generous, bland diet, a moderate quantity of wine, and the use of camphor, ether, opium, lobelia, &c., sometimes given conjointly, at other times separately, according to the judgment of her father, a gentleman of great sagacity, who from close and unremitting attention to all the phases and variations in her case, acquired a rare tact in the employment of these drugs. The neuralgic pains which attacked the chest and shoulder, and sometimes extended down the arm, were always relieved by a warm lotion containing tincture of aconite, in the proportion of one ounce of the common tincture to seven of water. Folds of lint saturated with the above lotion were laid over the painful parts, and evaporation prevented by surrounding the lint by a piece of oiled silk. This application used to give great relief.

Post-Mortem Examination.—Before opening the body, a careful examination was made by inspection, percussion, and measurement, when the following circumstances were noted. The whole of the front part of the chest was œdematous, and traversed by large tortuous veins which anastomosed freely with the superficial epigastric veins. The greater number of these vessels were noticed upon the left side. The left arm, from the shoulder down to the hand, was much swollen from œdema, and at its upper part were numerous veins inosculating with those of the neck, chest, and axilla. The right infraclavicular space was depressed, the left was full and prominent, and constituted part of the tumour already spoken of. The right shoulder was elevated, and the clavicle was separated to about the distance of an inch at its attachment to the sternum. Percussion yielded the same results as were noticed during life, with this exception, that there was complete dulness extending from the normal hepatic region, downwards for the extent of two inches below the margin of the ribs. The circumference of the neck and tumour measured above the

clavicle, was 16¼ inches; the distance from the nipple to the sternal end of the clavicle, 6 inches on both sides. The circumference of the chest upon a line with the nipples, was 27 inches; it being on the right side 12½, and upon the left 14½ inches; distance from the right nipple to umbilicus, 9½ inches, from the left 10¼. Nothing remarkable was observed on any other part of the body, except extreme emaciation. On opening the thorax, the heart and pericardium were observed lying to the right of the sternum, and distant about three inches from the mesial line. The pericardium was quite healthy and contained no fluid, nor was it adherent in any situation. The heart was of natural size, and free from any disease whatever, either of its walls or valves. The left side of the chest was occupied by an enormous mass of encephaloid cancer, which adhered firmly to the ribs, and was continuous with the tumour noticed in the neck. It was contained within well-marked cysts, which enveloped it in the same manner as the arachnoid surrounds the brain, and which, when slit open, allowed the cerebriform masses to be seen, presenting well-marked convolutions and sulci exactly resembling those of the brain. In a few situations, hemorrhagic clots intervened between the investing capsule and the surface of the mass. There was no adhesion to the front part of the ribs or to the sternum, diaphragm, or pericardium. No trace of pulmonary structure could be seen, except at the diaphragmatic portion of the tumour, where a thin layer of condensed lung was spread over it for a small space, and peeled off it, as if merely coherent from apposition—no bronchial tubes extended from this portion of lung to the cancerous mass, nor could any be traced in the latter—the left bronchus entered its upper part, but no traces of its ramifications could be discovered. Such were the characters of that portion of the mass within the chest, but as it was emerging from the latter situation, it had dislocated the clavicle, and was indented by the latter bone. At this point, the tumour pressed upon, and stretched out, the left subclavian artery and vein; the left carotid, though not so much interfered with, was pushed a little towards the mesial line. This artery, as well as the pneumogastric and sympathetic nerves, were pushed backwards by a process of the growth which proceeded towards the lateral processes of the cervical vertebræ, to which it took a strong attachment. On the anterior part of the tumour, the sterno-cleido-mastoid, and the sterno-hyoid muscles were spread out in riband shape, and their fibres were separated from one another. The brachial plexus passed through the middle of the growth, and could not be completely separated from it, even by the scalpel. The third stage of the subclavian artery was obliterated by a coagulum, and was not much larger than the radial. The phrenic nerve passed over the most prominent part of the tumour. The œsophagus was pushed towards the middle, and as noticed during life, the larynx trachea and thyroid gland, were shoved over into close contact with the right brachial plexus. The mass adhered firmly to the clavicle near the shoulder joint, and also took an attachment to the acromion, and a portion of it passed under the trapezius muscle. When removed from the body, the mass was weighed, and found to amount to six pounds and a-half. The right lung was quite sound, except at its inferior part, where we found three small encephaloid tumours, of the size of large currants, growing from the surface of the lung, and covered by the pleura. The liver was much enlarged from congestion, and when cut into, blood escaped in large quantity.

The other abdominal organs were all healthy. The brain was carefully examined. Some slight vascularity was noticed upon the pia mater, but there was no effusion either beneath the arachnoid or in the cavity of the ventricles. The origin and course of the third nerve were accurately examined, but nothing abnormal could be detected, and the same remark applies to all the cerebral nerves, and to the structure of the brain itself.

It is only within the last few years that the diagnosis of cancer of the lung has been established on a tolerably certain basis; yet cases are occurring

almost daily, which prove that many points connected with the disease remain to be worked out yet, by the diligent in this field of observation. It is to be regretted, that the nature of some of those cases was not even suspected during life, that others were under the observation of practitioners imperfectly acquainted with the existing state of knowledge upon this and other thoracic diseases, whilst a third class of cases has been carefully observed during life, but no post-mortem examination having been performed, we are unable to connect their signs and symptoms with anatomical lesions observed after death. It is, therefore, the duty of every one to add to the scanty list of accurately noted cases, any that may occur in his practice.

At the time the case of transposition of the viscera was noticed at the hospital (the details of which I laid before the profession in the April number of this Journal), some of my pupils mentioned to me, that a young lady was then under the care of a physician of this city, in whom the heart was observed to pulsate on the right side, and they inquired if it were not highly probable that it was also an instance of congenital transposition. I replied, that I considered it much more probable that the patient laboured under chronic pleurisy of the left side, with displacement of the heart to the right. In a few days the case came under my own care, and I had no hesitation, at once, in declaring it to be an example of cancer of the lung, and for the following reasons, to all of which I drew the attention of my clinical class when lecturing upon this affection:—

1. In nearly all the best observed instances of *primary* cancer of the lung, the disease has been ushered in by symptoms of pleurisy, and in many of them those symptoms have existed throughout; in some cases undergoing abatement, as the cancer advanced; in others, disappearing, or remaining stationary. To the former class belong the case under consideration, those by Graves,[1] Heyfelder,[2] Hughes,[3] Sims,[4] and Stokes;[5] and I doubt not, that, if the early history of some other published cases had been inquired into, that the disease would have been found to have commenced with an inflammatory attack of the lung, or its investing membrane—at least, this has been the result of my experience of the disease, and in the above case no exception was formed.

2. The only diseases producing enlargement of the side, absolute dulness, and firm resistance on percussion, absence of *vesicular* respiratory murmur, protrusion of the intercostal spaces, and detrusion of the heart, are, extensive empyema, and cancer of the lung or mediastinum. In the foregoing particulars these diseases resemble one another; but I have only once noticed enlargement and tortuosity of the veins of the chest in empyema, and in only one case have I noticed a *bruit* in a heart dislocated from pleuritic effusion, and though I have observed tumours forming on the exterior of the chest in empyema, yet such tumours were always soft, fluctuating, and devoid of pain. Œdema of the side has been often noticed in acute pleurisy, less frequently in chronic, but in no instance of either form of the disease, have I observed it, accompanied by *permanent* œdema of the corresponding arm; and empyema, so extensive, as to have produced such a great change in the size of the chest, and, in the relation of organs, might have enabled peripheric fluctuation to have been detected.

3. The want of pulsation, bruit, thrill, dysphagia, of laryngeal cough, together with the extent of dulness, &c., were opposed to the idea of the disease being aneurismal, although the paralysed condition of the left arm, and loss of pulse at the wrist and bend of the elbow, are not uncommonly observed in thoracic aneurism springing from the aortic arch or some of its branches; and the same cause might produce a varicose condition of the veins of the thorax and

---

[1] Dublin Journal of Medical Sciences.
[2] Heyfelder, Archives Generales.
[3] Guy's Hospital Reports.
[4] Medico-Chirurgical Transactions, vol. xviii.
[5] Dublin Journal of Medical Sciences, vol. xxi.

arm,—a point to which my friend, Professor Walshe, of London, has directed attention.

4. To the idea of extensive deposition of tubercles in the lung and tubercular disease of the cervical glands, pathology and clinical observation are both opposed, for I do not believe that tubercular disease of the lung ever produces enlargement of the affected side of the chest, or of any portion of it.[1] And in no case, no matter how extensive the deposit, have we complete dulness and total loss of respiratory vesicular murmur; and in extensive tubercular deposit, softening, and consequently the signs of cavities, would have taken place long before I saw the patient; nor should we find a healthy condition of the opposite lung, nor dislocation of the heart; and I need hardly add, that extensive tubercular deposition in one lung would soon have been accompanied by signs of disease in the other, by cough, hæmoptysis, hectic, &c.; and that external tumours, varicose veins, œdema, and paralysis of the upper extremities, are not seen in phthisis.

5. With the idea of the disease being chronic pneumonia (even granting its frequency, as some modern writers seem disposed to believe—an opinion to which my own experience is much opposed) the facts of the case were quite incompatible.

So that we find, that, with no other disease than cancer could I reconcile the history, symptoms, and physical signs of this case; and I shall now direct the reader's attention to some of its peculiarities.

Amongst the most prominent may be mentioned the absence of cough, of bronchitic râles, of stridor, of dysphagia,[2] of currant-jelly-like expectoration (for which, however, the daily epistaxis was a substitute), and of aphonia.

The paralysis and pain of the left arm are easily accounted for, by the pressure of the tumour on the brachial plexus of nerves, and by the same pressure acting upon the subclavian artery and vein, the absence of pulsation and the presence of œdema are explained. It is worthy of remark, that it must have been solely by mechanical pressure that the tumour produced paralysis, and not by involving the nerves in its own structure, for the larynx and thyroid gland being shoved over by it, into close contact with the plexus of the right side, paralysis, to a less extent, was produced of the right arm.

Dr Sims and others have noticed the œdema of the arm, resembling that of

---

[1] I am well aware that, on the authority of a verbal statement of Dr Chambers, of London, that some writers—amongst others, Professor Walshe—have alluded to an elevation or bulging forward of the infra-clavicular region, as a sign of incipient phthisis; but as I have anxiously looked for this sign for several years, without in one single case observing it, I can only account for the discrepancy by supposing that in Dr Chambers's cases both sides were affected, and the atrophy being better marked on the side where the least physical traces of incipient phthisis were observed, the opposite infra-clavicular region presented an appearance of comparative fulness or bulging, well calculated to deceive; for I have not unfrequently remarked (and had recently an opportunity of pointing out to my class at the hospital) the fact, that in incipient phthisis we may have marked atrophy of the infra and supra-clavicular spaces of one side, with comparative dulness and feebleness of respiratory murmur; whilst, on the opposite, there may be no atrophy, scarcely any dulness, with a harsh respiration, gradually becoming accompanied by a "crumpling sound," dry crackling, and then (as in the case just alluded to) sibilant and muco-crepitating râles; yet the condition of the opposite lung may remain as when first examined. It is only in this way that I can account for the striking anomaly said to have been observed by Dr Chambers.

[2] The earliest case recorded of cancer of the lung, attended by dysphagia, is that, I believe, given by Van Sweiten. He says, "For this last observation, I am indebted to the learned Dr Anthony de Haen, who exerts himself in the practice of physic with great applause at the Hague, and with indefatigable industry takes every occasion to inquire into the latent causes of diseases in dead bodies, and who opened this patient after his decease, before the celebrated Schwencke, professor of anatomy and surgery."

phlegmasia dolens; but the total absence of pulsation in the principal arteries,[1] with complete paralysis of one arm, and partial paralysis of the other, are now recorded for the first time; and though pain of the chest and through the shoulder is often mentioned, yet I know of no other instance where it extended down the whole arm. I may here observe, that pain of a constant nature, spoken of by Dr Stokes as a valuable diagnostic, was not present in this case; for, when once relieved by the aconite lotion, it did not often return, and even before my attendance it was not constant, nor, indeed, of late, was it frequent.

Dysphagia appeared only in the last two days of her illness.

As might be expected, some cause for the ptosis and contraction of the pupil was carefully looked for in the brain and third nerve, but without success; no trace of disease could be discovered. How, then, are we to account for these symptoms? We know that ptosis is usually accompanied by a dilated state of the pupil, and by paralysis of the superior, internal, and inferior recti muscles, and also of the inferior oblique; but, as was stated before, there was no paralysis of any of these muscles; and the pupil, though constantly contracted, became smaller and larger, according as the intensity of the light was increased or diminished. Nor can we suppose that the superior branch of the nerve was alone affected, for we know that that portion sends no twigs to the lenticular ganglion. If we appeal to experimental physiology, we do not receive a more satisfactory solution of the entire question; but for part of it we can account. It was ascertained by Longet, that division of the pneumo-gastric and sympathetic nerves, in the neck of some animals, was followed by contraction of the pupil; whilst, in others, it was followed by dilatation. Now, if the same cause produce effects so opposite, on such a delicate organ as the iris, it merely proves that the division of these nerves acts in disturbing the innervation of the organ, in one case producing a diminution of power of the circular, in the other, of the straight fibres of the iris; and if we admit this explanation to be correct, we can understand how, in an analogous experiment, the nervous power of the other branches of the third being diminished (for it is evidently owing to the connection of this nerve, and of the fifth, with the sympathetic, that the phenomena are produced) some of the muscles supplied by that nerve may be actually in a slightly paralytic condition, which may escape the observation of the patient and of his physician, unless a strong antagonising muscle be in action, as in the case of the orbicularis palpebræ in the foregoing case, and then the diminished power of the levator palpebræ becomes at once apparent. I make use of the term analogous experiment advisedly; for it must be evident, that pressure on the pneumogastric and sympathetic nerves produced the same effect, for the time being, as division would have done; so that, in this instance, disease imitated the experiment of the physiologist, and went far to corroborate it. This view is borne out by what was frequently observed, that on those occasions when the tumour of the neck became enlarged, and the venous system more congested, the ptosis was always more marked. Should the foregoing explanation not prove satisfactory to any of my readers, they are at liberty to account for the phenomena of contracted pupil with ptosis, and without paralysis of the muscles of the eye—the brain being healthy—upon any other hypothesis they may consider more convincing. I have offered the best that has suggested itself to me.[2]

* * * * *

---

[1] Dr Walshe says: "I have recently found the radial pulse very triflingly weaker and smaller on the affected (right) than on the other side; but in this case a mediastinal tumour co-existed with the disease, affecting the root of the lung; the difference has been observed independently of the former, and is, under all circumstances, rare." —*Treatise on Cancer*, p. 352.

[2] The iris receives a branch from the sixth nerve in several animals, and it has been supposed that it sometimes does so in man, which would account for the fact, that the pupil has not been affected in some cases where all the other muscles of the

In conclusion, I would remark, that this is the largest specimen of cancer of the lung on record. In Dr Graves' case, the diseased mass is said to have weighed more than six pounds, but the patient was a male adult. In the above instance, the mass removed weighed six pounds and a-half—some ounces more having been left behind—and the patient was a young girl of low stature and delicate formation.—From the *British American Journal of Medicine*, June 1850.

ON TAPEWORM. BY DR KERLÈ.

In the neighbourhood of Osnabrück, the only species of tapeworm is the Tænia solium, which is cured more easily than the other kinds of tapeworm, and also with different remedies. The author believes that tapeworm is becoming more frequent there than formerly. In the month of June he himself had procured the expulsion of it in thirty-seven cases. In the summer months, especially in June, the parasites appear to sicken, and throw off separate articulations from their bodies; and thus the patients, having their attention more directly called to their ailment, apply more commonly for medical aid during the summer. In places where there is much fish used, tapeworm appears to be more common. It is very common in Stockholm, Finland, and Russia. In Holland, he says, every second person (?!) is affected by it; whilst in Switzerland, one in ten. It occurs at all ages. Hufeland saw a worm thirty yards (*ellen*) long passed by a child six months old. The author also has seen it frequently in children, who, nevertheless, have complained very little of it. The opinion that tapeworms, lumbrici, and ascarides, never occur in one individual, is, according to him, incorrect; as is also the idea, that never more than one tænia solium exists in the same case. He has himself often expelled seven from one patient; and a Swedish physician saw eighteen expelled from a man in two days.—[Is not this a case recorded by Dr Haen?] In respect to the different lengths of the worms, he quotes Baldinger's authority for one of 700 yards, and the "Copenhagen Transactions" for one of 800 yards.—[These and other marvels are noticed in Dr Joy's article "Worms," in Cyclop. of Pract. Med.]

With regard to the oetiology, the author merely repeats Harvey's aphorism, —omne vivum ex ovo. The difficulty of expelling the worm he conceives to depend,—1st, On the structure of the worm, by which it can attach itself very firmly; and, 2d, On the tenacity of life of the animal. A worm lived twenty-four hours preserved in cold water, and moved when it was placed in warm water,—it did not die though placed first in cold, then in warm water, at intervals of twenty-four hours, and he has made the same experiments with detached joints of the worms. Tapeworms are not always killed, even by a boiling temperature. Rosenstein saw a living tapeworm in a boiled fish; and Coulet maintains, that the worms can remain twelve hours in a boiling liquid without perishing.

The only true indication of the presence of the worm, is the passing of detached joints; and he points out, that an attempt should never be made to cure the disease, unless the nature of the case has been shown by this unequivocal symptom; at least, under contrary circumstances, it is advisable to avoid all strong measures. The so-called trial-cases (*probe kuren*) are to be entirely rejected. The author attaches great importance to the recent expulsion of separate joints; for he has never succeeded in expelling the worm when he undertook the cure in a case where no joints had recently been passed.

Treatment.—Although he reprobates trial-cases, he gives the following formula to be used in such, under ordinary dietetical precautions:—Gamboge, calomel, of each six grains; semina santonica, half a scruple; jalap, sixteen

---

eye were paralysed from disease of the third nerve. In the above case, the singularity consists in our having only one muscle supplied by the third nerve, and only one set of fibres of the iris in a state of semi-paralysis.

grains, with a drachm of oleo saccharum of tansy,—this to be taken fasting, and at one dose. As his "mild" method of treatment, especially for children, he employs the decoction of pomegranate-root bark. This will succeed twelve times in twenty; but the root grown in northern countries is useless. His "powerful" method of cure, is by Schmidt's medicine; but he gives only a third part of the prescribed dose of the pills; and this, he says, does not fail twice in twenty times. [What these Schmidt's pills are, we have not been able to learn from any books in our possession.]—*Schmidt's Jahrbücher*, No. 11, 1850.

PROLONGED INCUBATION OF INTERMITTENT FEVER. BY M. VERDIER.

The "Journal des Connaissances Médico-Chirurgicales," for June 1850, contains an essay by M. Verdier on the above subject. The author adduces five instances in which individuals, whose ordinary residence was far removed from an aguish district, have, after a longer or shorter stay in such a district, a few months afterwards, and when at home, become affected with intermittent fever. He argues in favour of the doctrine which would connect such instances of fever with the previous exposure to marsh miasmata.

This is nothing new to British practitioners: there must indeed be few who, in their practice, have not met with cases similar to those recorded by M. Verdier. It would, however, appear that the opinions of medical men in France are, on the subject of the incubation of ague, in an unsettled, or at least a divided, state. Thus we find from the commentary on M. Verdier's cases given in the "Gazette Médicale" (Nov. 2, 799), that M. Nepple, among others, absolutely denies the possibility of the incubation of ague even for a day, holding that the operation of the morbific agent must be instantaneous—an amount of scepticism which can only be excused by want of opportunity of observation. It happens that, during the last few months, we have had in Edinburgh very numerous illustrations of the prolonged incubation of intermittents. During the spring and earlier part of the summer of 1850, many labourers (chiefly Irish) presented themselves for admission into the Infirmary, on account of agues, generally of the tertian type. We ourselves interrogated above a dozen of these patients, with a view of eliciting information as to the source of their disease. In every instance we found that the patient had been occupied during the preceding autumn in an aguish district, such as the "Fens" of Lincolnshire; in some there had been at that time an attack of ague; but in at least as many ague had not manifested itself till after the patients had resided for some months in Edinburgh,—where, as is well known, the disease rarely, if ever, is observed in individuals who have not been exposed to the malaria of districts of the country notorious for the production of ague. Evidence more decisive of the question regarding "incubation" could hardly be desired.

CASES OF CYANOSIS. BY DR STREHLER, OF ERLANGEN.

A mother, who had formerly suffered from rickets, gave birth to five children (three male and two female), all of whom died immediately, or shortly after birth, with symptoms of cyanosis. The father died, at the age of thirty-six, of phthisis. The last-born of the children had, three days after birth, an alarming leipothymia, from which it recovered; a month afterwards it was seized with a severe diarrhœa, complicated, two days before its death, with symptoms of cyanosis. On dissection, the following appearances were discovered:—Substance of the brain very soft; pons varolii and medulla oblongata firm; lungs in the normal condition; foramen ovale and ductus arteriosus still patent; intestines very thin and transparent; intestinal mucous membrane covered with a great number of white points. We are disposed to admit with Dr S. that the rachitic taint of the mother, which evidently predisposes to disease of the heart, may be fairly regarded as the etiological cause of the cyanosis which supervened in the cases of her progeny.

The father had, by another woman, a healthy child, which still survives.—*Medizinisches Correspondenz-Blatt Bayerischer Aerzte, quoted in Gazette Médicale de Paris*, 28th December 1850, p. 925.

PERICARDIAL FRICTION IN THE COLD STAGE OF CHOLERA. BY DR PLEISCHL.

In three cases which occurred in the General Hospital of Prague during the epidemic of 1849, the author heard, over the region of the heart, a rubbing sound, precisely similar to that observed in cases of pericarditis with plastic effusion. The observations were confirmed by Drs Finger, Bamberger, and Czermak. All three patients died; and their bodies were inspected by Dr Dittrich. In two of the cases the pericardium contained no serum in its cavity; its opposed surfaces had a hazy lustre, and sticky feel, being bedewed with a viscid moisture. In the third case, the pericardium contained a few drops of viscid, thickened serum; there was some ecchymosis beneath the visceral portion of the pericardium; the heart in other respects healthy,—except a white smooth spot, of the size of a silver groschen, situated on the front of the right ventricle, might be regarded as morbid. Dr P. admits that the pericardial friction without effusion must be exceptional, even in the algide stage of cholera, since he only met with it twice in 120 cases.—*Präger Vierteljahrschrift*, 1851, 1ster Band, p. 103.

[Probably the third case should be excluded, for it is not quite clear that the white spot,—a product, most probably, of former inflammation,—was unconcerned in the production of the friction-sound. The other cases are, we think, interesting, as proving that mere dryness of the serous membrane may, in certain circumstances, induce one of the most characteristic signs of pericarditis; and as showing, that a too exclusive reliance upon the falsely termed pathognomonic physical signs of disease may lead to error.]

SURGERY.

INGUINAL HERNIA. TREATMENT BY INJECTION.

This subject seems to possess some little general interest. The disease is common, and the surgeon is often applied to, to know how far it may be cured by injection. This method of treatment is not new. In his work on Operative Surgery, published in 1846, Dr Pancoast states that he had employed it eleven years before that date. The operation consisted of an injection into the sac of a stimulating fluid, by means of a minute trocar and canula, to which a syringe was afterwards adapted. This writer mentions Lugol's solution of iodine, or the tincture of cantharides, in quantity from half a drachm to a drachm, as the injection used. Neither is there anything new in attempts to obliterate the ring by adhesion or destruction of the sac. Such were, in the latter part of the last century, the ligature or excision of the sac and testis, by which "the Bishop of St Papoul found that more than five hundred children had been castrated in his diocese;" and the *royal stitch*, which, embracing the sac, preserved the testis to fulfil its legitimate function of making subjects for the king; and later, the operations which plugged the ring with a piece of the scrotum, and that which irritated it with gelatine threads, or acupuncture, and others, which have been for the most part abandoned.

The present patient, a young man of 21, healthy and of good habits, has had a left inguinal hernia for three years. Within the last year he has worn a truss, the hernia being often troublesome and tender notwithstanding. It is now, when allowed to descend, an enterocele of the size of a goose's egg, easily reducible, the ring readily admitting the middle finger; and under these circumstances the patient applied for a radical operation. I stated to him that the operation was not dangerous; that it probably would not cure him, though it might alleviate the inconvenience; the last perhaps greatly, perhaps not at

all. The instrument used, and which was made for me several years ago, consists of a minute silver syringe terminating in a fine tube. The latter carries at its point a perforated trocar, which serves at once to make the puncture and to deliver the injection. With this instrument, twenty-five drops of tincture of iodine were deposited at the ring itself, through a puncture in the skin made with a tenotomy knife. I will not undertake to say that I injected the sac. When the sac is thin, I do not believe it possible to say whether the instrument enters the sac, or whether it pushes the sac before it. You may perhaps transfix it literally; but there must be, in general, an uncertainty whether the injection actually penetrates the sac, or only bathes its exterior; and practically the difference, in producing inflammation, whether from contact or from continuity of tissue, must be of no great importance. The result of the operation may be considered as a question of theory and of fact. This process aims to obliterate or plug the ring by an effusion of adhesive lymph. Now the cause of hernia is a want of resistance in the tendon; and as we cannot make new tendon, the question is, how far lymph is capable of supplying its place. Lymph is a plastic material; liable to great absorption, and having a tendency to yield to pressure. It has very little of the resisting property of tendon. Most patients are obliged to wear a truss after the operation for strangulated hernia, which creates a considerable effusion of lymph. The tendency of most irreducible herniæ, where the ring is plugged by its adhering contents, is to increase. But theory should never stand in the way of fact. If it were possible to get at a series of statistics of this operation, the result would be conclusive. But in the absence of these, I will give the grounds for my own conclusions in respect to it.

1. I have operated in a number of cases, sometimes with relief, sometimes with none. In one case of a young child, the pressure of a light truss after the injection of ten drops tr. iodine, produced a small slough of the integuments.

2. I have been not unfrequently applied to, in common with other surgeons, by patients who had undergone the operation once, or even twice, to know what benefit would be likely to result from an additional operation.

3. A maker of trusses informs me that he frequently receives applications for trusses from patients unsuccessfully operated on; or where the relief was only temporary. On the other hand, it is quite probable that lymph diminishes the size of the tendinous aperture in certain cases, and sometimes to a considerable degree. In fact, I know patients thus operated upon several years ago, who believe that the liability to a descent of the hernial contents has been materially diminished in their cases, and who consider their condition improved by the operation, though they still wear a truss.

Now, under these circumstances, if there is no great danger attending the operation, it is justifiable; and I never heard of a fatal result from it; though peritoneal inflammation is occasionally quite considerable. So that a patient who desires to encounter this operation, not dangerous in itself, for a chance of obtaining greater or less relief from an inconvenience, may be gratified.—*Dr Bigelow's Clinical Lectures, Boston Med. and Surg. Journal*, December 1850. P. 339-40.

ON HYDROCELE OF THE NECK. BY PROFESSOR MUTTER.

Professor Mütter relates four cases of this affection, and subjoins a short account of its symptoms and treatment. The first case was a congenital one, and consisted of numerous sacs not having intercommunication, some of which reached a very large size, and, encircling the throat and chin, embarrassed the respiration. When they did so to a great extent, one of them was punctured with great relief, and that on two or three occasions. The child, however, died from œdema of the glottis, probably brought on by the compression of the distended sacs, which were found to be both large and numerous. The three others were acquired, and were all, as is the case with the bulk of recorded instances, situated on the *left* side. In one, in which a radical treatment was

declined, forty-four ounces of a chocolate-coloured fluid was drawn off, and several months elapsed before re-accumulation. In another case, a seton was introduced, which produced no bad symptom, and was followed by a radical cure. In the last case this was obtained by dissecting out the cyst, which was only the size of a walnut.

Speaking of the *treatment* of this disease in general, Dr Mütter observes, that the operation of *excision* should be confined to cases in which the tumour is small, circumscribed, and superficial, and when a small scar is not considered of importance, under which circumstance it is the preferable plan. *Repeated tappings*, care being taken to empty the sac of all fluid, lest it infiltrate into the cellular tissue, may be employed as a *palliative* in congenital hydrocele, when severer measures might be undesirable, and may sometimes effect a radical cure by keeping the sac constantly empty. In old cases it may be resorted to to relieve cogent symptoms, or when the patient will not submit to other means. The chance of a radical cure is increased by scratching the interior of the sac with the trocar before withdrawing it; and after the operation, moderate pressure should be kept up over the sac. The shortest plan of curing one of these tumours, if superficial, is *free incision of the sac*, following it by moderate compression; but the long and unsightly scar it leaves would be with many an insuperable objection. In order to convert the surface into a granulating one, several surgeons have combined with incision the *application of stimulating substances to the sac*, as lint, iodine, wine, &c. This, however, excites great inflammation, is followed by a large irregular cicatrix, and requires much time. *Injections*, Professor Mütter states, are abandoned, on account of the inefficiency of weak, and the danger of strong, ones. The *seton* is the best means for cases in which extirpation, repeated tapping, or incision, are inappropriate or have failed. No means is so promising, when the disease is unilocular and of long-standing. The treatment occupies several weeks, perhaps, but the method is safe, easy, slightly painful, certain, and followed by scarcely any scar.—*Philadelphia Medical Examiner*, N.S., vol. vii., pp. 257-274.—*British and Foreign Medico-Chirurgical Review*, January 1851.

[This extract curiously contains all the means that ought not to be used for remedying the disease in question, and omits the only mode of treatment which can be deemed easy, safe, and effectual. This is the injection of the tincture of iodine without admixture of any kind. In Mr Syme's clinical lecture the reader will find a case of the kind, in which the sacs, though very large, and containing a bloody fluid, were thus obliterated in the short space of three weeks.]

ANKLE-JOINT AMPUTATION. BY PROFESSOR HEYFELDER, OF ERLANGEN.

Cunigunde Schardt, of Michelau, 25 years old, married, of healthy build, had, when 10 years old, suffered from an inflammation in the chest; she first menstruated at the age of 16, and at 19 became pregnant for the first time. After an ordinary labour, she bore a healthy male infant, which she continued to nurse for nine months. Three years ago she bore another strong child, which she also nursed. Eight weeks after her last confinement, she became affected, without any particular cause, with lancinating pains in the left ankle-joint, and with swelling of the surrounding soft parts. These symptoms were increased by motion, but alleviated by prolonged rest. After three months had elapsed, fluctuation was felt about the outer malleolus, and much thick pus was evacuated by incision. Afterwards, ulceration and discharge of pus took place about the inner malleolus. Pus was subsequently discharged by different openings which took place spontaneously on the back of the foot, and from which bits of bone now and then came away. For a year past she had been unable to stand upon the foot; and as her strength was obviously failing, she entered the clinical ward of Erlangen on the 26th June 1849.

On the dorsum of the foot, about the tibio-tarsal joint, there were three openings, and on the sole of the foot a fourth, from all of which thin ill-con-

ditioned pus flowed. The instep was considerably swollen, and painful when attempts were made to stand or walk. Exploration with the probe revealed extensive caries of the os naviculare, the os cuboides, the calcaneum, and astragalus. Hence amputation at the ankle-joint seemed the only means of affording relief. This was accordingly had recourse to on the 27th June, the patient being put under the influence of chloroform, and the operative procedure of Syme being followed. The condition of the soft parts was favourable to the construction of the flap from the skin detached from the heel. The hemorrhage was trifling. The anterior and posterior tibial arteries were tied. After the articulating extremities of the tibia and fibula were sawed away, the posterior flap was brought into contact with the anterior edge of the wound by five stitches, and cold water dressing ordered.

When the dressings were changed, on the fourth day, union was found to have taken place in the whole track of the incision, except at the point where the ligatures hung out. These detached themselves ten days afterwards. In four weeks the patient was perfectly cured, so that she could, without pain, stand and walk upon the stump.

The anatomical examination of the parts removed exhibited caries of all the bones of the tarsus, especially of the ossa naviculare and cuneiform primum. The first metatarsal bone likewise was carious; the other metatarsal bones were loosened, and their upper surface roughened with bony deposit. The caries had also commenced in the removed portions of the tibia and fibula.—*Präger Vierteljahrschrift*, 1850, 2tes Band, p. 17.

MIDWIFERY AND DISEASES PECULIAR TO WOMEN.

CASE OF SUDDEN DEATH SEVEN HOURS AFTER DELIVERY—AIR FOUND IN THE HEART. BY MR BERRY.

E. G., aged 22, pregnant with her first child, was taken in labour on the evening of June 16, 1830. The pains at first were slight, and continued so during the night. In the morning of the 17th she became worse, and at two o'clock P.M. the pains were so strong as to induce her to send for her medical attendant, Mr Dyer, who remained with her till seven o'clock in the evening, when she was delivered of a male child. In twenty minutes after the birth of the child, the placenta was naturally expelled, with but little loss of blood. At half-past eight o'clock she was very comfortable, and at ten o'clock was carefully put to bed. At eleven o'clock she took some gruel, and expressed herself "as feeling as comfortable as she could expect." Her husband then lay by her side. About one o'clock in the morning of the 18th, he became alarmed by her difficult breathing, and feeling of faintness. He immediately sent for Mr Dyer, but before he arrived, at two o'clock, she was dead. She lived seven hours after her delivery. The cause of death could not be accounted for, as there was no hemorrhage, and apparently nothing in the condition of the patient to prognosticate such a termination. A post-mortem examination was allowed, and was made early in the morning of the 20th. The body was well formed and nourished. A thick layer of fat existed in the abdominal coverings. Upon opening the abdominal cavity the uterus was seen midway between the pelvis and umbilicus; the peritoneum covering it and the intestines was healthy, but pale. The stomach contained a small quantity of fluid. Liver healthy. The kidneys presented a granulated appearance; and the urine which remained in the bladder was ascertained to be, by the application of heat, slightly albuminous. Upon cutting into the uterus, it was found empty, and the vessels where the placenta had been attached patulous. The vagina contained, at its superior part, a moderately-sized clot of blood. Within the chest both lungs were congested, and contained scattered tubercles in their upper lobes. The heart was the size of a male heart, and apparently dis-

tended. Upon making an incision into it, a gush of air escaped, and the organ became flaccid; no blood was found in its cavities. About one ounce of serum was observed in the pericardium. The brain was healthy in every respect. No signs of decomposition existed in any part of the body.

Olivier, in the article "Air," in the "Dictionnaire de Médecine," p. 73, when referring to Legallois' experiments on the inferior animals, in which he had found sudden death occur from air penetrating into the vena cava inferior and heart, by the uterine veins, in female pregnant animals, asks the following question:—"Is it to a cause of this kind that we ought to attribute the sudden and unexpected death in females lately delivered, and where the autopsy disclosed nothing which could account for such a catastrophe." Does the case above detailed answer the interrogation of Olivier? The labour was natural, and concluded within the ordinary period; the placenta was expelled with but little loss of blood; every thing went on well for some hours, when suddenly the respiration became obstructed, and death followed in a very short period. Upon examination, nothing could account for these symptoms but the congested state of the lungs, and the air found in the heart. The mode in which the air effected its entrance appears to me to be as follows:—The uterus, after the removal of the placenta, contained a clot of blood, with the contraction of the uterus the clot was forced into the vagina, when the uterus relaxed air was drawn into its cavity, the clot in the vagina prevented the air from passing out of the uterus easily, and the mouth of the uterine veins being patulous, it was forced into them, and then passed into the general venous circulation. The entrance of the air was, in all probability, gradual, and, therefore, occupied some time before it reached the heart in sufficient quantity to give rise to those symptoms of obstructed respiration, which soon terminated in death. Dr Simpson, in a communication to the late Dr Reid on this subject, has expressed himself as follows:—"As to the mechanism of the introduction of air in such cases, supposing that to be the cause, I think we can understand it when we remember that the interior of the uterus after delivery, especially opposite the late seat of the placenta, is studded with venous orifices, and that if the air does once become introduced into the uterine cavity, from relaxation of the walls of the organ, it will be liable to be forced into these orifices, and hence into the general venous circulation, provided the uterus, in again contracting, is unable to expel its contents through the os uteri."—*Prov. Med. and Surg. Journal*, Nov. 27, 1850.

INTERVAL OF SEVERAL WEEKS BETWEEN THE DELIVERY OF TWINS. DR NEVINS.

Dr Nevins related to the Liverpool Medical Society the case of a woman who was delivered of a premature fœtus, and, after a period of six weeks, was confined of a full-grown child, which was still-born.

The case gave rise to a conversation on the subject of super-fœtation, in which Dr Lancaster said that it was reported to be not uncommon in Australia. When he first arrived there he did not believe in its existence; but so many cases were related to him by surgeons, of full-grown children being born some weeks after delivery of apparently full-grown fœtuses, that he could not doubt nor dispute the evidence.

Dr Macnaught had made careful inquiry on the plantations in Jamaica, on which cases of super-fœtation, related by authors, were said to have occurred, and he could gain no information of any such events within the recollection of any living person.—*Medical Gazette*, December 6, 1850.

ON CHLOROFORM IN MIDWIFERY. BY DR BEATTY.

Further information on the value of chloroform in obstetrical practice has been afforded by Dr Beatty. The result of his inquiries appears to be entirely confirmatory of the opinions put forth by Dr Simpson, and he especially states, that when properly exhibited, no danger accrues either to mother or child.

The reason why the parturient woman so seldom suffers from the use of chloroform in comparison with surgical patients, he states to be, in his opinion, that the former inhales it in the recumbent position, in which a minimum quantity produces the desired effects; and, secondly, that her stomach is seldom in a state of repletion—a condition which is now known to facilitate the occurrence of unfavourable symptoms. The grand desideratum, however, is the purity of the agent.

As regards its administration, Dr Beatty informs us that he always uses a handkerchief. In ordinary cases he commences the inhalation when the os uteri is nearly dilated. In ordinary labour he never produces insensibility, but removes the handkerchief as soon as the breathing becomes sonorous. In this way the patient is never deprived of consciousness, but she is relieved from the severity of the pains.

The main object of Dr Beatty's communication is, however, to show how chloroform may be used with advantage in lingering labours with inefficient pains, by combining its use with that of ergot. He narrates several cases in point, in which he first gave ergot to rouse the uterus to action, and then nullified the attendant pain by ordinary anæsthesia.—*Dublin Quarterly Journal.*—*Provincial Journal* for January 1851.

MATERIA MEDICA.

MURIATE OF LIME IN CERTAIN DISEASES OF THE SKIN. BY M. CAZENAVE.

Muriate of lime (chloride of calcium), which exists in many mineral waters, is a salt rarely employed in medicine. It is, however, according to M. Cazenave, a valuable remedy, which may, in many cases, replace the iodide of potassium, and which has the advantage of being easily borne, and may be continued for a length of time. Biett had employed it in scrofulous lupus. M. Cazenave uses it extensively, especially in those forms of disease in which the lymphatic temperament predominates; for instance, in chronic eczema, and especially in impetigo, in doses of from fifteen to thirty grains daily. He prescribes half-an-ounce of crystallised chloride in eighteen ounces of water, a table-spoonful to be taken two or three times daily in a cup of some appropriate decoction.—*Annales des Maladies de la Peau*, October 1850, in *Bulletin de Thérapeutique*, November 1850.

CHEMICAL EXAMINATION OF SQUILL. BY G. C. WITTSTEIN, OF MUNICH.

Fresh squill contains a volatile, alliaceous-smelling substance, probably a sulphurated ethereal oil, which has not yet been isolated. The substance which especially confers upon it its medicinal qualities is scillitine,—a bitter matter, yellowish, hygroscopic, and more sparingly soluble in water than in alcohol. The acrid taste and irritant action of squill upon the skin does not depend upon the scillitine, but upon a very acrid matter accompanying it, and which is separated from it when the watery solution of the extract is precipitated by acetate of lead, and the excess of lead removed by sulphuretted hydrogen. The acrid matter forms an insoluble compound with the lead, and acts the part of an acid; the bitter matter, scillitine, on the other hand, is of a neutral nature. Neither of these principles is volatile, therefore both of them are found in the dried root; but their properties are masked by the large quantity of gum which is present.

In order, therefore, to obtain the active principles, the dried squill must first be treated with water; the watery infusion then freed from gum by means of alcohol, and the fluid finally evaporated. The author proposes, in this way, to prepare a "spirituous extract of squill," for medicinal use.—*Buchner's Repertorium*, iv., 1850.

[The dose of crude squill is so small, that little necessity exists for a preparation of this concentrated nature. It is to be regretted that Dr Wittstein,

when investigating this subject, had not made a more precise examination of the so-called scillitine, the real nature of which is still unknown.]

CONCENTRATED SOLUTION OF TANNIN IN AFFECTIONS OF THE EYES. BY M. HAIRION.

In all formularies there are to be found collyria and injections of tannin, but in all these prescriptions the solution is weak, being about one or two parts to 120 of water. There is, therefore, nothing new in the employment of tannin as a collyrium. The peculiarity of M. Hairion's practice consists in the strength of the preparations which he employs; thus his ordinary solution is one part of tannin to three of distilled water; sometimes he employs it in fine powder, or in ointment, or in the form of a thick magma. He has used it in chronic and acute conjunctivitis, "vegetating granulations," corneitis with and without ulceration, chemosis, and pannus; and it is in this last affection that he has met with most success from its use, a complete cure having been effected in some cases very rapidly. In "vesicular granulations" he has had less success, especially when they are at their height. M. Hairion, after a very enthusiastic laudation of its utility, states that he has never met with any occurrences which could lead him to regret having used these large doses; it has never caused the least [inflammatory] reaction; it has never produced any caustic effect, nor given rise to indelible incrustations; and, moreover, its application is not painful to the patient.—*Archives de Méd. Belges, in Journ. de Pharmacie*, Dec. 1850.

[M. Hairion's strong solution of tannin is probably safe enough, and may answer well in some instances; we have used it in the proportion of one part to eight in chronic corneitis, with apparent advantage; but our experience of it is too limited to entitle us yet to pronounce on its value. We fear that our author's partiality to it leads him very much to exaggerate its utility.]

RESULTS OF THE USE OF CHLOROFORM IN 9000 CASES AT ST. BARTHOLOMEW'S HOSPITAL.

One of the most interesting questions connected with the subject of operative surgery, relates to the use of anæsthetic agents employed for the purpose of suspending the function of sensation. This question has assumed a moral, as well as a medical type. It has been urged, that sensation is a natural function of the living organism, and that to suspend it by artificial agency is to set at nought the ordinances of nature; and that man is born to suffering, as evidenced by the sensibilities of his body. If the soundness of this argument be admitted, it would be difficult to draw a line which would define the boundary at which moral and *immoral* suffering meet; or to say, in what form of suffering our remedial agents may be justifiably resorted to. The sensibilities of our frame are not given us by nature to the end of promoting pain, but to enable us to avoid it. Corporal suffering is no part of the discipline of the mind; nor can it even be generally asserted, that its excess exercises a salutary influence on the character. Every movement of our body instinctively points to the avoidance of bodily suffering; why, therefore, should we not as readily and unobjectionably employ the agency of anæsthetic medicines for the purpose of suspending bodily pain, under the circumstances of an otherwise painful operation, as we endeavour to mitigate the bodily suffering of any other patient, cast down on a bed of sickness? Will not the objection to the anæsthetic action of opium to a region affected by a neuralgic pain, or to the system generally, hold as strongly, as that of another agent of the same principle, given to avert the pain of an operation?

The medical arguments against the use of anæsthetic agents, have a somewhat better foundation. That great and sudden determination to the brain, and an unnatural circulation of venus blood, results from their employment, is undeniable.

It is undeniable, if the quantity administered be large, and long continued,

that symptoms resembling those of apoplexy present themselves, in the form of extreme congestion of the vessels of the face, stertorous respiration, and total insensibility; and it cannot be denied that occasionally its full administration leads to headach, vertigo, and langour of some days' duration; and cases are recorded in which death itself has followed in the course of an hour or more after its employment. It must be observed, however, in pursuing this question in strict accordance with the laws of evidence, that we have no *proof*, in the cases above referred to, that death was the direct effect of the supposed cause. The parties administering it, were not fully experienced in the mode of its application. They entertain the *opinion*, that death was referable to it, while it cannot be disputed that the fatal issue may be attributable to other causes: and, in one example, it appears more reasonable to refer the death of the individual to a suspension of the function of respiration by violence, than to any obnoxious agent circulating through the lungs, or brain. On the other hand, the records of St Bartholomew's Hospital point to its successful administration in upwards of nine thousand cases; in not one of which, including the aged and the young, the healthy, the infirm, and the asthmatic, has its employment left a stain on its character, as an innocuous agent of good. Under all circumstances, its careful employment may be unhesitatingly resorted to in all cases, excepting only such as are marked by determination to the brain of an apoplectic type; secondly, under circumstances of great and serious exhaustion from loss of blood; and, thirdly, in diseases of the heart. In these conditions of the system, it is perhaps better avoided.

The agent in general use is chloroform, and one word may be added as to its administration. It appears indisputable that its influence on sensation, precedes that on consciousness. I have employed it on several occasions, in which a patient has been conscious of all that has been passing around, and yet who has declared himself to have been totally insensible to pain. This state of his system has arisen from the moderate use of the agent, ample, indeed, for all purposes of utility, though somewhat difficult to regulate in quantity sufficient for the required object.

I prefer its gradual administration. I do not think it desirable to exclude atmospheric air, employed as a diluent during the process of inhalation. Its influence should be gradual, not sudden. I consider its application through the medium of a cambric handkerchief laid on the face, preferable to the use of instruments made for the purpose of excluding atmospheric air, and food should be rigidly avoided before its administration, otherwise sickness will frequently follow.

Against the occasional convictions or objections of others to its employment, I place the strong, and to my own mind the unanswerable fact, that it has been successfully used in so large a number of cases in St Bartholomew's Hospital since the period of its introduction; that these cases have been indiscriminately taken, and that its objections have not yet made their appearance before the observant eyes of the medical staff of that institution, either by promoting danger during the operation, or protracting the recovery of the patient after it. In one class of cases its employment is especially applicable, viz., in that form of disease in which the pain of an operation is the chief warrant for its non-performance, and in which the recovery from a chronic disease is left to nature, that might be greatly hastened by the hand of art; such, for example, as the removal of a piece of dead bone.

Up to the period of the introduction of chloroform, a surgeon was very unwilling to subject a patient to the painful process of sawing and chipping away portions of dead bone, with a view to reach the medullary cavity, because the operation was both a painful and a protracted one. The consequence was, that an hospital bed was occupied by a patient thus affected, for many months, to the exclusion, perhaps, of three or more claimants, who

would have successively occupied it. But by the aid of chloroform the operation is now performed unconsciously to the patient, and the period of his recovery greatly abridged. With the three exceptions above mentioned, I cannot hesitate in strongly recommending its administration in all cases of large surgical operations, believing its discovery to be the greatest blessing conferred on the profession of surgery during the last century; and although I have seen its employment pushed, on many occasions, to the apparent verge of apoplexy, I cannot say, even in such examples, that the good has not largely predominated.—*Skey's Operative Surgery*, pp. 16, 17, 18.

CHLOROFORM AND ETHER. DR REESE'S NOTES OF HOSPITAL PRACTICE.

The employment of these anæsthetic agents by inhalation has been signally successful in every department of hospital service.

Our method of employing these agents has been either by applying to the mouth and nostrils a hollowed sponge, moistened with ʒj. or ʒij. of chloroform, or ʒss. of the mixture; or, by means of a towel or handkerchief in lieu of the sponge, and which, on some accounts, is to be preferred. In no case have we persisted continuously to apply either of these agents so as to exclude the atmospheric air, but have always allowed the alternate inhalation of the air and that of the chloroform or ether; and our rule has been to remove the agent from the mouth and nostrils as soon as anæsthetic effects have become manifest, re-applying the sponge or towel occasionally, if necessary. With these precautions, our experience has been so entirely free from any untoward or unpleasant consequences, that we can scarcely feel the force of the objections recently urged against anæsthetic agents, and are constrained to apprehend that the mischiefs described have resulted either from the want of due caution in their employment, or the lack of discrimination in the subjects, by overlooking existing pathological states which contra-indicate their use. Certainly, we can have no sympathy with the surgeon whose unsuccessful operations are ascribed by himself to etherisation; much less is any man authorised to attribute the want of success in another to the fact that either chloroform or ether had been used in the case.

The following are a few of the specialities in which these agents have been employed here, namely:—

1st. In *reducing dislocations;* and here our experience has proved the invaluable importance of the discovery which has furnished to the profession these means of completely relaxing the muscles and ligaments of the larger joints. In several instances, luxations at the shoulder and hip of long duration, which, after repeated trials, could not be reduced by any amount of physical force applied in the ordinary way, were, after the inhalation of chloroform by the patient, found capable of ready reduction with the thumb and finger. If no other good were conferred by chloroform, this alone would render the discoverer a benefactor to the world.

2d. In hysterical, epileptic, and puerperal convulsions, we have had frequent opportunities of witnessing the speedy and effectual relief afforded by the inhalation of chloroform and ether; and, in the latter of these forms of disease, after extensive venesection and the other most potent antispasmodics had been tried in vain.

3d. In delirium tremens, we have frequently been able permanently to calm a violent patient, and induce profound and protracted sleep, after both lupuline and opium had failed.

4th. In paroxysmal or spasmodic asthma, a very few inhalations of ether or chloroform, without even approaching full anæsthesia, will be found more effectual for relief than any other remedy.

In tedious, protracted, or severe labour pains, and especially when mal-presentation, or other causes, render painful operations necessary, whether manual or instrumental, the inhalation of chloroform, with the precautions already named, has been found to be uniformly safe, and pre-eminently useful, both to

the mother and child; while it divests the most formidable operations of obstetric surgery of their terrors, alike to the physician and his patient. We have not encouraged the use of this agency in natural labours, unattended by any considerable severity, although in these cases no evil has happened to mother or child in our hands; but, in all cases in which it has become important to lessen the sufferings of the mother, we have uniformly administered the chloroform, and have had every reason for undiminished confidence in its innocence and utility; indeed, we had good ground to believe that in several cases our patient would have died undelivered if we had been without chloroform; and yet, in more than one of these, the child as well as the mother was saved. Nor have we been able to detect, in a single instance, any subsidence or diminution of the labour pains, either in frequency or force, during the moderate degree of anæsthesia, which is all that is necessary for any useful purpose in obstetrical practice. And yet we have been obliged, in some severe labours, to continue the repetition of the inhalation during several hours—applying the chloroform to the nostrils and mouth for a few moments only, however, at the commencement of every pain—our patient importunately pleading for it, after having felt its efficacy in relieving her sufferings. Such has been our hospital experience in certainly one of the largest lying-in departments in our country.

Surgical Operations.—In all cases, when the severe or formidable operations of surgery have been called for, whether by the knife, saw, or chisel, or red-hot iron, we have not hesitated to give our patients the benefit of chloroform; and, in all this variety of infliction, we have witnessed entire immunity from pain, and in most cases there has been no consciousness of the operation. The limb has remained unheld and unmoved while the actual cautery has been made repeatedly to traverse a joint; and in one instance of the amputation of a thigh, the patient remained ignorant of his mutilation until the fifth day, when it became necessary to dress the stump, he all the while supposing that his leg was bound up in splints, and complaining of that very common, though imaginary, sensation of the toes of the amputated limb. The success of operations, and the early healing of the wounds thus made, have in no instance been hindered or delayed, so far as we could perceive; and we have even surmised the contrary. In primary amputations, after compound or comminuted fractures, when these are judged necessary, the additional shock to the nervous system, so much dreaded, is in a great measure obviated by the judicious use of chloroform; and, whatever may be the prejudice existing or engendered by panic-makers, who are for the most part mere theorists, the extensive experience and success of practical men, here and elsewhere, should disabuse the profession and the public of the erroneous views which have been promulgated of late, on the basis of a few unsuccessful and probably indiscreet experiments, and isolated cases of unfortunate termination. The profession should not only allow their patients the advantages derived from this new agency to protect them from suffering, but they should feel that they are not at liberty to hazard the safety of their patients by withholding from sufferers of every class the benefit of anæsthesia, now that science has conferred this boon upon afflicted humanity.—*Dr Reese*, in *American Journal of Medical Sciences*, Jan. 1850.—*London Medical Gazette*, Jan. 10, 1851.

[We have noticed lately, in various Journals and essays, a statement to the effect, that it has been proposed by some of our American brethren to dilute chloroform before using it, with the addition of two or three parts of sulphuric ether, or to use it mixed and diluted with alcohol, in the form of chloric ether. Some pamphlets have been sent to us, showing that one or other of these mixtures is imagined to be a very great improvement upon the use of chloroform in an isolated and undiluted state, inasmuch as (it is alleged) the sulphuric ether or the alcohol dilutes the chloroform, so as to render it less strong and less dangerous in its exhibition. Now, setting aside the objections to such mixtures that arise on the score of bulk, smell, &c., we would suggest to our

American brethren, whether the same object is not far more easily and directly attained by the mode of anæsthetic practice which has always been followed in Edinburgh. The vapour of chloroform ought, say our American brethren, to be rendered less powerful, by diluting it with the vapour of sulphuric ether or alcohol. The vapour of chloroform, we say, may be made, and in practice is constantly made, less strong at any time at will, and, in fact, is always reduced to any amount of strength which may be considered requisite, by diluting it during its exhibition with the vapour of common *atmospheric air*. In short, admitting a greater or less quantity of air to be mixed with the vapour of chloroform, at the time of its administration, by keeping the handkerchief or sponge that is used, at a greater or less distance from the mouth and nostrils of the patient, effects exactly the same object as diluting the vapour of chloroform with the vapour of sulphuric ether or of alcohol, by mixing these ingredients in a liquid form before their exhibition. Nay, we believe it to be a far superior mode of diluting chloroform, when dilution of it is thought necessary; because, assuredly, the vapour of atmospheric air is not so irritating to the chest, as the vapour either of sulphuric ether or of alcohol; and atmospheric air, when breathed, leaves less exciting effects upon the chest and system, than either the vapour of sulphuric ether or of alcohol. We do feel surprised that our American brethren should not have seen this very evident answer to their alleged objections against using pure chloroform. We shall only add, that the proposals we have adverted to, have come principally from the members of the Medical School of Boston; and, after all, we cannot wonder at them hesitating to put aside altogether sulphuric ether, seeing that their school had the glory of first introducing it into practice.]

ON THE USE OF CHLOROFORM. BY PROFESSOR GROSS, OF NEW YORK.

I have used chloroform extensively during the last two years, and am disposed to believe that, as an anæsthetic agent, it is, in every respect, preferable to sulphuric ether. The first case in which I exhibited it was that of a coloured woman, who had an enormous tumour of the thigh, involving the femoral artery. The operation was performed in February 1848, and, although necessarily tedious, was unaccompanied by the slightest pain. The patient soon fell into a tranquil sleep, and so continued during the dissection. Since that period I have employed chloroform constantly in all my most important operations, and have never, except in one solitary instance, witnessed any disagreeable effects from it. The case to which I refer was that of Benjamin Cunningham, of Western Virginia, sixteen years of age, who had a large tumour on the left side of the neck. He was tall and slender, and of a feeble, delicate constitution. The operation was performed before the medical class of the University of Louisville, in November 1849, and lasted less than five minutes. The tumour weighed nearly six pounds. The quantity of chloroform administered was very small, not certainly exceeding one drachm and a-half; it was inhaled from a small cup-shaped sponge. The patient became affected in a few seconds, falling into a profound sleep, and thus continuing for fifty-five minutes. The pulse during the whole of this time was small and feeble, but perfectly distinct; the countenance was pale; the eyes were turned up, and the pupils dilated; and the extremities were cold. There was no vomiting, and apparently no nausea. As soon as the excision of the tumour was over, and several small vessels tied, the youth was removed from the amphitheatre into my private room, where, under the influence of cold air, of volatile alkali freely applied to the nose, of sinapisms to the præcordial region, and of warmth to the feet, he gradually awoke to consciousness. It may be observed here, that the utmost care was taken, during the performance of the operation, to prevent the entrance of air into the veins of the neck. Had this precaution been neglected, it might have been difficult to determine whether the effect in question was produced by this cause alone, or partly by it and partly by the chloroform. Cunningham rapidly recovered from the operation without an untoward symptom.

This is the only instance, out of several hundred, in which the use of chloroform at all alarmed me. For a time it was uncertain how the case would terminate; and I must confess I felt greatly relieved when I saw the young gentleman regain his consciousness.

I have administered chloroform at almost every age, from that of sixteen months up to that of seventy years, in both sexes, in slight operations as well as the most severe, and under various circumstances of health and disease. In some of my cases it has been found necessary to keep up a sustained action of the remedy for nearly half an hour.

I have remarked that there is much diversity in different individuals, in relation to the susceptibility of the system to chloroform. In some, as in young Cunningham, the smallest quantity sufficed to produce the desired effect; while, in others, a large dose was necessary, as well as a longer exhibition of the remedy. Children usually require very little, much less, comparatively speaking, than grown persons, and in them the effects also seem to be less evanescent.

I have not noticed, in any of my cases, that the use of chloroform exerted any injurious effect upon the recovery of my patients; that it excited an unusual degree of fever or local irritation; or that it retarded, impeded, or prevented the adhesive process. I have repeatedly witnessed the occurrence of erysipelas after it, but *not* more frequently, I think, than would have taken place had the operation been performed without it. The excitement consequent upon the administration of the remedy has usually passed off in a short time; and in no instance have I witnessed any secondary effects that were justly ascribable to its influence.

In several of my cases transient opisthotonos appeared to have been produced by the inhalation of this article. This phenomenon was strikingly displayed in a young gentleman of Covington, Ky., to whose hip I applied the actual cautery on account of coxalgia. While under the influence of chloroform, which he inhaled rather badly, his head was forcibly thrown backwards and the body forwards, precisely as in opisthotonos. The effect did not last longer than fifteen or twenty seconds. No stiffness of the jaws was noticed.

Since I have commenced the use of chloroform, I have abandoned that of sulphuric ether altogether, being persuaded, from ample experience, that it is, in every respect, preferable. It produces the desired effect in a much shorter time, causes less excitement of the brain, is more persistent in its action, and is more easily inhaled. In a word, I have every confidence in its efficacy and safety, and could hardly imagine a more desirable anæsthetic agent.

Finally, my rule always is to bring the system gradually, not rapidly, under the influence of this remedy,—an object which is usually attainable in thirty or forty seconds, provided the patient gives us his thorough co-operation, and does not embarrass us by his obstinacy or timidity.—*New York Register of Medicine and Pharmacy*, January 1851.

Part Fifth.

MEDICAL NEWS.

EDINBURGH MEDICO-CHIRURGICAL SOCIETY.

MEETING II.—*December* 18, 1850.—JAMES SYME, Esq., President, in the Chair.

ON LEUCOCYTHEMIA. BY DR BENNETT.

Dr Bennett read a communication on leucocythemia, or excess of colourless corpuscles of the blood. This communication has since been published. (See Monthly Journal of Medical Science, January 1851.)

Dr Keiller recollected a case from Dundee, which there was great reason to

believe must have been of the same nature as those just detailed by Dr Bennett. His friend, Dr Paton of Dundee, would probably be able to furnish the particulars.

Dr W. T. Gairdner made some observations on the interest which these cases possessed in connection with the theory of phlebitis and of purulent deposits. He also observed that caution was necessary in inferring, that the condition of the spleen alluded to by Dr Bennett was very common in the East. Dr Bennett had remarked, that the common enlargement of the spleen in ague was not attended with this peculiar alteration of the blood; and the concurrent testimony of Indian practitioners established the connection of enlarged spleen with ague in India; so much so, that in a late report to the Government, the amount of malaria in certain districts in Bengal was tested by the examination of the per centage of cases of enlarged spleen to be found in the population.

Professor Goodsir remarked that Dr Bennett had indicated a probable source of the white corpuscles in the cases which he had collected. He asked if Dr Bennett could explain the cause of the apparent diminution in the amount of the red corpuscles, in some of these cases, in which the blood had been analysed?

Dr Bennett alluded to the observations of Gerlach and others upon the blood in the spleen. Small yellow corpuscles were described and figured as occupying the cavities of some of the colourless corpuscles. When these colourless corpuscles were in excess, it was probably in virtue of an arrested or abortive development of the coloured element of the blood.

Professor Goodsir considered this speculation as highly probable, and an important step in the pathology of the disease under consideration. The circumstance that the white took the place of the coloured corpuscles in these cases, was highly interesting in a physiological point of view, tending to establish the connection between the lymphatic and circulating system through the medium of the blood glands.

Dr Christison pointed out the fact, that the analyses of the blood, which had been quoted, did not distinguish the relative proportions of white and red corpuscles. By the process of analysis adopted (and which he himself had been the first to propose) the whole corpuscular elements, both white and coloured, were included together under the head of "corpuscles." The analyses showed an extremely impoverished condition of the blood—a diminution of its corpuscular elements—but did not prove that one of these elements was diminished at the expense of the other.

CASE OF TUMOUR FOR WHICH THE OPERATION OF OVARIOTOMY WAS ATTEMPTED MORE THAN TWENTY-FIVE YEARS AGO, WITH DISSECTION.

In the absence of Dr Myrtle, *Dr Taylor* read the case of Magdalene Berry, which had been for twelve years under Dr Myrtle's observation, and had recently terminated fatally. On dissection, a cicatrix was seen extending from the sternum to the pubes. The abdomen did not appear larger than is usual between the fourth and fifth months of pregnancy. A large tumour was felt, strongly adherent anteriorly to the abdominal parietes, occupying the lower part of the abdomen, and moveable. On opening the abdomen, strong adhesions were found between the cicatrix and omentum. The vessels of the omentum were very large, as was reported twenty-five years ago by Mr Lizars in his account of the operation, which he attempted for the removal of the tumour. There were firm adhesions between the anterior surface of the tumour and the abdominal parietes, and between its superior and posterior surfaces and the large and small intestines. The peritoneal surfaces of the bowels were adherent at various points. The tumour was found to be a fibrous tumour of the uterus connected with its fundus by a narrow fold of peritoneum. Both ovaries were small, and in their proper position. The uterus was atrophied. There was great softening and dilatation of the heart. The patient, while under Dr Myrtle's care, was affected with general dropsy. She had derived marked benefit from

the use of diuretics and purgatives. Death had taken place from apoplexy. In Mr Lizars' work on "Extraction of Diseased Ovaries," it is recorded, that immediately after the operation, which it was deemed imprudent to complete, violent inflammatory symptoms supervened, which left permanently well-marked results notwithstanding the abstraction of 111 oz. of blood within thirty-six hours or so after the operation, as well as the administration of anti-phlogistic remedies. The actual cautery was afterwards had recourse to, as also a seton drawn through the anterior portion of the tumour and superincumbent soft parts. Dr Myrtle's communication concluded with some observations on the difficulty of arriving at a correct diagnosis in cases similar to the above.

The *President* made some observations on the case as an additional illustration of the unjustifiable nature of the operation of ovariotomy. The recovery of the patient, after the multiplied dangers to which she was exposed by the incision, the resulting inflammation and the seton, was very remarkable.

Professor Simpson could not agree with the president in condemning the operation of ovariotomy as in all cases unjustifiable. It had been performed in many cases in which he believed its adoption was unjustifiable; but where the patient was evidently soon to die, in consequence of repeated tappings, or otherwise—and where the question was one of certain and speedy death from the disease, or possible recovery and continuation of life from the chances of the operation—and where, in addition, there was no counter-indication to the operation from adhesions, &c., he believed it might be the duty of the surgeon to afford the patient the chances of escape by ovariotomy; and the actual number of recoveries after the operation, seemed sufficient to justify its adoption under such circumstances.

OFFICE-BEARERS.

The following office-bearers for the ensuing year were elected unanimously:

President.—James Begbie, M.D.

Vice-Presidents.—J. H. Bennett, M.D.; William Seller, M.D.; Douglas Maclagan, M.D.

Councillors.—James Andrew, M.D., Cantab.; J. S. Combe, M.D.; Daniel Kerr, Esq.; Thomas Wright, M.D.; Charles Sidey, Esq.; W. T. Gairdner, M.D.; Alexander Keiller, M.D.; W. Burn Murdoch, M.D.

Treasurer.—Robert Omond, M.D.

Secretaries.—John Taylor, M.D., and William Robertson, M.D.

VARIETIES.

College of Physicians of Edinburgh.—The second of the series of lectures to be delivered this session in the hall of the College, will be given by Professor Bennett, on the evening of Monday the 10th of February. The subject of the lecture will be, "On Colour, as observed in Plants and Animals."

Saline Artesian Well.—The boring of the famous artesian well, the Schonborn, at Kissingen in Rhenish Bavaria, which commenced in 1822, has recently been brought to a successful termination. This immense work, of the success of which people were beginning to despair, has given results never witnessed before. Kissingen is situated in a saline valley about 990 feet above the level of the Baltic. In the month of June 1849, after seventeen years of work, the boring had reached a depth of 1820 feet, having previously passed through several strata of salt separated by masses of granite. There was then encountered, for the first time, a stratum of carbonic acid gas, followed by new granitic masses, and at length on the 12th of June a violent detonation overthrew, but without injury to any body, the scaffolding which covered the mouth of the well, and almost immediately there was seen issuing from the orifice a column of water, nearly five inches in diameter, which rose with prodigious force to the height of 100 feet, diffused itself on all sides like the branches of a magnificent palm tree, and thus formed the most extraordinary *jet d'eau* imaginable. The water,

clear as crystal, issues from the soil at a temperature of 66° Fahr., charged with 3·4 per cent. of pure salt, and gives a volume of 40 cubic feet per minute. It is forced up by an atmosphere of carbonic acid gas acting with a force of fifty atmospheres. The total depth of the well is 2047 feet. It is calculated that this spring will yield annually about 293 tons of salt, which, deducting expenses, will add L.30,000 to the revenue of Bavaria.—From *Gazette Méd.* in *Journ. de Pharmacie*, December 1850.

MUSTACHES.—Our contemporary, the "Union Médicale," announces the discovery, by the "National and Military Gazette" of London (a journal with which we are unacquainted), of certain hygienic virtues in mustaches. These consist in the hygrometric and thermometric properties possessed by the human hairs,—so that, when growing below the nostrils, they absorb the moisture, and prevent cold air from entering the respiratory passages. Statistics prove their value, as they prove everything else. In the regiments that wear mustaches, pulmonary affections, colds, &c., are unknown; whilst in the regiments in which the men shave, catarrhs, pneumonias, coughs, &c., are common!

CHOLERA IN JAMAICA.—In a letter just received from Dr M'Nab, we are told that the cholera has ceased, having destroyed one-eighth of the population.

EX NIHILO NIHIL FIT.—It is difficult to make the public understand that it is impossible, by any refinement of chemistry, to detect anything, except the vehicle, in the homœopathic medicines properly prepared, although some of the homœopathic medicines have been found to contain poisonous doses. A fact has come to my knowledge: A London wholesale druggist informed one of the most respectable citizens of Newcastle, a few days ago, that his firm manufactured sixty pounds weight of homœopathic drugs every fortnight, and sold them to the various homœopathic institutions; and that they were so well aware of the farce, as never even to attempt the troublesome mode of manufacture required by homœopathy, so that the drugs really and truly contain nothing.—*Dr Glover*, in *Lancet* of January 11, 1851.

PENSION TO MRS LISTON.—A pension of L.100 per annum has been recently conferred by Government upon the widow of the late eminent surgeon, Mr Liston.

PUBLICATIONS RECEIVED.

The Anatomist's Vade-Mecum. By Erasmus Wilson, F.R.S. London, 1851. Fifth Edition.

On Vaccination and Revaccination as Preventive of Small-Pox. By Alexander Knox, M.D. London, 1850. (From the London Journal of Medicine.)

Additional Observations on the Nitrate of Silver as a Therapeutic Agent. By John Higginbottom, Hon. F.R.C.S., Eng. London, 1850.

New York Register of Medicine and Pharmacy. Vol. I. Nos. vi. and vii.

On Bengal Dysentery and its Statistics. By John Macpherson, M.D., First Assistant Presidency General Hosp. Calcutta, 1850.

Observations on Chronic Hydrocephalus, &c. By Francis Battersby, M.B., T.C.D. (From Edinburgh Medical and Surgical Journal, Jan. 1851.) Edinburgh, 1851.

Observations on the Epidemic Ophthalmia of the Tipperary and Athlone Unions. By William R. Wilde, F.R.C.S.I., &c. (Reprinted from the London Journal of Medicine, of Jan. 1851.) Dublin, 1851.

On the Treatment of Cancer by the Regulated Application of an Anæsthetic Temperature. By James Arnott, M.D. London, 1851.

The London and Provincial Medical Directory. London. Churchill. 1851.

The Scale of Medicines with which Merchant Vessels are to be Furnished: with Observations on the Means of Preserving the Health of Merchant Seamen, &c. By T. Spenser Wells, F.R.C.S. Lond., 1851.

Braithwaite's Retrospect of Medicine, July to December 1850. London, 1851.

The Philosophy of Vital Motion. By C. Bland Radcliffe, M.B. London, 1851.

Observations relating to the Science and Art of Medicine. By William Wegg, M.D., Cantab. London, 1851.

Remarks on Insanity. By Henry Monro, M.B., Oxon. Part II. London, 1850.

On the Cure of Popliteal Aneurism by Pressure at the Groin. By J. M. O'Ferrall, M.D., M.R.I.A. London, 1850.

On a Remarkable Effect of Cross-Breeding. By Alex. Harvey, M.D. Aberdeen, 1851.

NOTICE TO CORRESPONDENTS.

Communications have been received from Dr Smellie, Wick; and Dr Kirkwood, Berwick-on-Tweed. Others have been privately acknowledged.

Part First.

ORIGINAL COMMUNICATIONS.

ARTICLE I.—*On Fatty Degeneration of the Heart, chiefly in reference to some points of Diagnosis.* By JAMES BEGBIE, M.D., F.R.S.E., Fellow of the Royal College of Physicians of Edinburgh.

(*Read to the Medico-Chirurgical Society of Edinburgh, 15th Jan.* 1851.)

THE injuries and diseases of the brain may be speedily— they are seldom immediately—fatal: the lesions and affections of the heart are often the cause of sudden death.

Yet, the numerous and ever-recurring instances of the extinction of life, which arrest the attention of all minds, by the striking rapidity of their course, or by the fearful suddenness of their issue, have been too readily regarded, both in the popular, and in the professional view, as examples of the apoplectic seizure. The great centre of the nervous system has been too commonly considered the seat of the fatal lesion: the central organ of the circulation has, till lately, been too much overlooked in the large share it has taken in these appalling calamities.

Morbid anatomy has disclosed to us many alterations of the structure of the heart, which have proved the cause of immediate dissolution; and many more, which, either directly or indirectly, have led to a rapid consummation. Our improved means of diagnosis have taught us to anticipate such a termination to many of the cardiac affections with which we are every day familiar: our advancing knowledge of their pathology, and the aids of organic chemistry, may, ere long, point to more successful means for staying the progress, if we cannot avert the issue, of these severe and fatal lesions.

Microscopic observation has recently revealed to us certain remarkable changes in the muscular structure of the heart, of slow and insidious development,—a fatty degeneration of its fibres—a morbid condition most difficult to detect during life, hitherto unassailed by any remedy, and which, there is reason to believe, has been productive of most fatal consequences, and the immediate cause of the sudden and unexpected demise of many distinguished men.

Histologists have recognised two varieties of fatty disease of the heart. In the one form, the fat, composed of oil cells, is deposited

on the surface of the organ; it then gradually encroaches on, and insinuates itself between, the muscular fibres, so as greatly to conceal, and ultimately to impoverish and waste them. In the other form, the fatty matter, composed of granules and small oil-globules, occupies and fills the sheath of what was previously muscular fibre. The one variety consists in the *growth* of fat upon the muscular tissue; the other, in the *degeneration* of the structure itself into fatty matter. The two conditions appear to arise from different causes; the one is the result of an accumulation, in the blood, of the elements of fat, the other is the result of decay and disintegration.[1]

Rokitansky has described a third perfectly distinct species of fatty degeneration, occurring almost exclusively in hypertrophied hearts, which, at the same time, exhibit the remains of earlier endocarditis and carditis. In this form, the fat does not accumulate in masses, there being no fat vesicles inclosed within fasciculi of cellular tissue; but is beaded, as it were, in minute microscopic granules, closely interlaced and embedded among the primitive fibres of the heart's muscles.[2]

The growth of fat upon the heart has been described by some of the earliest writers on morbid anatomy, though they appear to have been ignorant both of its nature and consequences. The fatty degeneration of the muscular tissue has only recently attracted the notice of the British, French, and German authors. In our own country, Duncan, jun., Cheyne, and Adams, were amongst the earliest observers. They have been followed by Smith, Stokes, Paget, Quain, and many others; while, on the continent, the subject has been illustrated by the labours of Laennec and Andral, of Rokitansky and Hasse, and other cultivators of pathological anatomy. It is not my purpose, however, to enter at any length into the literature or history of this formidable disease. The subject is full of pathological interest, but it has already been handled in an able manner by Dr Quain, in whose recent essay will be found an admirable summary of our knowledge up to the present moment. All that I contemplate now, is to review some passages in the life and death of two distinguished men, who have lately passed from amongst us, with the view of assisting to solve the question,—Are there any symptoms diagnostic of fatty degeneration of the heart, by which its existence can be inferred, with anything like certainty, during life?

Dr Chalmers died in his sixty-eighth year. It was my privilege to know him well, and to see him often, during the last fifteen years of his life. The world knows his genius, his talents, and his labours. To a mind of the highest order, and of wondrous energy, he united a hale and vigorous, a manly and robust, frame of body; and in the pursuit of the best interests of his fellow-men, he spared no exertion,

---

[1] Quain. Medico-Chirurgical Transactions, vol. xxxiii. p. 151.

[2] Manual of Pathological Anatomy (Sydenham Society), vol. iii., p. 315. Hasse's Pathological Anatomy (Sydenham Society), p. 170.

either mental or physical, in carrying out the great object of his life. He was hardly ever incapacitated by infirmity, or loss of health, in prosecuting his enterprise; and from early manhood to green old age, even up to his latest hour, he toiled, and spent his energies and his strength. I mention these things to show, that the fatal disease which lurked within, which was progressive in its nature, and probably of long standing, could neither have produced serious uneasiness, nor proclaimed its presence by any unequivocal signs. One serious, one alarming illness, it is true, overtook him some years before his death; and though we can now look back, and somehow connect that first indication of disordered circulation with a well-grounded suspicion of commencing cardiac disease, yet his complete, his perfect recovery, and the absence ever afterwards of any manifestation of a like kind, were calculated to lull and repress all apprehension of coming danger. The attack to which I refer happened in the winter of 1834, thirteen years before he died. It was thus :—He was engaged in the early part of the day in attending a meeting of the Presbytery of Edinburgh, and had taken an interest and share in the subjects of discussion. On returning home with a friend, he was suddenly seized, on the street, with loss of power and diminished sensation over the right side, the arm and leg being chiefly affected. He felt, to use his own description, as if instantaneously a large weight of books had been placed in one of the pockets of his great-coat, and so thrown him entirely to one side. He must have fallen to the ground, but his friend supported him, and had him conveyed to a neighbouring house, and, after a little, to his own home, whence I was called to see him immediately on his arrival. I found him in bed, calm, but impressed with the conviction that he was struck down by a formidable disease. His mind was quite entire; nor did it suffer in the least during the course of his illness. His speech was somewhat affected, his articulation imperfect. The muscles of the right side of the face were partially paralysed; those of the arm and leg decidedly so. Sensation over the whole of the right side was much impaired, and particularly over the thigh and leg, which he continued to beat firmly with the left hand, in the hope, as he said, of recalling the banished sensation. The face was pale, the skin cool, the pulse soft and frequent. There was no headache or giddiness, nor any pain or uneasiness beyond what has been described. The treatment varied in nothing from that usually pursued in such cases,—moderate bleeding, purging, diaphoretics, rest, quiet—in a word, the antiphlogistic plan. Under this ordeal he daily improved; sensation and motion were gradually restored, and, after a few weeks' confinement, he returned to his avocations, and engaged as heartily and laboriously as ever in his literary and professional duties.[1]

Events in the church, of the deepest moment and most intense

[1] During the month of June following, Dr Chalmers apprehended a renewal of similar symptoms; but the threatened attack passed away, and never returned.

interest, by and by thronged upon him and occupied his mind; and negotiations, in high quarters, connected therewith, pressed upon his attention year after year. He was found equal to the time. Perhaps it might be said with truth, that these matters to which I allude told upon him. He was now approaching his grand climacteric, yet he was firm and robust. With accumulating years came a disposition to obesity; and with the silver grey on the massive forehead, came also the pallid and somewhat sickly look of fading health. Yet he seldom complained; or, if indisposed, it was only by some trivial ailment arising from indigestion. He was sometimes sick at stomach, but he was never faint, nor ever swooned away. He sometimes manifested a sense of exhaustion; but even then, it was remarked that, with uncommon alacrity, he could, when occasion required, rouse himself to great mental or bodily efforts. He had no præcordial pain, or distress in breathing; no palpitation of the heart, or intermission of the pulse. He ascended heights with wonderful facility; he slept on either side, and his rest was calm and refreshing. Had any symptoms at this time called for exploration of the chest, we might perhaps have discovered, by the assisted ear, that the impulse of the heart was feeble and indistinct; and that its first sound was imperfectly heard. We might, perhaps, have conjoined these signs with the soft, but not feeble pulse, the pallid countenance, the time of life, and the ready disposition to take on fat, and so have persuaded ourselves that degeneration of the muscular tissue had already made progress. But it could only have been a reasonable conjecture: there were no symptoms so unequivocal as to lead us to infer, that this particular lesion was to prove the cause of death. The manner and circumstances of that event I shall briefly relate.

Early in May 1847, when in the enjoyment of his usual health, Dr Chalmers repaired to London to give evidence on an interesting question before a committee of the House of Commons. He preached on three successive Sundays with all the eloquence and energy of former days; and during the week was much engaged with the business of the committee. His minute and lengthened examination, the manner in which it was conducted, and the deep, the intense interest which he felt in the whole proceedings, bear witness that even then, when within a few days of his death, he had no symptoms of serious illness. On his way homewards, he visited some near relatives in the south of England, and reached Edinburgh at the close of the month in excellent health and spirits. He attended public worship on Sunday the 30th; he met his family in the usual exercises of the day; and retired to rest at an early hour; being engaged on the following day to read to the General Assembly a report, which had occupied much of his time and thoughts. On Monday the 31st, not having responded to the usual call in the morning, his chamber was entered, and he was found cold and lifeless. I saw the body before it was disturbed. It lay in the half

recumbent posture; the head supported by pillows; the arms folded on the breast. The bed-clothes were scarcely disordered; and on them rested a basin, which had received the contents of the stomach. He had been overtaken by sickness and vomiting; but at what hour it is impossible to fix. This last circumstance of vomiting, coupled with his former paralytic attack, rendered it very probable that some cerebral lesion was the cause of death.

The examination of the body was conducted by Dr Bennett on the evening of the following day, the 1st of June—the weather at the time being unusually sultry. Dr Bennett has furnished me with the following description:—

External appearances.—The body was fat and robust; the head large; the chest broad. Putrefaction had taken place to a considerable extent. The features were swollen, bloated, and of a purple colour. The neck and upper part of the chest were of a green colour, and emphysematous.

Head.—The integuments of the scalp unusually thick; on dividing them, fluid blood oosed from their cut surfaces. The cranium was of great thickness and density. On raising the calvarium it was every where fully a quarter of an inch thick, and in some places three-eighths of an inch; it was unusually weighty. The two surfaces of the arachnoid membrane were pretty firmly united superiorly, and were lacerated on separating the brain. The angle formed superiorly where the falx turns down, was crossed on both sides, from before backwards, by firm bands of chronic lymph and fibrous tissue, some of which were nearly an inch long. A calcareous deposit, the size of a pea, was situated about the centre of the falx. The cerebral arachnoid was thickened, and perfectly opaque in the immediate neighbourhood of some of the larger meningeal vessels, presenting, on removal, a white glistening appearance; it was easily removed from the cerebral convolutions, which, superiorly, were large, and the sulci deep. The brain weighed fifty-three ounces avoirdupois, and in its structure was everywhere healthy. The choroid plexus contained small groups of simple cysts. A large quantity of fluid blood came from the divided arteries at the base of the brain.

Chest.—Both lungs were slightly adherent at their apices by one chronic adhesion of limited extent. On the left side, the pleura at the seat of adhesion was thickened, over an extent the size of a shilling, about one-eighth of an inch. The serous surfaces and lungs were otherwise perfectly healthy. The pericardium was healthy. The heart was flabby and unusually soft. The coronary artery was loaded with calcareous deposit, much contracted, and in one place obliterated, presenting considerable resistance to the knife. The heart contained no blood; its endocardial lining was deeply stained of a reddish and purple colour; the valves were healthy. The walls of the left ventricle, towards the base, and at their thickest part, were only half an inch thick; at the apex, in one place, only one-eighth

of an inch thick; the surface of the section showed that the muscular substance had undergone the fatty degeneration. In some parts of the ventricle, half the substance was completely fatty; in other parts, nearly its whole thickness was fatty, with a thin line one-sixteenth of an inch thick of muscular substance only remaining. Even this muscular substance was of a pale and yellowish colour. The right ventricle was unusually thin, and, superiorly, was so soft, that it broke down under the finger, and could be torn like a soft pulpy substance. Everywhere the substance of the organ was soft and friable.

Abdomen.—Liver small, of a dirty olive-green colour throughout. Gall-bladder distended with yellow fluid bile. Spleen small; on section, presenting a black colour. The omentum and sub-peritoneal cellular tissue were greatly loaded with fat. The kidneys were imbedded in a surrounding mass of fat an inch thick, they were soft and flabby, of a dark mahogany colour, but in texture healthy. The stomach, internally towards the larger curvature, of a greenish hue, with slight extravasation of blood below epithelium. Intestines were healthy.

Microscopic Examination.—The substance of the heart throughout consisted of fatty granules. The muscular fasciculi could scarcely be seen, although here and there traces of the longitudinal fibrillæ could be observed. No transverse striæ were anywhere visible.

I should mention that Dr Bennett suggests, as the probable cause of the want of blood in the heart, that it had drained away, in its fluid and putrid state, from the divided cerebral vessels, during the examination of the contents of the cranium.

Before venturing a remark on this interesting case, I would recall to the recollection of the Society the history of another eminent individual, who was known to many of us, and whose life and death, in many respects, bore a remarkable resemblance to those now related.

Dr Abercrombie died in his sixty-fifth year. I knew him long and well. I had the happiness of living, during many years, under his roof; and was witness, through his life-time, to his zealous professional labours, his unwearied industry, and his active benevolence. He enjoyed, during a long series of years, uninterrupted health; so that I can scarcely call to remembrance an occasion on which, for more than a few days together, he could be said to have been unfitted for professional duty; till the illness, which we well remember, filled his mind with the most serious apprehensions, in regard to his future usefulness. It was in the winter of 1841, three years before his death, that, in driving to a professional engagement, he was suddenly seized with loss of power and impaired sensation over the left side, but without the loss of consciousness or any affection of speech. He immediately returned home, and gave orders for the abstraction of blood by cupping. Dr Hunter, who was called to him at the time, found him under great anxiety, complaining of præcordial uneasiness, and slight headache, with frequent sighing, cold skin, and pallid countenance. The pulse was frequent and small at first, but,

after a while, it subsided in frequency and rose in strength. Blood was taken both from the temple and arm to a large extent, and active purging was pursued by the patient's desire. I need not, however, pursue the details of this attack, which have been given elsewhere,[1] farther than to state, that it was Dr Abercrombie's decided impression that his illness was paralysis, connected with cerebral disease, and that impaired mind, and enfeebled bodily power were to be the lasting consequences. It certainly never occurred to any of his medical advisers to connect the symptoms with deranged circulation, arising from a damaged heart. He did complain, I remember well, of præcordial uneasiness—it never amounted to pain—and of something more than uneasiness in the left arm and shoulder, and at the base of the scapula; but the circulation was regular, though feeble, and the feebleness might readily have been accounted for by the active measures which he himself employed, and the scanty fare to which he subjected himself,—treatment which no long time afterwards was found to have been entirely misplaced.

Dr Abercrombie recovered from this attack, and in the course of a few weeks was able to resume his professional duties. He continued pallid, however, and carried about with him somewhat the air of an invalid, but his mind was as active and intelligent as ever. Previous to this attack, he had been observed to have become rather corpulent and fat. He continued to improve, and his health was considered completely re-established. He never had any threatening of return of similar symptoms during the three succeeding years, nor any indication of latent disease, beyond what was remarked by Dr Alison, when, in accompanying him up stairs a few days before his death, he observed that he was breathless. On Thursday, the 14th November 1844, Dr Abercrombie was in his usual health; he engaged with his family in the devotional duties of the morning; he breakfasted with appetite, and, after visiting a domestic in the family, he retired to the water-closet. He was there sufficient time to justify the suspicion that some illness had overtaken him; and, on opening the door, he was found extended on the floor in the arms of death. A few convulsive sobs were heard,—and life departed.

The body was examined by Mr Goodsir, whose account of the morbid appearances I take the liberty again of laying before the Society.

"*Head.*—The skull-cap was thick. The crista galli, the posterior clinoid processes, and the bones of the head generally, were powerfully developed. The brain was softer than might have been expected at the time of the examination (fifty hours after death). There were no traces of old or recent effusions of blood. The internal carotid, the circle of Willis, and all the arteries of the organ were studded with much atheromatous deposit; and the internal carotids, at their last curve, were slightly dilated. The organ was of great

[1] Edinburgh Medical and Surgical Journal, vol. lxiii., p. 227.

size, and weighed sixty-three ounces avoirdupois. The ventricles were capacious, without an increased amount of serum, which appeared to correspond to the great size of the cavities.

"*Heart.*—The pericardium contained a large clot of blood, enclosing the heart as a mould. On the posterior surface of the left ventricle, two-thirds from its base, and about an inch and a-half from the septum, there was a fissure or rent, a quarter of an inch long, in the direction of the fibres of the ventricle, with ragged and ecchymotic edges. Towards this fissure a considerable branch of the left coronary artery passed, but did not enter into it. In the fissure the ruptured orifice of a vein was visible, and a bristle introduced into it passed on towards the base of the ventricle, and appeared on the cut surface of the posterior wall of the ventricle, made on opening the heart. In the neighbourhood of the rupture two irregular ecchymotic spots were situated, through which passed branches of the coronary artery. These spots consisted of effused blood, but their connection with ruptured vessels could not be distinctly made out. The serous membrane over them was quite entire. Both coronary arteries were much dilated as they passed off from the aorta. Throughout their course they contained in their walls much atheromatous matter, but were not ossified. The aorta and its semilunar valves also contained a considerable amount of the same deposit, but the former was not enlarged. The heart was slightly enlarged and dilated, rather loaded with fat, and remarkably soft, as if from interrupted nutrition. All the cavities of the heart were found empty. Extensive adhesions of the right pulmonary and costal pleuræ. The other viscera were healthy."

When the case of Dr Abercrombie was originally brought under the notice of this Society, immediately after his death, comparatively little attention had been given to the peculiar lesion which terminated his life so suddenly and so unexpectedly; indeed, fatty degeneration, as now understood, had scarcely been recognised, though, on the occasion referred to, Mr Goodsir distinctly announced his opinion, that the remarkably soft and flabby state of the heart, as if from interrupted nutrition, was owing to that morbid change. We are now better informed in regard to the history of the atheromatous deposits in the blood vessels,—which are only changes contemporaneous with the more serous lesion,—and with the rupture of the muscular fibres, which is now recognised as one of the most common terminations of fatty heart. Mr Goodsir has kindly communicated to me the result of a microscopic examination made subsequently to the drawing up of his report of the inspection of the body, from which it appears, that the muscular fibres of the ruptured portion of the ventricle (alone examined) had undergone the fatty degeneration, being here and there interspersed with fatty granules, generally in transverse rows,—and that there were masses of fat cells or globules at intervals,—while some of the fibres were nearly empty.

Dr Abercrombie and Dr Chalmers were both of the age when the

tendency to fatty degeneration has been shown to be most prevalent. Among 83 cases collected by Dr Quain, death occurred in 14 between the ages of 50 and 60; in 18, between 60 and 70; and in 14, it occurred between 70 and 80. These eminent men had also indicated a proclivity to the disease by a constitutional tendency to take on fat, and in doing so, to assume the pallid and sickly look of diseased nutrition,—facts which have been remarked by many observers. They terminated their lives in the manner most common to those who suffer from it, the one by rupture of the heart, the other in syncope. Of Dr Quain's 83 cases, 28 died by the former, and 26 in the latter mode.

It has been remarked by Mr Paget, that the principal character which all the cases of fatty degeneration of the heart seem to present, is, that they who labour under this disease are fit enough for all the ordinary events of calm and quiet life, but are wholly unable to resist the storm of a sickness, an accident, or an operation.[1] There may be much general truth in the statement; and in the two cases just recorded, it is worthy of remark, that up to the hour of death the distinguished individuals were actively engaged in the labours of the two most arduous and onerous professions, and were overtaken, as in other recorded instances of this particular lesion,—the one, it is believed, by the act of vomiting; the other, by an effort at stool.

I have purposely selected these two cases, because their previous history was well known; the every-day life of the individuals for weeks, and months, and years, was noted, before the final event arrived: the clinical history of the disease is short, but it is complete. "It is not the manner of death that it is profitable to understand in this disease," says Dr Latham, speaking of a closely allied affection of the heart, "but the manner of life; how life and the great organ of life are affected by it, at its beginning, and during its progress, and what indications they show, enabling us to minister to it remedially." "The two cases which I have related," the context is still applicable to my present object, "surely do not contain what we are in search of, in respect of the particular disease; but they contain, nevertheless, matter for reflection. They convey, not a clear understanding, but some conceivable notion, how life itself, and the very attributes and conditions of life, in different organs, may interfere to prevent that perfect knowledge of diseases which we seek from their symptoms." * * * "They show a disease so tardy and so gradual, as to convey no perception of what it is to the blood-

[1] I have lately seen, with Dr Richard Mackenzie, a respectable citizen, approaching the age of 75, who had recently submitted to be cut for stone, and had a large calculus, weighing nearly two ounces, extracted by Mr Syme; and who, at the period I saw him, was labouring under an attack of pneumonia, from which he perfectly recovered, while, during many previous years, he had manifested the signs of fatty heart, so far as these can be diagnosticated during life, including also the well-marked arcus senilis.

vessels or to the nerves, and to awaken no sympathy in them, and call forth no signs from them of its existence. Its whole clinical history is comprised in its fatal event."[1]

Dr Quain, however, in referring to the fact that fatty degeneration of the heart is said to be frequently found after death, where its existence had not been suspected during life, has expressed the opinion, that such cases would not often be met with, if a full inquiry could be made into their symptoms; and then goes on to remark:—"I feel the truth of this observation, more particularly in those cases which occur in the progress of advancing life, when, whilst the system generally keeps up its powers tolerably well, the heart suffers from some local cause, such as diseased coronary vessels. In those cases the balance between the system and heart is lost, and phenomena as clear and as pointed as any that can indicate the nature and seat of disease are developed."[2]

What reply do the two cases which have now been related to the Society make to this unqualified statement? The subjects of these cases had made progress in advancing life. They expired in a sudden and unexpected manner; and examination after death disclosed the hidden cause in the peculiar lesion of the heart, accompanied by the morbid condition of its vessels, to which Dr Quain refers; yet, during life, neither of them exhibited any of those clear and pointed phenomena to which he alludes. The testimony of Dr Abercrombie is peculiarly valuable in relation to this point; himself a careful and long-experienced observer of the phenomena of disease, and anxiously alive to all manifestations of it in his own person, he never acknowledged, as I firmly believe he never felt, any of those symptoms of degeneration of the heart indicated by Dr Quain,—symptoms which, I apprehend, will be found to be more intimately connected with those other morbid conditions and lesions of the organ which usually co-exist with fatty degeneration. Let us shortly inquire what those signs, general and physical, are, which are said to denote this degeneration of the heart, with the view of discovering any modification of their features in the history of the cases now under consideration.

First in order, have been mentioned symptoms of cerebral congestion, such as *giddiness, drowsiness, coma.* No symptoms of this kind were at any time witnessed; and though manifestations of interrupted circulation in the brain might have been expected to flow from the want of muscular power in the heart to propel the blood brought by the veins from the head, yet never, if we except the attack of paralysis which both Dr Abercrombie and Dr Chalmers experienced long before death, was there any evidence of cerebral congestion. It may, however, be a subject of interesting inquiry, how far these paralytic seizures, which we have noticed,

[1] Lectures on Diseases of the Heart, vol. ii., p. 160.
[2] Medico-Chirurgical Transactions, vol. xxxiii., p. 160.

were connected with early changes in the muscular fibres of the heart; though, on the presumption that they stood in the relation of cause and effect, it is difficult to account for the exemption from future atacks which both patients experienced, the one during the three, the other during the thirteen, last years of life.

The occasional occurrence of apoplexy and palsy, in connection with fatty degeneration of the heart, may, more readily, be traced to other morbid conditions of the organ, which co-exist with that particular degeneration, than to the degeneration itself, such as hypertrophy of the left, and dilatation of the right, ventricle; more especially, when these again are associated with disease of the blood-vessels of the brain.

Faintness and *syncope* are next recorded as symptomatic of fatty degeneration of the heart; but they were not observed in either of the cases now related; though there is reason to believe, in the absence of other causes to account for death, that a fatal syncope at last overtook the distinguished individual, the subject of the first sketch. But syncope is not an uncommon symptom of other affections of the heart; and may naturally be expected to arise where the left chambers of the organ are deficient in muscular power in transmitting the blood to the brain, whatever may be the immediate cause of this loss of power, whether the growth of fat upon the organ, the degeneration of its muscular tissue, or the dilatation and thinning of its walls from other morbid changes. It has been observed that the peculiar degeneration, we are now considering, is at times confined to one ventricle, and at other times to its fellow, and that often both are equally affected. May not the prevalence of head symptoms at one time, and of syncope and faintness at another, be accounted for by the part of the organ implicated ?. When the right ventricle is softened and wasted, comatose symptoms may be looked for, in consequence of the interruption to the free discharge of blood *from* the brain: when the left ventricle suffers, then syncope may be expected, from the want of full and free circulation *within* the head: while, when the chambers of both sides equally participate in the degeneration, the circulation may be feeble and slow, but it will not be liable to those disturbances which are likely to arise from the balance of power being lost between the two sides of the heart.

Shortness of breathing is another symptom frequently observed in those labouring under fatty heart; but it was not present in either of our cases. I have already remarked, that Dr Chalmers continued to the last to ascend heights with wonderful facility; and Dr Hunter informs me, on the best authority, that Dr Abercrombie, during the autumn which preceded his death, was in the habit of ascending the hills in the neighbourhood of his summer residence, without experiencing any difficulty of breathing; and all, who were accustomed to meet him professionally, must remember the quick and alert manner in which he mounted to the bed-room floor, in cases of sickness. It has, however, been noticed, that an experienced ob-

server had, on one occasion, and that a few days before his death, remarked that, in ascending the steps of his own house, Dr Abercrombie was breathless. It was the first and the only remark of the kind; and perhaps it is right to mention, that it was made at a time when Dr Abercrombie was recovering from a bronchial attack which had confined him for a day or two to the house. But we must expect dyspnœa in cases of softened and degenerated hearts; it is not, however, a constant symptom, and, even when present, may be found to be connected with other co-existing hindrances to the free circulation through the lungs.

Præcordial pain is another of the characteristic symptoms of this disease of the heart; and certainly has been acknowledged by many who have fallen victims to it. But it is a remarkable fact, that in the case both of Dr Abercrombie and of Dr Chalmers it was entirely wanting. The former was sensitively alive to every morbid feeling; the latter was not indifferent to suffering, but, with the exception of the præcordial uneasiness which for several days affected Dr Abercrombie during the illness which we have referred to as occurring three years before his death, he was never known to complain of pain, nor had he at any time the least suspicion that he was the subject of cardiac disease; while, in the instance of Dr Chalmers, I cannot call to recollection the smallest complaint of pain, anxiety, or distress in the region of the heart. Both had the coronary vessels extensively diseased; but neither suffered the symptoms of angina pectoris, unless it were in the moments which preceded dissolution.

Functional derangement of the liver and chylopoietic organs has been noticed as a constant attendant on this form of heart disease. Hasse informs us that, so far from being a merely local morbid condition, as some pathologists believe, it is the result of various affections in other organs; and, though not manifested by any specific local symptoms, its presence may, nevertheless, be inferred with tolerable certainty from collective symptoms referable to other parts of the body. Thus he mentions that, in functional disturbance of the larger organs of secretion, and especially of those engaged in the elaboration of venous blood, we meet with fatty encumbrance of the heart in its second stage; the anormal condition being then conjoined with other changes, all dependent more or less directly upon hepatic or pulmonary disease, or, at any rate, indicative of venous plethora.[1] Various morbid conditions of the liver are noticed among the collected cases of Dr Quain; and in the instance of Dr Chalmers this part was also found diseased. It was the organ, especially in his later years, which was most readily disturbed, being quickly influenced by errors in diet, or by stimulating beverages; hence he was led to abandon the use of wine and fermented liquors, and to confine himself entirely to water. I may take the liberty of recording an inte-

[1] Pathological Anatomy, p. 169.

resting fact in his history, which he communicated to me, and which bears upon our present subject,—namely, that the continued use, for a few weeks together, of a mixture of spirits, water, and sugar, was invariably attended with a manifest increase of fat, which subsided on the disuse of the stimulating drink;—a fact which, taken in connection with the circumstance of fatty heart being frequently found in those addicted to the free use of spirits, may lead to the suggestion of prophylactic measures in the case of those who appear to labour under that disease.

The state of the pulse cannot be depended upon as a diagnostic mark of fatty heart. In some cases it has been found irregular, in some weak, in others slow; but it is subject to much variety, according to the complications of cardiac disease which concur with this degeneration of the muscular fibre. In both Dr Abercrombie and Dr Chalmers, the natural pulse was soft and weak; it was never irregular, nor at any time of such a character as to raise any apprehension of organic disease. Hence, in neither case was auscultation employed as a means of discovering the physical signs which are said to belong to fatty degeneration. Admitting, however, the discovery of a feeble impulse, a feeble first sound, and extended præcordial dulness,—signs which the state of the heart after death might have led us to infer existed during life,—still these, when combined with any general signs recognised during a long course of years, could not have led to a distinct and satisfactory diagnosis; while I am willing to acknowledge that, taken in connection with the age of the individuals, their pallid complexion, and tendency to obesity, they give room for the conjecture, that the great organ of life in both, was yielding, through the progress of time, to those organic changes, which mark the decay of its structure, and foretell the not distant cessation of its long-continued functions.

ARTICLE II.—*Case of Stricture of the Urethra, in which the Operation of Division of the Stricture by External Incision was followed by a Fatal Result.* By R. J. MACKENZIE, F.R.C.S.E., Junior Ordinary Surgeon to the Royal Infirmary, Edinburgh.

THE following account of this case was read at a meeting of the Medico-Chirurgical Society, on the 15th of January. The details of the case are given partly from short notes of my own, but chiefly from a very accurate report of the case made by my house-surgeon, Dr Harley, in the hospital journal:—

Andrew Cree, a labourer, æt. 41, was admitted into the Royal Infirmary, under my care, on the 4th December last, labouring under stricture of the urethra.

He stated, that he had been treated for stricture twelve years before, and that, under the use of bougies, which were occasionally

passed for about a twelvemonth at that time, his condition was so much improved that he suffered little annoyance from the stricture.

He remained tolerably well for two or three years, when the difficulty of voiding his urine began to increase, and for the last two years his symptoms had been much more urgent than at any previous period.

At the time of his admission into the hospital, the calls to void his urine were nearly constant, day and night, and his water was passed (frequently involuntarily) either in drops or in a very small spiral stream.

On the day of his admission, I examined his urethra, and found a stricture, about three inches from the orifice of the urethra, through which I was unable to pass the smallest-sized instrument.

On the 6th (two days afterwards) I passed No. 1 bougie into the bladder, and found that a second stricture existed at the bulb. Around each stricture a considerable degree of induration existed. The induration anterior to the scrotum appeared to extend along the urethra for about an inch, but was most strongly marked about three inches from the orifice of the urethra, where a narrow band seemed to grasp the instrument tightly.

On the 9th I passed No. 2, and on the 15th, No. 3,—each time with very considerable difficulty, the instrument, after being passed into the bladder, being so tightly grasped as to require a considerable amount of force to withdraw it.

The condition of the patient was now greatly improved. He was able to retain his urine perfectly, and did not pass it oftener than twice or thrice in the twenty-four hours, and in a tolerably good stream.

After each instrument was passed, however, he suffered from long-continued rigors, which were generally followed by slight temporary febrile disturbance. On one occasion I was obliged to suspend the use of the bougie for four or five days, on account of one of these febrile attacks.

Between the 15th and 31st December, I repeatedly attempted to pass No. 4 bougie, but invariably failed in passing it through the anterior stricture, although as great a degree of force was employed in the attempt as appeared to me justifiable. No. 3 was several times passed into the bladder, but was grasped on each occasion as tightly as at first. The induration around each stricture remained undiminished.

Under these circumstances, the case appeared to me to be one peculiarly suited for the operation proposed by Mr Syme,—the division of the urethra, upon a grooved director, by external incision.

The patient having acceded most willingly to the proposal, I had a grooved staff made of the size of No. 3 bougie, and ascertained by one or two trials that it could be passed through both strictures into the bladder.

On 31st December, the patient being, as far as I could judge, in an excellent condition for the performance of the operation, retaining his urine for ten or twelve hours at a time, his tongue clean, and his pulse natural, I performed the operation in the presence of Mr Syme and my other colleagues in the hospital.

The grooved staff being introduced into the bladder, the patient inhaled chloroform, and was placed in the usual position as if for the operation of lithotomy. I made an incision, about an inch in length, over the anterior stricture, and after feeling the groove of the staff through the urethra behind the indurated constriction, I inserted the point of the knife at this point, and ran it forwards along the groove, dividing the constricted portion of the urethra till the resistance offered to the knife was entirely overcome. The extent of this stricture was greater than I had expected, the indurated part of the canal, which I divided, being fully an inch in length. To make sure that I had divided the whole stricture, I slid a director gently both backwards and forwards along the groove of the staff at the extremities of the incision. The bleeding from this incision was very trifling (I should think not above half an ounce). I then made a second incision over the situation of the bulb, of about an inch and a quarter in length, and proceeded in the same manner, entering the knife behind the induration around the stricture, and passing it forwards till the resistance offered by the indurated tissues to the edge of the knife was overcome. The extent to which the urethra was divided in this incision was about half an inch. I then withdrew the staff, and introduced No. 8 catheter into the bladder, and satisfied myself that it slid freely backwards and forwards along the urethra. The catheter was retained in the bladder by the usual fastenings, and the patient was removed to his bed. The bleeding which took place from the second incision was more copious than from the first, but the blood lost during the operation, from beginning to end, did not altogether exceed four ounces. The bleeding continued for a little after the patient was put into bed, but its amount was very trifling, and it soon ceased spontaneously. A plug was placed in the mouth of the catheter, and withdrawn occasionally by the patient to allow the urine to escape.

On the following day, at the hour of visit, he was remarkably well; he had slept well during the night, and had no complaint of any kind.

On the 2d of January (the second day after the operation) I withdrew the catheter. The patient was as well as on the previous day.

On the 3d, I found that he had had a slight rigor in the morning, which had been followed by a little febrile disturbance. At the hour of visit, however, he seemed to be going on quite well. His pulse 78, and soft, and the urine passing freely, both by the posterior wound and by the natural outlet. Both wounds had a perfectly healthy appearance.

About ten o'clock in the evening, I received a message from Dr

Harley, saying that some alarming symptoms had suddenly made their appearance. On going to the hospital, I learnt that during the afternoon he had had a severe attack of shivering, after which he complained of giddiness, and his countenance assumed a look of anxiety. It was not, however, till the evening, about nine o'clock, that any alarming symptoms made their appearance. He then suddenly became pale, his features contracted, his pulse very rapid, and scarcely perceptible at the wrist. When I saw him at ten o'clock, he had all the appearance of a patient suffering from the shock of a severe injury; he was quite sensible; the surface of the body was cold and clammy; his features shrunk; the pulse was scarcely perceptible at the wrist; he complained of a choking sensation, and had all the aspect of a dying man. He had passed water freely in the afternoon; there was no distension of the bladder; both the wounds had a perfectly healthy appearance; there was no swelling in their neighbourhood; no swelling or induration of the perineum, or in the prostate, when examined by the rectum. The sheets were stained by a few drops of blood from the wound in the perineum. Warm bottles had been placed around him, and brandy had been freely administered. I desired that this treatment should be continued, and that he should take in addition some camphor mixture with carbonate of ammonia.

In a few hours he had rallied somewhat; his pulse became more perceptible, and he felt better.

At visit the following day he was better; his pulse very rapid, but quite perceptible at the wrist. He had passed water freely, but in small quantity, both by the wounds and the natural outlet. Mr Syme saw him with me, and, with myself, was satisfied that nothing could be detected in the perineum to account for the constitutional symptoms. The wounds had the same healthy appearance as on the previous day. The same treatment was continued, and he improved a little during the afternoon, till about four o'clock, when he again suddenly sank into the same state of collapse as on the previous evening; and, in addition to his previous symptoms, the stomach now rejected all fluids as soon as they were swallowed. Sinapisms were applied to the epigastrium and to the calves of the legs. On visiting him about eight in the evening, I found that the vomiting had continued constantly since the afternoon, and had not been in the least degree checked by creosote and some other remedies which had been given. He was now evidently dying. The whole surface of the body was perfectly cold; the pulse was not perceptible at the wrist, and scarcely so at the axilla; his breathing was laboured and very rapid, and his breath cold; he had constant hiccough, and vomiting of a dark greenish-coloured fluid; his countenance was sunk, and he had very much the appearance of a patient in the last stage of cholera.

Being at a loss to account in any way for the presence of these symptoms, it occurred to me that, if it were possible to maintain his

strength for a time, he might still rally from the state of prostration in which he now was, and the only means by which it appeared to me that this object could be attained was transfusion. In this opinion Dr Graham Weir, who was with me, concurred; and, with his assistance, from ten to twelve ounces of blood were safely transferred from one of the porters of the hospital into the veins of my patient. The effects of the transfusion on the patient were immediate, though not strongly marked. He said immediately that he felt "warmer and stronger," and in a short time his pulse was felt at the wrist, regular, and about 126 in the minute. The vomiting became less urgent, but again returned towards morning, and continued till the following afternoon.

On the following day (the 5th), at the hour of visit, his extremities were still cold, and his pulse of the same frequency, and weak. He had passed a small quantity of urine by the wound voluntarily, and without difficulty.

On the 6th, his appearance was decidedly improved. His pulse was 100, regular, and much stronger. He had still, however, a good deal of hiccough, and his countenance had an increased look of anxiety.

In the evening his pulse had fallen to 88; his tongue was clean and moist; he had taken some nourishment with relish, and had passed a larger quantity of urine freely by the natural outlet. The healthy appearance of the wounds continued, and the perineum and prostate were free from swelling or induration.

On the following day (the 7th), I found him much worse. His pulse was rapid and irregular; his tongue brown and dry; the surface of the skin over the whole body was of a bright scarlet hue; there was suppression of urine, with muttering delirium, and *subsultus tendinum*. He gradually sank, and died at 8 P.M. on the 7th (the eighth day from the performance of the operation, and three days after the transfusion).

A *post-mortem* examination of the body was made by Dr Gairdner on the following day.

The abdominal organs, including the kidneys and ureters, were healthy without exception.

The skin being reflected from the genital organs, the penis was divided about two inches from its orifice, the pubes sawn across on each side, and the whole urinary organs removed. The surface of each wound in the urethra had a gray sloughing appearance, which, however, was confined to the immediate neighbourhood of each wound,—there being no suppuration or sloughing of the texture of the *corpus spongiosum* or bulb, or in the cellular tissue immediately surrounding these parts. The deep perineal fascia, and the textures around the *levator ani*, were free from any morbid appearance. The anterior incision of the urethra was from an inch and a quarter to an inch and a half in length—the posterior about half an inch. These

incisions were found, on laying open the urethra, to correspond in length to the extent of the constricted portions of the urethra,—the anterior stricture being an inch and a quarter in length, and much narrower at one point at its anterior extremity than in the remaining part of the stricture—the posterior stricture occupying the situation of the bulb, and about half an inch in length. A considerable degree of induration still existed in the sub-mucous tissues around each divided stricture.

The portion of the urethra between the posterior stricture and the bladder was very much dilated; and the muscular fibres around the membranous portion of the urethra were strongly developed, giving this portion of the urethra somewhat of the fasciculated appearance presented by the inner surface of the bladder in this as well as other cases of old-standing stricture of the urethra. The prostate and tissues surrounding it were perfectly healthy. The plexus of veins between the *levator ani* and prostate were carefully examined, as well as the veins in the neighbourhood of the incisions in the urethra, but were not found to present any signs of inflammation. The bladder was contracted and empty. Its mucous coat was here and there slightly congested, and especially at one point at its *fundus*, apparently from its having been in contact at this part with the point of the catheter. The muscular coat of the bladder was strongly developed, being uniformly about half an inch in thickness. In the substance of the muscular wall, above the peritoneal reflexion, on the posterior surface of the bladder, and a little to the right side of the mesial line, an abscess existed, containing fully a drachm of healthy pus. The posterior wall of this abscess was formed by the peritoneal covering of the bladder. On examining the pelvic viscera before they had been removed from the body, some slender and soft bands of lymph were observed uniting the most prominent part of the wall of this purulent collection to the peritoneal covering of the rectum. Lymph was also exuded on a small spot of the mucous membrane of the bladder, as well as in the sub-mucous texture forming the anterior wall of the abscess. A similar, but still smaller, exudation of lymph existed on the mucous membrane, a little below the opening of the ureter.

On opening the chest, about a quart of purulent fluid was found in the cavity of the right pleura, both surfaces of which were coated, to the thickness of about quarter of an inch, with soft yellow lymph. The right lung was mostly flaccid and partially compressed, but presented several condensed nodules and points of congestion, which yielded a sero-purulent fluid on pressure. The largest of these condensed nodules was about an inch and a half in diameter, very deep purple externally, but presenting at its centre the grayish colour usually seen in the first stage of the secondary deposits, which take place as a consequence of purulent infection. On sections being made of the left lung, it showed a few small points congested and partially solidified, yielding on section a frothy purplish gray fluid.

I have been further informed by Dr Gairdner, that the blood in the ascending *vena cava*, when examined under the microscope, was found to contain structures similar in appearance to true pus corpuscles, and that a loose decolorised clot, found in the left auricle, and having an unusually granular opaque appearance, was found to contain similar corpuscles in large quantity. In both these situations, these bodies very much exceeded in numbers the ordinary proportions of white corpuscles in the blood.

On examining the wound at the bend of the arm, the edges appeared everted and slightly callous,—having, likewise, a grayish discoloured appearance. All the tissues around the immediate edges were perfectly healthy. The whole of the superficial veins throughout the arm were carefully examined, but neither their internal membrane, nor the blood within them, presented the slightest appearance of disease.

I need not apologise for having entered somewhat minutely into the details of this case, which I have considered it my duty to lay before the Society.

Its bearing upon the value of the operation proposed and advocated by Mr Syme, must be judged of by those who have watched its progress, and who have now heard its details. I can only say for myself, that it will not deter me from repeating the operation in similar cases.

It must be said of this, I think, as well as of all other operations, however trifling, that it is not free from dangerous consequences. It is well known that small operations, such as the removal of a finger or toe, the excision of small tumours, and incisions of all kinds, have repeatedly proved fatal, and this most commonly from the complication, which proved fatal in this case, the absorption of pus into the circulation.

The disease for which the operation is performed (confirmed stricture of the urethra), is one of a most distressing kind—one, which itself frequently proves fatal, and which, under the ordinary means of cure, is a tedious and frequently unsatisfactory subject of surgical treatment.

The operation, in the case I have related, has, it is impossible to deny, been directly productive of fatal consequences. It was performed at a time when the urinary irritation, produced by the disease, had been overcome by the partial dilatation of the urethra, and when the patient, as far as I could judge, was in a most favourable state for the performance of an operation. The operation was performed, and the after-treatment conducted, on the principles laid down by Mr Syme, and which have been followed, in his hands, with so much success.

With regard to the transfusion, I must state that, had I known that the symptoms which existed were the consequence of *pyæmia*, I should not have thought of adopting such a measure. I resort

to transfusion when at a loss to assign any distinct cause for the state of extreme prostration which was present; and I think no one, who had seen the condition of the patient on the night of the 4th, could doubt that the effects of the transfusion were to prolong his life during the three following days.

ARTICLE III.—*Case of Tumour in the Middle and Anterior Lobes of the Brain.* By WILLIAM TRAILL, Esq., Surgeon, Arbroath.

MR A. B., a farmer, aged 31, tall and robust, with an unusually small head, of active and temperate habits, but nervous and excitable, subject in boyhood to obstinate chronic ophthalmia, and since manhood to frequent attacks of dyspepsia, one of these terminating in jaundice,—in March 1846 had an attack of inflammation of the hepatic peritoneum, probably involving the duodenum, which yielded to general bleeding and other antiphlogistic measures, but left ever after a considerable degree of torpor of the bowels, requiring the habitual use of aperients.

On the 10th May of the same year, without any premonitory signs, he fell down insensible; on which I was sent for. When I arrived he had recovered, but was still torpid and confused, and had that peculiar expression of countenance which is commonly found after an attack of epilepsy; but I could not ascertain that there had been convulsions or foaming at the mouth, or the peculiar epileptic cry. A similar attack, I was informed next day, had occurred some hours after; but of this one I could not obtain any more particular account than of the first, his friends having been too much agitated to be capable of observing correctly.

In a few days he was able to be out and attend to his ordinary employment; and in the course of a few months he recovered his usual health and energy, except that ever after he was subject to frequent slight attacks of confusion or stupor, lasting only for a few minutes, and attended with the appearance of sparks of fire before the eyes. With this exception, and a torpid state of the bowels, he continued in good health, and took a great deal of active exercise, both on foot, and, after the spring of 1848, on horseback, and had no real fits of epilepsy till 26th May of that year, having been thus more than two years exempt from them.

On the day previous to that date he had been transacting some business at a distance from home, and, after staying over the night, left the place on horseback, with the intention of being in Arbroath by a certain hour. But on the journey his horse unfortunately lost a shoe, which accident mistimed and excited him a good deal, and induced him to ride very fast. When within four or five miles of Arbroath, he had taken a fit on horseback, and was found lying on the road, but not hurt. He afterwards mounted his horse, and rode into town,

transacted some business, and then called on me. This fit was evidently owing to the violent exercise and mental excitement. From this period till March 1850 he continued to have fits at intervals of from one to six months. On one occasion he had three in one day. All the seizures since 26th May 1848 were undoubtedly epileptic. Latterly, a rash over the forehead appeared after each, and extravasation on both sclerotics after several. During the intervals of these attacks, although in good general health, and able to take a great deal of exercise without fatigue, he continued subject to daily attacks of slight stupor, and was generally nervous, excited, anxious, and unable to bear much mental exertion, although the mind was commonly clear, and the bodily functions natural. The pulse was frequent, but otherwise natural.

There did not appear to be any adequate cause to which the first attack could be attributed. Betwixt thirteen and fourteen years before, he had sustained a severe contusion on the head, from a beam of wood falling upon it; but this did not seem to produce any serious inconvenience at the time, although his friends say he had been subject ever since to occasional severe headachs. There did not appear to be any epilepsy in the family; but his father died, after suffering for many years from some nervous complaint, which greatly affected his spirits, rendering him incapable of mixing with society. His mother died of an affection of the spinal cord, slow in its progress, and producing general paralysis. A sister, who had been long in delicate health from a chest affection, died of erysipelas. These three deaths, as well as that of his grandmother (a woman beyond ninety), all occurred within two years, and greatly unhinged him; for he had lived with them in family from his infancy, and was warmly attached to them. The treatment employed hitherto had been local bleeding, blistering, cold applications, aperients, spare diet, and abstinence from all stimulants, exercise in the open air, which he always enjoyed and bore well, and the careful avoidance of inordinate mental excitement and exertion. A caustic issue was kept open in the nape, and full courses of the metallic tonics—zinc, copper, and silver—employed, with regular cold sponging every morning. These means, till the unfortunate occurrence in May 1848, promised to be attended with permanent success.

In February 1849 he went to Edinburgh, for the purpose of consulting Professor Christison, who advised perseverance for another month in the use of ammoniuret of copper,—the tonic he was then using,—a month's excursion, and separation from business, and, if no amendment, a cautious trial of arsenic, continuing its use till its physiological action should begin to appear, at the same time giving the volatile tincture of valerian;—a trial of extract of nux vomica, or of strychnia, was also proposed, if the disease should not yield to the other means. These suggestions were acted upon till February 1850, but without any satisfactory result. Dr C. was, therefore, again consulted. His opinion had been, from the first,

that there was organic disease within the head. As the patient continued to be well nourished and muscular, he proposed the trial of a system of very low diet.

It was some time before I could persuade his relations to allow him to be subjected to this severe plan of treatment; but in the course of a month, convinced of the hopelessness of the case, they were brought to give their full consent to a trial of it.

Having first withdrawn blood pretty freely from a vein, his diet was gradually reduced till he was allowed only three ounces of animal food and six ounces of bread daily, or their equivalents. Tartar emetic and digitalis were also used as auxiliaries. He never had an epileptic fit after this, and for three or four months enjoyed better health, both of body and mind, than he had done for a considerable time previously; but afterwards he became gradually feeble and languid, so that it was necessary to make some improvement in his diet; and he fell into a most distressing state of hypochondriasis and nervous irritability, which continued till within the last few days of his life, when he gradually fell into a state of complete coma, and died on the 20th December.

The head was examined by me two days after death, along with my brother, Mr John Traill, who also saw him frequently during his illness; and the following were the appearances noted:—Membranes natural, but strongly adherent to the bones; veins rather enlarged, and containing dark fluid blood; slight serous effusion between the arachnoid and pia mater; substance of the brain healthy, but rather pale; nearly in the centre, and towards the lower part of left hemisphere, a tumour, fully the size of a walnut, of an oval form, of a dark pinkish colour, and of somewhat firmer consistence than the brain, in its centre partially softened and broken down, and mixed with purulent matter; in the lower and anterior part of the same hemisphere, another similar tumour, about the size of a large hazelnut, of like structure, but without softening—both completely circumscribed, and the portions of brain with which they were in contact appearing entirely of their natural structure. The ventricles contained about an ounce of limpid serum. The pituitary gland enlarged, and of a dark colour, and that portion of the dura mater which covers it, and lines the sella turcica, preternaturally vascular, and of a livid colour. The bone which forms the sella turcica also dark coloured, and on the left side its process partially absorbed.

The other contents of the cranium appeared to be in a normal and healthy state.

4th February 1851.

ARTICLE IV.—*Case of Tumour in the Anterior Lobe of the Brain.* By A. KIRKWOOD, M.D., &c., Surgeon to the Berwick Dispensary, and to the Infirmary of the Berwick Union.

THE subject of this case, Thomas Helay, æt. 44, sailor, was admitted into the Infirmary of the Berwick Union Workhouse the 7th November 1850. The following is his history, as gathered from himself some time previous to his death, and since from his relations.

Towards the end of 1839, or beginning of 1840, while on board a vessel on her voyage from Liverpool to America, he was precipitated from the topsail yard into the tops, by the mast giving way during a heavy gale of wind. He alighted on his head, part of the wreck at the same time falling upon him,—causing fracture of the femur, compound fracture of the leg, and, as we shall afterwards see, though he was not himself aware of it, a dislocation of the thigh upon the pubes—all of the right side. He remained for about an hour and a half insensible, and for several hours after felt stunned and confused. About the third or fourth day he became affected with jaundice, which lasted above a week. The vessel, being much damaged by the storm, put into Greenock for repair, where she arrived a fortnight after the accident occurred. During the whole of this time the poor fellow was without medical or surgical aid, except some rude bandaging from his messmates,—there being no surgeon on board. He was taken to Greenock Infirmary, where he remained above four months; and it was, after a few days' residence in that institution, that he was seized with his first epileptic fit.[1]

From that time till the period of his death these paroxysms continued, at first at intervals of six or seven weeks, and gradually becoming more frequent, till latterly they occurred about every ten days, though not with any degree of regularity.

With the exception of this disease, he enjoyed good health, and, indeed, after leaving the Greenock Infirmary, he went three voyages as cook. At first he was not unconscious during the fits, but for the last four or five years he was so.

When standing erect, the right inferior extremity appeared much shorter than the other; the thigh was considerably abducted, and capable of motion to a very limited extent, and the foot completely everted—causing, as may be supposed, a great halt and peculiar mode of walking. This, with the increasing frequency of the fits,

[1] I regret that I cannot give any history of the case while he resided in this hospital. I wrote a few days after the patient's death to the "senior surgeon" of the institution, and received a reply from the house surgeon, stating that he could find no such case recorded; nor, upon inquiry of the medical officers, did they recollect it. Thinking this might arise from a mistake of seven years that I had inadvertently made as to the time of his admission there, I wrote to that effect, but have not received an answer.

rendering him incapable of following his vocation, he became about four years ago an inmate of the Berwick Union Workhouse.

On the 8th of November last he complained to me of intense headach, with a feeling of great anxiety and restlessness, which, with total want of sleep, made me apprehensive that a paroxysm of unusual violence was about to ensue. He was accordingly removed to the hospital, and ordered an active dose of calomel and compound extract of colocynth; a mustard cataplasm to the nape of the neck; a hot foot-bath, and the application of cold water to the shaven scalp. For the remainder of his case I shall give a brief abstract of my private journal.

November 9, 11 A.M.—The nurse states that at 3 A.M., after a fearful scream, he was seized with a very violent paroxysm, lasting till 7; and after half an hour's remission another occurred, which continued till about half an hour ago. At present he is in a semi-comatose state; head hot; pupils contracted; pulse 110, very feeble; respiration laboured. Bowels have not been opened; ordered turpentine enema, a blister to the nape of the neck, and the application of ice to the head; small quantities of beef-tea at frequent intervals.

8 P.M.—Has had several slight fits during the day, and remains much the same as at the morning visit. The blister has not yet risen. The clyster brought away a large quantity of most offensive feculent matter.

10th, 11th, and 12th.—With the exception of a few hours' calmness on the night of the 9th, he has had a series of slight fits, lasting about five minutes, and with only about a quarter of an hour's remission betwixt each. On the evening of the 12th he became quite comatose; pulse 108, very feeble. Ordered a blister to the vertex and a turpentine enema.

13th, 11 A.M.—Appears better; is sensible to signs, but cannot speak, and is quite deaf; he follows with his eyes the movements of persons in the ward; the whole of the right side is paralysed; pulse 96, a little more firm; pupils not so much contracted.

He remained in this state till 9 A.M. of the 15th, when, after a violent scream, he was seized with a paroxysm, which continued without the slightest intermission till half-past 9 P.M., when he expired. The convulsions during this last attack were almost entirely confined to the left, or the *paralysed* side.

Inspection Sixteen Hours after Death.—The left arm was rigidly bent up towards the shoulder, and the fingers convulsively incurvated. The muscles of the left thigh and leg felt very rigid; the *tibialis anticus* and *extensor communis* muscles were so much on the stretch, in consequence of the convulsive flexion of the foot and toes, as to be distinctly traceable with the eye in their entire course. The muscles of the other leg and thigh were also rigid to the touch; marks of an old irregularly-shaped wound, about four inches long, over the middle third of the tibia, were very visible; and a hard

irregular swelling was distinctly to be seen and felt in the upper, anterior, and inner part of the thigh. This extremity was three inches shorter than the left.

On laying back the scalp there were no visible marks of fracture or other injury of the skull; neither, on opening the cranium, was there any evidence of disease or injury on its inner surface. The vessels of the dura mater were congested, but not more so than might have been expected from the difficult and imperfect respiration previous to death. Upon opening the membrane, a pale, greenish-yellow spot, not more than a few lines in diameter, and rising slightly above the level of the convolutions, was observable in the right anterior lobe of the cerebrum; it was hard to the touch, and in direct contact, but unconnected, with the dura mater, or rather its arachnoid lining. The brain having been removed, a corresponding spot was found on the inferior surface of the same lobe, along the inner surface of which, wound the olfactory nerve, smaller than its fellow of the opposite side. These spots were found to be opposite surfaces of a large tumour, completely surrounded, with these exceptions, by the cerebral substance, with which, however, it had no connection. A portion of the white substance opposite the middle of the posterior surface of the growth, of about three-fourths of an inch in diameter, showed marks of inflammation and softening; with that exception, the entire brain was healthy in appearance. The tumour was hard to the touch, having at the same time a feeling of elasticity, of a pale, greenish-yellow colour, with a somewhat rough and shaggy surface, and was of an irregularly round shape, measuring in its antero-posterior circumference eight and a quarter inches, and in the opposite direction eight inches, weighing five and a half ounces and fifty grains avoirdupois, the brain by itself weighing three pounds one ounce; it was supplied with blood by a branch of considerable size, proceeding from the anterior cerebral artery. On making a slight incision into the softest part of the tumour, a fluid, of a thick creamy consistence, and deep dull red colour, escaped, part adhering to the knife; and the entire contents were enclosed by a thick fibrous capsule. On examination by the microscope, the fluid matter which had escaped seemed to consist principally of blood corpuscles; but as I have had but little experience in the use of this most valuable aid to pathological research, I resolved to preserve the tumour entire for the benefit of some one more *au fait* with that instrument. [I have accordingly, at Dr Christison's request, sent it to Professor Bennett, whose well-known experience will, I trust, enable him to confer upon the readers of the Monthly Journal the advantage of his researches.]

On comparing the tumour with the descriptions of cerebral tumours given by Monro, Abercrombie, Craigie, Copland, Rokitansky, and in the "Cyclopædia of Anatomy and Physiology," it appears to me, so far as I have examined it, to coincide most nearly with that

called hæmatoma. On a late inspection of the specimens of cerebral tumours contained in the valuable Museum of the College of Surgeons of Edinburgh, there appeared none in the slightest degree resembling this.

The integuments and muscles having been removed from the anterior surface of the right thigh and pelvis, the head of the femur was found lodged upon the pubes, in a shallow cavity, formed partly by absorption and partly by bony deposit, the bone being kept in its place by a firm fibro-ligamentous attachment to the surrounding osseous surface, which allowed motion, but to a very limited extent. Signs of an old fracture betwixt the two trochanters were very observable. The surface of the bone was very irregular, from the deposit of callus; but it seemed as if the inferior portion of the bone had been driven like a wedge into the upper, to which it was united at an oblique angle. The tibia having been laid bare under the cicatrix formerly noticed, presented also indisputable evidences of an extensive and irregular fracture. It was not deemed necessary to open the thoracic or abdominal cavities, as none of the contained viscera had exhibited any symptom of organic disease during life.

Remarks.—The first thing that must strike every reader is the singular recovery of the patient from injuries so complicated and serious, and under circumstances so adverse, as those described in the early history of this case.

Under the most favourable auspices, and with every advantage of professional skill, a compound fracture of a large and important bone is justly considered a serious injury; but when, in addition, we have another important fracture and dislocation,—all remaining for a fortnight, during a storm at sea, without treatment, or very probably worse than none,—these, again, complicated with concussion of the brain and jaundice,—an escape from death does seem almost miraculous. To the surgeon, such a case would present no little difficulty; for, with fracture so very high in the thigh, it would be next to impossible, I should say, to return the head of the bone to its proper cavity.

That the epileptic fits were consequent upon the accident, there can be little doubt. The patient, previous to it, had had no paroxysm, nor any symptom of cerebral disease; and his mother and brother, of whom inquiries were carefully made, were aware of no hereditary predisposition to either. I cannot believe that to the injury of the head can be ascribed either the epilepsy or the post-mortem appearances. The concussion could not have been a very serious one. Its effects passed rapidly off; and, in the interval betwixt the fall and the first epileptic paroxysm, there was, as far as I could learn from the patient, no symptom showing any lesion or disease of the cranial contents. We must, then, I think, look elsewhere for the cause, and I am of opinion that we have sufficient in the nature of the injuries to the lower extremity. It is well known that injury, irritation, or

certain diseases of remote nerves, are capable of producing general convulsions; and that the two former must have existed in this case must be apparent, considering the length of time the jagged ends of the bones were left to move amongst the soft tissues: the position of the dislocated bone itself could not but produce stretching and compression of an important nerve,—the anterior crural,—and it must be recollected that, from the continued agony, want of sleep, and probably considerable hemorrhage, the patient was rendered in a most favourable condition for the operation of these causes. I am confirmed in this view of the case by the statement of the patient, formerly noticed, that at first he was not unconscious during the fit; thus making it appear that it was more of a convulsive than a regular epileptic character. The fits, with each repetition, must have produced a temporary derangement of the circulation of the brain, as well as irritation of that organ,—causes sufficient to excite the growth of this tumour. As it increased in magnitude, it would of itself become a source of irritation and cause of the disease, the fits increasing in frequency and severity as it grew in size, till at length, acting like a foreign body, it produced inflammation, softening, and consequent coma, paralysis, and death.

I believe that tumours found in the brains of epileptics are almost always the consequence, not the causes, of the disease; for how very few are the cases of early epilepsy in which any morbid appearances are found within the cranium?

It is singular that such an immense mass should have existed, without producing any degree of paralysis, or loss of intellect. For a man in his station, he was rather above the average degree of intelligence.

In regard to another complication in the case,—the jaundice,—that might be an accidental concomitant; but it is also probable that it arose from some local injury to the liver or its appendages at the time of the fall.

BERWICK-UPON-TWEED, January 14, 1851.

Note by Professor Bennett.

On examining the tumour, of the size, weight, and general appearance so accurately described by Dr Kirkwood, I found it to consist of a firm resistent shell or capsule, about one-eighth of an inch thick, containing coagula of blood of a brick-dust colour. The capsule, externally, was of a straw colour, like that of coagulable lymph; but two-thirds of its thickness, internally, was dark red, passing into black. A small portion of the external layer of the capsule, examined under a power of 250 diameters linear, presented a dense mesh-work of fibres, running in waved bundles, and varying in size from the one-twelfth to the sixteen-hundredth of an inch in diameter. The thicker internal layer was composed of similar fibres, mingled with masses of blood corpuscles, in various stages of disintegration. The internal coagula were composed of a few similar fibres, numerous molecules

and granules, and a mass of blood corpuscles, diminished in size, and variously altered in shape, but still presenting their normal yellow hue.

From the result of this examination, there can be little doubt that the tumour consisted entirely of a mass of blood which had been extravasated into the cerebral hemisphere. The external portion had coagulated in the form of molecular fibres, which, aggregating together, and becoming firmer, constituted the external capsule. The internal portion of the clot was also gradually undergoing those changes generally observed to follow extravasation of blood in internal parenchymatous organs.

This interesting case offers a wide field for speculation regarding the pathology and diagnosis of cerebral diseases; but as I propose to allude to it more particularly in another place, in connection with the subject of apoplexy, much repetition will be avoided by my not entering upon it at present.

ARTICLE V.—*Case of Placenta Prævia.* By WILLIAM SMELLIE, M.D., Wick, Caithness-shire, late of the H. B. Co.'s Service.

AT four P.M., on the 10th November last, I was called to attend Mrs R——, eight months pregnant of her sixth child. The information conveyed by the patient and the midwife in attendance was, that, for a fortnight previous, there had been a constant oozing of blood through the vagina, which had induced the patient almost closely to keep her bed; that for eight days she had felt no *life*, but a sensation of cold and weight in the region of the gravid womb; that for three days the discharge of blood had considerably increased. There had yet been no labour pains; and she was becoming much exhausted. I found the pulse very weak, and above 100; she complained of sickness and faintness, and the bowels were under the influence of castor oil, which had been taken the night before. On examination, I found the vagina filled with clots; and the napkins and towels which had been used were quite soaked with blood. A gush of blood followed the removal of the clots, but immediately ceased. After a lengthened manipulation, I failed to discover the os uteri, and for the time was compelled to defer further interference; and, having plugged the vagina, I ordered her a little wine, and left her, giving strict injunctions to the midwife to send for me on the first appearance of labour pains. I called again, at eight P.M. and found her as I had left her, no flooding having recurred; and, there being no pains, I did not remove the plug to make any examination. The pulse had become a little stronger. About midnight I was again summoned, with the intelligence that some slight pains had come on. I immediately removed the plug, and at once discovered a placental presentation, with the os uteri so far dilated as to admit the hand. I tried to extract the placenta before the

child, after the method proposed by Professor Simpson; but finding much difficulty in separating it, and being unwilling to lose time, as the flooding had returned, I at once thrust my hand through the placenta, which was inserted directly over the os uteri, found the head in the first cranial position, passed my hand upwards, seized the feet, and extracted by turning a still-born male infant, about the eighth month, and apparently about eight days dead. The placenta came away immediately after the child; the cord was of unusual length, and had three turns round the neck of the child.

My patient, though much exhausted, continued remarkably well until the third day, when she was seized with febrile symptoms, pulse full and 130, and severe pain in the hypogastrium;—these symptoms yielded to a full dose of calomel and opium, with hot poultices over the abdomen frequently repeated; and she subsequently made a good recovery. One or more of Mrs R.'s former children had been still-born, but not from a like casualty, as I learned from the midwife, who had attended her in all her confinements.

WICK, 13th January 1851.

ARTICLE VI.—*Case of Tumour, for which the Operation for Ovariotomy was Performed more than Twenty-five Years ago.* By JOHN YOUNG MYRTLE, M.D., F.R.C.P.E.

I FIRST visited Magdalene Berry, the subject of the following brief sketch, in 1838, when she was found suffering much from dyspnœa and general dropsy; she also informed me she had a tumour, which Mr Lizars had endeavoured to remove many years before, but was defeated, and that of late it had been greatly on the increase.

It may not be improper to recapitulate the history of the case, as recorded by Mr Lizars in his "Observations on Extraction of Diseased Ovaria." "Magdalene B., an unmarried woman, aged 34 years, about six years ago, while serving as cook to a family, observed a tumour, about the size of a hen's egg, in the hypogastric and right iliac regions, which occasioned no pain, until about six months afterwards, when she received a severe fall, while carrying a large basket. The pain at this period was so severe, and the tumour increased so rapidly, that she went into the infirmary of this place, when, after remaining for some time, she was dismissed as incurable. After this, she was recommended by a practitioner in town to apply leeches and chamomile poultices, and to undergo a course of mercury, all of which measures were put in practice, and so far relieved her, that she was able in three months to return to service, where she remained about half a year; when becoming alarmed from the rapidity of the increase of the tumour, which was now accompanied with occasional suppression of urine, and irregularity of the catamenia, she was compelled to

leave her service. She applied to several medical gentlemen, who prescribed various remedies, until calling on that scientific physician, Dr Poole, who thought there was some prospect of affording her relief by operation. The first time I saw her, the abdomen was fully larger than that of a woman in the ninth month of gestation, and felt hard and firm, so that I considered the tumour, from its immobility, to be strongly adherent to the parietes of the abdomen, and hence, that it was an unfavourable case for operation. Her health, however, was good; she had considerable muscular strength, had a healthy countenance, a good pulse, regular bowels, but the catamenia stopt; and having entreated earnestly that something might be done, as she felt herself becoming daily less able to assist herself or her family, the 24th of April was fixed for the day of operation. On the day preceding she took a cathartic.

"The day was remarkably cold for the season; for, although a large fire had been put on by seven in the morning, the thermometer had arisen only to 66° by one o'clock. When the heat of the room had risen to 70° of Fahrenheit, which was between one and two o'clock, a longitudinal incision was made through the integuments, from the sternum to the pubes. At the sternal extremity the peritoneum was wounded, and one finger of the left hand was here introduced, then another, and the peritoneum laid open to the pubes; the same was then done upwards to the sternum, when a multiplicity of convoluted vessels presented themselves, of various magnitude, from the thickness of a finger to that of a crow's quill. At first I thought them the intestines, for they appeared extremely fleshy; then I imagined them the blood vessels of a placenta, which they still more resembled; indeed, such was their resemblance to the vessels of that organ, that the same idea struck one and all of the gentlemen present. On minute examination, however, they were found to be the blood vessels of the omentum majus, enormously enlarged, running on the surface, and into the substance of the tumour, which appeared an enlarged ovarium. Finding that it was impracticable either to dissect these vessels from the surface of the tumour, or to secure them, in consequence of their great number, I abandoned the idea of extirpating the mass, in which decision I was supported by the gentlemen present. I, therefore, punctured, with a large trocar and canula, the centre of the tumour, but nothing flowed. I next made a small but deep incision with a scalpel, when the tumour appeared solid and cartilaginous, and a vessel bled a little. I lastly punctured the lower part of the tumour, being anxious to reduce its bulk, but only pure blood flowed. The lips of the wound were now approximated and stitched, adhesive straps applied, compresses of lint and linen, with a shawl as a binder, and the patient carried to bed. This operation was performed in the presence of James Dease, Esq., surgeon to the forces; Drs Poole, Campbell, and Millar; Messrs James Scott, George White, and many other surgeons and students."—*Observations on Extraction of Diseased Ovaria*, pp. 19, 20.

Soon after the operation, inflammatory symptoms supervened to a great degree, so that it was necessary to resort to sharp antiphlogistic treatment, opiates being freely administered, and blood drawn to the extent of 111 ounces, within thirty-six hours or so after the operation. In a few days the violence of the symptoms began to abate, and within a fortnight the patient was convalescent.

When I first saw her in 1838, on examining the abdomen, a broad cicatrix was observed extending from sternum to pubes, and evidently preventing, to some considerable extent, the equal distension of the abdomen. A firm tumour, about the size of the uterus in the fifth month of pregnancy, occupied the centre of the abdomen, with much greater fulness towards both hypochondriac regions, and a well-marked feeling of fluctuation over both those points, the abdomen, as a whole, being rather larger than at the full period of pregnancy.

As she suffered very much from bronchitis of a chronic form, and general dropsy, I then had recourse to a gentle course of mercury, combined with squills, small doses of digitalis, and other diuretics, and occasional purgatives, which were followed by the iodide of potassium and various bitters. The lower limbs were also regularly bandaged, as much on account of a painful varicose condition of the veins, as the œdema under which she laboured. Under this treatment the more urgent symptoms steadily disappeared, and, from the removal of the ascites, the abdomen was decreased to its size about the fifth month of gestation.

Everything in the shape of medicine was shortly afterwards given up, save the administration of mild purgatives and diuretics, as occasion required. Professor Simpson visited the patient with me two or three years ago, as a subject of professional interest, but made no special examination, as there were no symptoms present bearing upon the tumour.

The patient died from apoplexy on the morning of the 23d of November 1850; and two days afterwards a post-mortem examination took place in the presence of Dr Simpson and several other gentlemen,—Mr Lizars, though invited, being, from a professional engagement, unable to attend.

The abdomen did not appear larger than it does about the fifth month of pregnancy, the integuments being flaccid, save in the situation of the cicatrix formerly mentioned, which was from an inch and a half to two inches broad. The tumour extended right across the abdomen, but did not extend beyond the ribs, was moveable, but, from strong adhesions anteriorly, the parietes of the abdomen moved along with it. On opening the abdomen, the whole region of the parietes corresponding with the extensive cicatrix did not exceed about a sixth part of an inch in thickness, and adhered superiorly to the omentum very firmly, and inferiorly to the tumour over almost all its anterior surface, so that great care was requisite in this part of the dissection. The vessels of the omentum were very large, as represented by Mr Lizars, as also those on the surface of the tumour itself.

Strong and extensive adhesions existed between the tumour and intestines superiorly and posteriorly, as well as between the intestines themselves and the peritoneum. The tumour itself and its connections were disturbed as little as possible till the arrival of Professor Simpson, who could not be present till this point of the proceedings; he then pointed out both ovaries, small, though healthy, and in their normal position, and stated that the tumour was a fibrous growth of the uterus itself.[1] The tumour was attached to the fundus by a pedicle, between two and three inches long, which was found, on careful dissection by Professor S. afterwards, to be a fold of peritoneum, the uterus being so atrophied as to make but a slight inequality in the appearance of the vagina and pedicle, and could be but little distinguished by the touch, as they were much of the same breadth and thickness, and ran quite in the same mesial line; the uterus being, before the careful examination above mentioned, thought to be merely the os tincæ. The preparation is now in the possession of Dr Simpson.

Dilatation and softening of the heart existed to a great extent, as also a general varicose state of the veins.

I was certainly always in the belief that this was ovarian disease, from what was recorded in its previous history, as well as from the means of diagnosis within my own observation; and while the ascites existed to a great extent, the difficulty of diagnosis was to no small degree increased, on account of the peculiar effect of the very strong adhesions, dividing, as it were, the abdomen into something like two

---

[1] In the "Lancet" for 18th and 25th January, pp. 80 and 93, it is stated that Dr Robert Lee, of London, mentioned to the Medico-Chirurgical Society that Dr Simpson believed the tumour to be ovarian, until the two unaffected ovaries were shown to him at the dissection. Now Dr Simpson never made, during the patient's life, any physical examination or diagnosis of the case, so as to ascertain the precise nature of the tumour, and certainly at the dissection he was, as I have stated, the first to point out to us that the tumour was truly a fibrous tumour of the uterus, and that both ovaries were untouched. On having my attention drawn to the "Lancet," I wrote a letter to Dr Simpson, pointing out the entire incorrectness of the statement that had been given to the Medico-Chirurgical Society by Dr Lee; and I understand this note was forwarded by Dr Simpson to Dr Lee, with a request that, as he had made inadvertently a mis-statement of the facts to the Society, he would now be so good as set the matter right. But "there are more false facts than false theories in medicine," said Dr Cullen, and so it has proved in the present instance. Dr Lee stated, in relation to this individual case, what was incorrect, to the Fellows of the Medico-Chirurgical Society, and his mistake (though pointed out) is yet unretracted. This fact will show the degree of reliance that the Fellows of the Society ought to place upon any similar statement of Dr Lee's. He gave a statement, some months ago, to the Society, relative professedly to all the known cases of the operation that M. Berry was submitted to,—viz., ovariotomy, and their results. But if, with regard to this single case, Dr Lee has fallen into, and persists in, error, of what use is it that he should pass in review all the other cases of the operation? What credit can we attach to his results?

cavities longitudinally, the firm fibrous tumour being in the centre. At an early stage of the complaint, it seems probable much light might have been derived from local examination, and the use of the uterine sound, of so much utility in obscure affections of those organs; but from the length of the pedicle, and the atrophied state of the uterus and ovaria, it is somewhat questionable, as far as I can understand, how far we might have been led to a true and accurate knowledge of matters, which, considering the almost universal mortality attending removal of the uterus, or tumours so situated and connected as the present, is no doubt a matter of weightiest importance. There can be but little doubt, however, that had the tumour been really an ovarian one, this case might have been added to the record of successful cases of ovariotomy, seeing the main dangers were fully incurred as well as successfully withstood, as may be well observed alike from the history of the treatment of the case in Mr Lizars' observations as from the strongly-marked results of violent inflammatory action, perceptible on dissection.

Mr Lizars mentions that, previous to the operation, the abdomen was larger than at the full period of gestation, but takes no notice of fluid in the cavity, or anything else, to account for it; the tumour he distinctly specifies as very firm and cartilaginous; and though, according to the account of the patient and her brother, a seton had been afterwards drawn through the tumour, yet we cannot well understand the discrepancy as to bulk, after the ascites was removed, up to the period of her death, unless we account for it by a very considerable decrease in the quantity of fat, as tumours of such consistence are not regarded as likely to be greatly diminished by absorption from any cause.

Mr Lizars also mentions in the details of this case, previous to the portion I have taken the liberty of extracting, that the patient complained from time to time of retention of urine; but there seems no mention made as to the length of the pedicle, which might have some effect as to that point, as she had not been troubled to any extent latterly with urinary complaints; nor was there any impediment to the action of the alimentary apparatus, as might have been expected from the very firm and extensive adhesions, especially from the close manner in which the tumour was connected with them.

As far as I have seen recorded, operations by peritoneal section, for tumours of the uterus, have been, in a measure, uniformly fatal where they have been removed,—a few illustrative cases may be enumerated.

In the 17th No. of the "British and Foreign Medical Review," a case is recorded, in which Mr Heath, of Manchester, on operating for ovarian tumour, found it a uterine growth, which was removed, with a fatal result in seventeen hours.

Dr Olay, of Manchester, records a case of a similar character, in

which ligatures were applied, but the tumour not removed, and a fatal event took place in an hour and a half. He has also another unsuccessful case; but I am not certain as to whether the tumour was removed or not. His third case, in which both ovaries and uterus were removed, survived till the fifteenth day after the operation.

Dr Atlee, of Philadelphia, in the 38th No. of the New Series of the "American Journal of the Medical Sciences," likewise relates a case of abdominal tumour,—in which he had performed gastrotomy, but found uterine disease,—which was not removed; the patient recovered to no inconsiderable extent; and died, several months afterwards, apparently of peritonitis and erysipelatous inflammation along the course of the abdominal cicatrix, shortly after an exploration, for the purpose of enucleation, preceded by incisions into the os uteri.

Dr Francis, in his American edition of Dr Denman's work on midwifery, relates a case as coming under his own observation, and gives a drawing of the tumour; the subject of the case was thirty-two years of age; the uterus and appendages were of the natural size, but the main tumour was attached to the fundus by a pedicle, about an inch long, was above ninety pounds in weight, fibrous in structure, and had six tumours attached to it, the largest of which amounted to little short of two pounds. This case occurred in the practice of Professor Mott, of New York; but there was no operation. The tumour here resembled much, in some points, that found in Magdalene Berry,—in position, origin, and structure; but differed, so far as it was fatal in early life, and was unaccompanied by any appreciable change in the uterus itself.

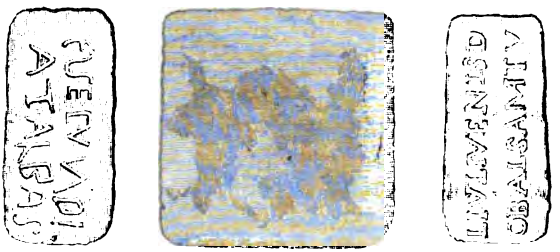
In the "Library of Medicine," Dr Simpson throws out some very appropriate remarks concerning the various bearings of fibrous growths connected with the uterus, and which account for the tumour in the case of M. B. having either existed in the walls of the uterus near its external surface, or immediately under the peritoneum, and in its gradual development following the law there laid down, tending towards the surface to which it was nearest, and rising, like the impregnated uterus, into the abdomen, carrying its long narrow peduncle of peritoneum along with it.

M. Boivin makes mention of small pedunculated tumours, external to the uterus, and gives a variety of illustrations. Dr Churchill and many others also notice the occurrence of similar fibrous growths, but give few cases closely resembling the present.

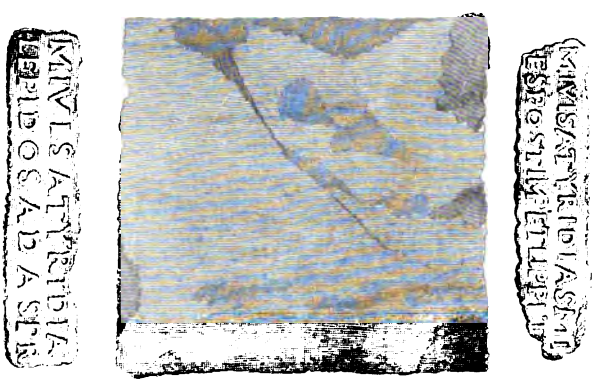
In conclusion, it may be worthy of observation, that though the tumour in this case had been one capable of successful removal, the operation might not have been the means of prolonging life, though it might have greatly conduced to the patient's comfort in the way of relieving the varicose condition of the veins, which was such a fertile source of pain and disability for many years.

No V

No VII



No VI



ARTICLE VII—*Notices of Ancient Roman Medicine-Stamps, &c., found in Great Britain.* By J. Y. SIMPSON, M.D., Professor of Midwifery in the University of Edinburgh.—*(Continued from page 50.)*

IN the first part of these Notices (see Monthly Journal for January), I have described four of the Roman medicine-stamps found in Great Britain.[1] I have now to give some account of several other similar relics of the Roman medical men who practised in these islands. In doing so, I shall follow an order chronological in reference to the time of the discovery of the stamps. No. IV. (which I have already described in my former communication) was the first specimen discovered in Great Britain, and was found early in the last century. The fifth, which I have next to notice, was brought to light some years subsequently.

No. V. This stamp was found at Bath, in the year 1731, in a cellar in the Abbey-yard. It was shortly afterwards exhibited to the Antiquarian Society of London by Mr Cutler. Mr Mitchell of Bristol, who possessed the stamp about the middle of the last century, submitted it also for examination to the Royal Society of London. I have, through Mr Norman of Bath, and other friends in England, attempted to trace out the present proprietor of the stamp, with a view of ascertaining more correctly the exact nature of the inscriptions upon it; but these efforts have been quite unsuccessful.

Mr Lethiecullier presented to the London Antiquarian Society plaster-casts of the inscriptions on the stamp; and three of these plaster impressions of it are still preserved in the London Antiquarian Museum. These plaster-casts, however, are very imperfect, and the lettering upon them is now defective at some of those very points that are otherwise the most difficult to determine.

Some manuscript notices of this Bath medicine-stamp exist in the Minute-books of the Antiquarian Society (vol. IV. [1744], p. 210; and vol. VIII. [1757], p. 29), with an impression taken with ink from the inscriptions. For copies of these I am deeply indebted to the polite kindness of Mr Akerman, the distinguished secretary to the Society. The outline in Pl. VI., No. V., presents a copy of a rude drawing of the Bath stamp given in the minute-book of the Antiquarian Society for April 27, 1732.[2] I am indebted to Mr Akerman for this sketch, which is interesting as giving us the form of the stone. There is given retrograde, as on the ori-

[1] I omitted to state that the two stamps in the British Museum, furnished by Mr Birch (see Nos. III. and IV.), are supposed to have formed part of the collection of Sir Hans Sloane; but no note exists as to the locality in which they were originally discovered.

[2] Catalogue of Antiquities, Coins, &c., in the possession of the Society of Antiquaries of London, by Albert Way, Esq. 1847, p. 12.

ginal stone, on the exposed sides of this sketch, one of the inscriptions corrected from the pertaining plaster-cast in the Museum; and below it, in the Plate, is an impression of this inscription.

In 1788 Mr Gough published, in the "Archæologia," "Observations on Certain Stamps or Seals Used Anciently by the Oculists;"[1] and in this communication he has given, amongst others, copies of the inscriptions on the medicine-stamp found at Bath; but without making any attempt to read and decypher these inscriptions. He appears to have seen the stone itself, as he describes it as "square, of a greenish cast, and perforated." He gives the following as the legends or inscriptions on the four sides of the stamp:[2]—

1. T. IVNIANI THALASER
   AD CLARITATEM
2. T. IVNIANI CRSOMAEL
   IN M AD CLARITATEM
3. T. IVNIANI D[illegible]VM
   AD VETERES CICATRICES
4. T. IVNIANI HOFSVMADρV
   EC VMODELICTA AMEDICIS

The two first of these inscriptions are given with sufficient distinctness and accuracy, and do not offer any great difficulties in the way of explanation. But the two last have been copied so imperfectly,—and, perhaps, so inaccurately,—by Mr Gough, as to surround their meaning with no small degree of uncertainty and doubt.

In all of the four inscriptions, the name of the proprietor or oculist, T. JUNIANUS, is perfectly distinct. The first side reads as follows:—

1. T. JUNIANI THALAS*s*ER AD CLARITATEM. *The Thalasser (or Marine Collyrium) of T(itus?) Junianus, for clearness of vision.*

The collyrium *Thalasseros* (θαλασσερος) is mentioned by several of the old Greek and Roman authors, who have discussed the subject of diseases of the eye and collyria, as by Galen,[3] Myrepsus,[4] Aetius,[5] Alexander Trallianus,[6] and Paulus Aegineta.[7]

The name itself—*Thalasseros*—is evidently derived from θαλασση, the sea. Fuchs, the translator of Myrepsus, avows that he can form no conjecture as to why the collyrium was termed Thalasseros (quam autem ob causam nescio). In Cornarius' translation of Aetius, it is entered as "Thalasserum *hoc est marinum.*" And in all prob-

[1] Archæologia, or Miscellaneous Tracts relating to Antiquity, published by the Society of Antiquaries of London, vol. ix., p. 227.

[2] For the purpose of explanation, I have changed the order of the sides, bringing forward as the 2d what Mr Gough gives as the 4th side.

[3] Kuehn's Galen, vol. xii. [4] Sectio xxiv., De Collyriis, p. 662.

[5] Tetrabiblos Sermo iii., cap. 110, p. 434.

[6] De Arte Medica, lib. ii., cap. v., p. 175.

[7] Dr Adams' Translation, vol. iii., p. 554; and vol. i., p. 421.

ability it originally received its high-sounding and attractive appellation from the marine colour of the preparation, the hue of the collyrium being, as we have already seen, sometimes the cause and source of its distinctive appellation, as in the collyria termed Melinum, Cygnus, Cirrhon, &c. It has been conjectured that the name was imposed upon it in consequence of one or other of its ingredients being of marine origin. But in none of the formulæ given for it by the authors already named, does any sea ingredient enter into its composition.[1]

The object of the *Thalasseros* in our inscription was to produce clearness of vision (claritatem). Galen describes the *Thalasseros* of Hermophilus as "accomodatum ad suffusiones et ad omnem hebetudinem visus; facit et ad incipientem suffusionem" (vol. xii., p. 781). Myrepsus assigns to it the powers of "lachrymas retinens, ad inchoantes suffusiones et nyctalopas, et ad recentem pupillæ dilatationem" (sect. xxiv., cap. 51). It is adapted, according to Tralhianus, "ad hebetudinem, et incipientes suffusiones; et callos exterit." (Lib. ii., cap. v., p. 175.)

2. T. JUNIANI CERUSSOMAELINUM AD CLARITATEM.—*The Leaden Melinum (or Golden Yellow Collyrium) of T. Junianus, for clearness of vision.*

I have already had occasion to speak of the signification and qualities of the collyrium named *Melinum*. In the Colchester stamp the *Melinum* is invested with the same supposed properties as the *Crsomelinum* in the above legend on the Bath seal,—namely, "ad claritatem."[2] The prefix CRSO, in *Crsomelinum*, admits of more than one interpretation. Galen gives four different formulæ for collyria *Melina*. Three of these contain, as one of their ingredients, the *Cerussa*, or carbonate of lead; and the prefix CRSO, may possibly stand as a contraction for *Cerussa*, implying the presence of this medicine in the collyrium. And in relation to this view, it is to be recollected that this preparation of lead was, in these ancient times, held in some esteem as a local application in eye diseases. Galen recommends it as an anodyne in pains of the eyes, and as a general astringent and sedative application.[3]

Another, and perhaps more probable meaning, has been suggested to me by my friend M. Sichel. He supposes the CRSO to be a contraction for CHRSO, *golden* (from χρυσος gold), the prefix marking the golden colour of this *melinum*, or yellow collyrium. In this way we would have Junianus selling his "Golden Yellow Collyrium" to

[1] Thus, for example, the ingredients in the collyrium Thalasseros, as given by Paulus Aegineta, are "calamine, 8 ounces; verdigris, 2 ounces; Indian ink, 8 ounces; white pepper, 4 ounces; median juice, 1 ounce; opobalsam, 2 ounces; and gum, 6 ounces."

[2] Kuehn's edit. of Galen, vol. xii., p. 769 and 786.

[3] Commentar: in App., vi. 31. De Simplic: Med., ix. 39. Adams' Paulus Aegineta, vol. iii., p. 420.

the colonists and natives of Bath some sixteen centuries ago. And we all know that "Golden Ointment" for the eyes is an application not by any means unknown to the practitioners and apothecaries of our own times.

3. T. JUNIANI DIEXUM AD VETeRES CICATRICES.

In the above line I give the reading of the third side of the Bath medicine-seal, such as it stands copied into the manuscript minute-books of the Antiquarian Society for 17th Nov. 1757. By turning back to the inscription, as given in a previous page, from Gough, it will be seen that the medial letters IEX are in a rude Brittano-Roman character, which allows us only to guess at their true signification. Unfortunately, the plaster-cast of this side of the stamp does not happen to be preserved with the others, so as to enable us to ascertain the probability of either reading; and it is more than doubtful whether the inscription thus given by these opposed authorities is correctly copied, either by Gough, or in the Society's minute-book. And I believe I state the general experience of all who have worked at the decyphering of Roman and other inscriptions, in observing that the perplexities connected with the reading of them has often been produced, much more by grave errors in the published copies of the inscriptions, than by actual difficulties in the decyphering of the original, after a true copy has been once obtained.

In the present instance, by reversing the usual mode of procedure in such investigations, we may perhaps arrive at the probable truth. In other words, if we consider the disease prescribed for, we may possibly arrive at a knowledge of the drug prescribed. Now the affection on this side of the Bath stamp is *old cicatrices* (*Veteres Cicatrices*). This disease, or rather result of disease, is mentioned on various Roman medicine-stamps discovered on the continent of Europe, as on examples found at Verona, Lilleborne, Nais, and St Evroult, and in one which I shall notice in the sequel, lately detected in Ireland. In all the instances which I have just named, the collyrium indicated on the inscriptions as the remedy AD VETERES CICATRICES, is the collyrium termed DIAMYSOS or DIAMYSUM, which contained, as its principal ingredient, the metallic preparation known under the name of Μισυ, or Mysy, among the ancient medical authors; and Marcellus Empiricus gives a formula for the formation of a collyrium DIAMYSOS from it. Looking to these facts, in relation to other analogous Roman medicine-seals, it seems not an improbable conjecture that the word on this third side of the Bath stamp is the same, perhaps more or less mis-spelt or contracted; and consequently, that the whole inscription is *T. Juniani Diamysum ad Veteres Cicatrices*. The re-discovery of the stamp itself can alone settle this and other difficulties connected with it.

If we judged of the nature of the inscription by the characters of the letters, as given by Gough, the disputed word might perhaps be more correctly read DRYCUM or DRYXUM. And possibly, in this

way, it may signify an astringent and detergent collyrium, made from the bark, acorn, or galls of the Drys (δρυς) or oak—a tree that held a place in the materia medica of Hippocrates, Galen, and the other ancients, and which still maintains its place in our own modern pharmacopœias. Dioscorides and the other old pharmaceutical authorities, describe the Drys or Quercus as possessing desiccant, astringent, and other properties; and they attribute especially these powers to the gall excrescences, or, as they incorrectly deemed them, the fruit of this tree. According to Oribasius, the gall of the oak —"siccat, repercutit, contrahit, constringit, et particulas infirmas roborat."[1]

Further, in favour of the present supposition, that the collyrium of the inscription may possibly be named from the *Drys*, I may take the present opportunity of mentioning that the ancient Roman oculists seem to have pursued, in regard to old cicatrices of the eye, a treatment which is not followed by their successors in modern times. "All cicatrices on the transparent part of the eye," says Aetius, "appear white, omnes cicatrices in nigro oculi *albae* apparent;"[2] and consequently give, by their presence, a disagreeable and disfiguring effect to the eye.[3] Some of the Roman oculists seem to have used various collyria, for the purpose of dyeing or changing the colour of these white cicatrices, and of thus imparting to them some kind of tint that rendered the appearance of the eye, and the distinction between the transparent cornea and its white cicatrices, less marked and striking. For this purpose the gall-nuts of the oak or *drys* appear to have been greatly used. Aetius does not approve of the practice of tinting cicatrices; but, in a chapter bearing the heading of "Albuginum Tincturæ," he describes half a dozen applications and collyria that might be employed for the purpose of staining and correcting the colour of old cicatrices of the eyes, lest, he adds, his readers should be ignorant of the means which might effect this (ut ne ignorentur ea quæ hoc facere possunt). In three or four of these collyria the gall-nut forms a leading ingredient,[4] and it seems to have been generally used previously to, or in combination with, blue vitriol (*atramentum sutorium*). Myrepsus gives a "collyrium tingens crassas albugines et cicatrices," containing galls with chalcanthus, or copperas, roasted lead, &c.; and a second formed of burnt and washed lead, &c., combined with unripe galls.[5] Paulus Aegineta mentions two dyes for cicatrices, both of them containing galls along with chalcanthus.[6] Alexander Trallianus gives a collyrium

[1] Medicinalium Collectorum, Lib. xiii., p. 490.

[2] Tetrabiblos, Sermo iii., cap. 37, p. 382.

[3] Galen defines cicatrices and albugo of the eye as follows:—"Cicatrix ppellatur ubi nigro oculi ex alto ulcere membranæ crassities supervenit, t color *albior* apparet. *Albugo* nihil a cicatrice differt, nisi quod ex ulcere major cicatrix simul et crassior in iride nascitur," vol xiii. p. 775.

[4] See Tetrabiblos, Sermo iii., cap. 40, 37.

[5] De Collyriis, cap. 72 and 73.

[6] Dr Adams' Translation, vol. i., p. 418.

for staining cicatrices, which he pronounces "valde generosum." It consists principally of chalcanthus and galls.[1]

4. T. JUNIANI HOBSUM AD ρUECUMO DELICTA A MEDICIS.

This fourth legend on the Bath stone offers the most puzzling of all the inscriptions hitherto found upon the Roman medicine-stamps discovered in Great Britain. As Mr Gough gives it, the last words of the inscription (DELICTA A MEDICIS—esteemed by physicians) are alone intelligible. The plaster cast of this side of the seal, contained in the Museum of the Antiquarian Society of London, contains an extremely imperfect copy of the second line, and not an over perfect one of the first; but we see enough in it to be quite aware of the great carelessness with which Mr Gough had originally copied the whole inscription. The second last letter in the line is not the Greek ρ, but the Latin Q; and the name of the collyrium is not HOFSUM, as he gives it, but apparently PHOEBUM. At all events there is a P, which he has omitted, before the H; and the two medial letters, which he read F S, are seemingly E B. Such is the conclusions to which the examination of the lettering of the cast itself forces me; and what is much more important, because affording far stronger evidence than mine, Mr Akerman reads this inscription in the same way. I may add, that (as I am informed by the same gentleman) the word is copied and written as PHOEBUM, in the several notices contained in the minute-books of the Antiquarian Society, and to which I have already referred; and Gough's ρ always given as Q.

Still, with all these emendations, I confess myself quite at a loss to decipher, satisfactorily, the inscription. The spelling of all the inscriptions on this stamp is executed very carelessly,—as in *crsomaelinum* for *crysomelinum; thalaser* for *thalasser;* and possibly the term QUECVMO may be a mis-spelling by the engraver for LEUCOMA. If so, the inscription would stand as

T JUNIANI PHOEBUM AD LU
ECOMA DELICTA A MEDICIS.

"*The Phoebum of T. Junianus for Leucoma, esteemed by physicians.*"

I am not aware that any of the old authors have described a collyrium under the name of PHOEBUM. But it looks like one of those high-sounding titles which the oculists were so fond of selecting and assuming, and we find described in their works collyria with such semi-astronomical appellations, as *Sol, Aster, Lumen, Phos,*[2] *&c.*

I shall venture only one more remark, viz., the possibility of the

[1] De Arte Medica, lib. ii., cap. vi., p. 175.

[2] Thus Nicolaus Myrepsus describes the "Collyrium nominatum Sol:" the "Collyrium Aster, hoc est stella;" the "Collyrium dictum Lumen." See his Opus de Compositione Medicamentorum, sect. xxiv., cap. 2, 3, 22, &c. Trallianus describes the collyrium Phos, &c., p. 174.

term being PHORBIUM and not PHOEBUM. "The Phorbium," observes Galen, "possesses attenuating, attractive, and discutient powers. They apply its seeds, mixed with honey, to Leucoma; and it is believed to have the power of extracting spicula of wood."[1]

No. VI.—Mr Douce published, in the "Gentleman's Magazine" for 1778, p. 472, a notice of a square flattened Roman medicine-stamp, a quarter of an inch thick, and each side or edge measuring about two inches. These four sides contained the following series of inscriptions:[2]

1. MJULSATYRIDIA
   LEPIDOSADASPR
2. MJULSATYRIDIASMIr
   (n)ESPOSTMPETLIPPIT
3. MJULSATYRIDIALI
   BANUADSUPPURAT
4. mJULSATYRIPENI
   CILLENEEXOVO

The name of the oculist, M. JUL. SATYRUS, is sufficiently distinct, and occurs with each of the four legends of the stamp. When we analyse further the inscriptions on the four sides of the seal, they severally read as follows:—

1. M (*arci ?*) JULii SATYRI DIALEPIDOS AD ASPeRitudines.—*The Dialepidos or Copper collyrium of Marcus Julius Satyrus, for granulations of the eyelids.*

The three first sides of this stamp have the special collyria inscribed upon them, beginning each with the letters DIA, from the Greek preposition δια, "with," and here signifying "made with." The three principal ingredients in the three first inscriptions are all given, combined with this initial preposition δια, and under their Greek appellatives, λεπιδος, σμυρνα, and λιβανος, forming instances, among many others, of the anxiety of the ancient Roman oculists to invest their drugs with all the mysterious attraction and formality of a Greek name; just as some modern English physicians foolishly enough consider it still proper to write always the names of the drugs which they now prescribe in the language of the ancient Romans, thus, like their predecessors, acting upon that principle in the weakness of human nature which holds "omne ignotum pro mirifico."

---

[1] De Simplic. Medicam. Kuehn's Ed., xii., p. 152.

[2] Mr Gough published, in the "Archæologia," a sketch of this stone, which is copied into Pl. II., No. VI. Some wax impressions were taken of the stone, but the stone itself was (it is stated in the same volume of the "Gentleman's Magazine," 1778, p. 510), "lost out of a pocket that had a hole in it, and probably, instead of gracing a Museum, has contributed its mite towards mending the roads."

I have already described (see No. II., side 2) the composition of the collyrium termed *Dialepidos*, and the origin of the name of the inscription from the *lepidos*, or scales of the oxide of copper.

2. M. JUL. SATYRI DIASMIrnES POST IMPETUM LIPPITudinis.—*The Diasmyrnes or Myrrh collyrium of M. Jul. Satyrus, after the commencement of ophthalmy.*

The principal ingredients in the collyrium *Diasmyrnes*, namely, myrrh (μυῤῥα or σμυρνα), was a drug to which important therapeutical virtues were formerly ascribed. It was applied in the treatment of various diseases; and in reference to affections of the eye, it had the power, according to Dioscorides, of filling up ulcers of the organ, removing cicatrices and scales obstructing the pupil; and besides, it cured eruptions and granulations of the eyelids (oculorum ulcera complet, exteritque albugines, et ea quae pupillis tenebras offundunt; quin et scabritias seu asperitudines expolit).[1]

Various collyria, bearing the name of Diasmyrnes, or Diasmyrnon, from myrrh being their leading ingredient, were used by the ancients. Aetius has one of his long chapters on collyria headed "Collyria Diasmyrna et Chiaca appellata."[2] Actuarius, in his section "De affectionibus Oculorum," speaks of the collyria *Diasmyrna* (quæ ex myrrha constant) in the plural number, and as well known in his time.[3] Paulus Aegineta, in discussing the treatment of hypopion or suppuration in the cornea, speaks of sometimes making the abscess burst; and if so, then, he adds, "cleanse the ulcer by means of the more potent remedies, such as those called *Diasmyrna*,"[4] &c.

Galen gives several collyria *Diasmyrna* as the Diasmyrnum Odorum Synerotis,[5] the Diasmyrnum Glaucidanum, and the Diasmyrnum ex hœmatite.[6] And in his work "De Simplicium Medicamentorum Temperamentis ac Facultatibus," he states, "Sed et collyria sunt plurima quæ medeantur suffusionibus, et maxime quod plurimam recipit myrrham, quale est quod a Democrate compositum est, quod vocant *Diasmyrnon*."[7]

3. M. JULII. SATYRI DIALIBANUm AD SUPPURATionem. The *Dialibanum, or Incense collyrium of M. Jul. Satyrus for Suppurative discharge from the eyes.*

Frankincense (*thus*, λιβανος λιβανωτος)[8] was frequently used by the

---

[1] Opera, Lib. i. cap. 77, p. 43. See, also, on its properties, Galen's works, by Kuehn, vol. xii., p. 127; Adams' Paulus Aegineta, vol. iii., p. 349.

[2] Tetrabiblos, Sermo III., cap cix., p. 429.

[3] De Methodo Medendi, Lib. vi., cap. v., p. 310.

[4] Paulus Aegineta's Works, Vol. i., p. 417.

[5] Kuehn's Edition, vol. xii., p. 774.

[6] Ibid, p. 746.

[7] Ibid, p. 257.

[8] Saffron, Crocus, and Myrrh form, as we have seen, prominent ingredients in the ancient collyria. Actuarius lays down the differences among the therapeutical effects of these three eye-medicines, with the following rare subtlety:—"*Crocus* et *Myrrha* hoc inter se dissident, quod ille moderate adstringat, hæc

ancient oculists in their collyria. According to Galen[1] and Paulus Aegineta,[2] in consequence of its detergent powers, it apparently cleanses and fills up ulcers in the eyes (expurgare et implere quæ in oculis consistunt ulcera videtur). It has the power, according to Oribasius, "astringendi, calefaciandi, *caliginem oculorum* discutiendi, cava ulcera implendi, ad cicatricem perducendi,"[3] &c.

Alexander Trallianus gives a formula for the collyrium *Dialibianum ad chemosim efficax;* and he describes the Dialibanum as, like the Libianum, of much use in eye diseases, and particularly for inflammations which are accompanied with ulceration (multi est usus, maxime ad inflammationes quae cum ulcere infestant).[4] Celsus recommends it in ulcers of the eye following pustules (fit quoque proprie ad hæc quod διὰ λιβάνου vocatur).[5] Marcellus Empiricus, who offers two recipes for its composition, ascribes to it the power of being efficacious in the disease noted on our inscription, namely, "ad suppurationes oculorum."[6]

When speaking of the treatment of suppuration of the eye, Galen lays down the following indications for the use of the *Diasmyrnes*, and *Dialibanum*:—"At quando pus, quod in oculis est, digerere placet, collyriis quæ myrrham habent, maxime utemus; quæ utique et *Diasmyrna* Graeci proprie vocant; his certe minus, sed reliquis melius faciunt quæ *Dialibanum* vocant."[7]

4. (M) : JUL : SATYRI PENIC*illum* LENE EX OVO. *The mild Penicillum of M. Jul. Satyrus, to be used with an egg.*

The term *Penicillum* has been found inscribed on several different Roman medical-stamps, as upon specimens discovered at Vieux and Paris, each marked with *lene penicillum;* upon one discovered at Nais (*penicillum ad omnem lippitudinem*); and upon another at Famars. Its signification has given rise to several opinions somewhat differing from each other.

M. Grivaud considers the *Penicillum* indicated on the Roman medicine-stamps, to be merely a small brush or hair-pencil, such as is still used at the present day to wipe away the more viscid discharges that may be found adhering to the palpebræ and eyelashes.[8] According to M. Sichel, the *Penicillum* consisted of a pledget or folds of charpie, which they used both for the purpose of cleansing the eyes, and of introducing into them soothing washes and collyria.[9]

---

vero citra adstrictionem non instrenue discutiat, humiditatisque exsiccet: suntque generosiora facultatibus quam *Thus*, quapropter etiam discutiunt magis, verum quod detergendi vi careant: in ulcerum curatione thuri ceu inferiora cedunt."—De Methodo Medendi, lib. vi., cap. v., p. 305.

[1] Kuehn's Edit., Vol. xii., p. 60. [2] Dr Adams' Edit., Vol. iii., p. 217.
[3] Medicinalium Collect., Lib. xii., p. 425. [4] De Arte Medica, Lib. ii., p. 173.
[5] Milligan's Ed. of Celsus, p. 290, Lib., vi. 13. In Aldine Ed., 1528, διὰ βαλάνου.
[6] De Medicamentis Liber, cap. viii., p. 280.
[7] De Methodo Medendi, Lib. iv.
[8] Recueil de Monuments Antiques, tom. i., p. 281.
[9] Cinq Cachets Inedits des Oculistes, p. 13.

M. Eloi Johanneau,[1] and M. Duchalais,[2] describe the *Penicillum* as a soft and fine sponge, employed in applying collyria to the diseased eye. Blancardi, in his Lexicon Medicum, defines the word *Penicillum* as "lint reduced to charpie, and besmeared with ointment to be applied to ulcers."

The word *Penicillum* occurs in the writings of Celsus and Pliny, and is used by these ancient authorities in such a manner as to give countenance to each of the preceding opinions. Thus Celsus, in his observations on the diseases of the eye, twice uses the term. In inflammation of the eye, he recommends the eyes to be fomented with a *Penicillum*, squeezed out of a warm watery decoction of myrtle or rose leaves, before local medicines and collyria are applied to them.[3] In another passage, he states that in intense ophthalmia the white of an egg or the milk of woman, dropped into the eye with a *Penicillum*, relieves the inflammation, and that this may be used by the patient when neither a physician nor other medicines are at hand.[4] Here we have the *Penicillum* used by the same author as a mechanical means both of cleansing the eye and of making local applications to it. Pliny, in his Chapter on Sponges (De Spongiarum Natura), speaks of a variety of very fine sponge under the name of *Penicillum;* and this, when soaked in a preparation of honeyed wine (mulso), was applied to tumours of the eyes.[5] These *Penicilli* were also (he adds) useful, when very soft and fine, in cleansing the eye in ophthalmy.[6]

There is, therefore, no doubt that the term *Penicillum* was used to designate a soft sponge, and perhaps also a brush or pledget of charpie that was occasionally employed in ophthalmic practice, for the double purpose of fomenting or cleansing the eye, and of dropping local applications into it. But it seems very unlikely that a stamp should be used by the oculist to mark the material of these Penicilli with. It would be both difficult and unnecessary to stamp in any way either a piece of sponge or of charpie with such an inscription as that found upon this and the other Roman seals. And

[1] Bottin ; Melanges d'Archæologia, p. 114.

[2] Observations sur les Cachets, p. 41.

[3] "*Penicillo* fovere oculos oportet, ex aqua calida expresso, in qua ante vel myrti vel rosæ folia decocta sint."—*Milligan's Celsus.* Liber vi., cap. vi., sec. 9, p. 288.

[4] "Quo gravior vero quæque inflammatio est, eo magis leniri medicamentum, debet, adjecto vel albo ovi, vel muliebri lacte. At si neque medicus, neque medicamentum præsto est, sæpius utrumlibet horum in oculis *penicillo* ad id ipsum facto infusum, id malum lenit."—*Ibid.* Lib. vi., cap. vi., sec. 8, p. 286.

[5] On the Properties and Uses of *Mulsum* in Ocular Medicine. See a very full account in the Second Book of Alexander Trallianus, p. 176.

[6] "Mollissimum genus earum *Penicilli*, oculorum tumores levant ex mulso impositi : iidem abstergendæ lippitudini utilissimi : eosque tenuissimos et mollissimos esse oportet."—*Naturalis Historiæ.* Liber xxxii., cap. xi., p. 289. In regard to the locality from which these sponges were procured, Pliny afterwards adds,—"Trogus autor est, circa Lyciam *penicillos* mollissimos nasci in alto, unde ablatæ sint spongiæ."—*Ibid.* P. 290.

I would venture to suggest, that it appears much more probable that the collyrium, ointment, or lotion, that was to be used with the sponge or charpie, was sometimes designated *Penicillum*, from the special mode in which it was to be applied; in the same way as we have found various eye-drops passing under the general designation of *Stactum*, from the special mode in which they were applied to the diseased organ. In this way the *Lene Penicillum*, in the legend of our present oculist-stamp, would not signify the material which was used in the application of the medicine, but the name of the medicine as indicative of the mode in which it was to be used. The employment of the *Penicillum* mixed with an egg (ex ovo) is often indicated upon the oculist-stamps; and in the ancient Roman authors it is a mode in which many of the collyria were directed to be prepared before they were applied to the diseased eye.

No. VII. In his paper in the "Archæologia," vol. ix., Mr Gough published a sketch and account of a medicine stamp, inscribed on three of its sides, and remarkable in one or two respects. The sketch which he has given of it is copied into Plate II., No. VII. After quoting the three inscriptions on its sides, Mr Gough gives the following very brief and unsatisfactory account of the reading of the stamp. "From the inscriptions," he observes, "we learn that the owner's name was F. L., or FLAVIUS SECUNDUS, and that his composition was made of Opobalsamum and Myrrh, and the white of eggs."[1]

Mr Gough pointed out that the third side of the stamp was engraved in letters of a rude and negligent form, and different in character from the inscriptions on the two other sides. But he failed in seeing, or in pointing out, that the remaining sides are both imperfect; the latter half of one of the inscriptions, and the first half of the other, being deficient in consequence of the stone having been broken and reduced in size. When this circumstance is attended to, the inscriptions on the three sides stand as follows:—

1. LJULIVENISD . . . . . .
   OPOBALSAMTU . . . . . .

2. . . . . ASMVRNESBIS
   . . . . . MPETUEXOVO

3. FSEKUNDI
   ATALBAS.

The name of the proprietor is evidently L. JUL. IVENIS; and I may remark in passing that the cognomen of IVENIS is one which has been found recurring among the Roman pottery-stamps found in England.

It is impossible to fill in, with anything like precision and certainty, the defective words in the two first inscriptions. But judging from the analogy of other similar and more perfect stamps, these two inscriptions probably read somewhat as follows when the seal was entire.

[1] Archæologia, vol. ix., p. 240.

1. LUCII JULII IVENIS DIAPSORICUM OPOBALSAMATUM ad Claritatem.—*The Opobalsamic Diapsoricum of L. Jul. Ivenis, for clearing of the sight.*

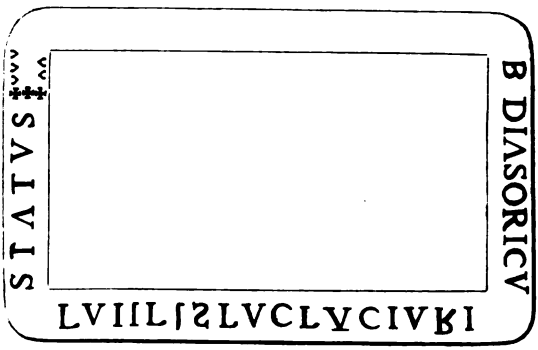
The adjective, *opobalsamatum*, has hitherto been generally found united upon medicine stamps with one of two collyria, viz., with Stacticum (as in seal No. IV.); or with Diapsoricum, as in seals found at Jena and Lyons. The D preserved in the first line is, in all probability, the initial letter of the latter collyrium.

The *Psoricum* was a mixture of cadmia and chalcitis, according to Dioscorides, Celsus, and Pliny; or of litharge and chalcitis, according to Galen, Aetius, and Paulus Aegineta. This metallic compound derived its name of *Psoricum* from its supposed utility in the treatment of parts affected with the eruption of scabies or *psora*. The eyelids, according to the ancient oculists, were the occasional seat of eruptive or pruriginous inflammation (*psorophthalmia, scabrities, prurigo*, &c.) In enumerating the diseases of the lining membrane of the palpebrae, Galen mentions, among others, *sycosis chalazosis*, and *psoriasis*.[1] Various collyria employed for the removal of these affections were termed *Psorica*, and most of them, though not all, contained the metallic compound alluded to. "Quae scabras in palpebris affectus persanant, ob id *Psorica* appellantur."[2] When speaking of the specific affections of the eyes and their appropriate local applications, Actuarius, in the same way, remarks, "Quæ scabiosis palpebrarum medentur, id circo *Psorica* appellantur."[3] He gives (p. 307) formulæ for various forms of the *Collyrium Psoricum;* as the *Psoricum aridum*, the *Psoricum* Aelii, &c. Aetius recommends the collyrium *Psoricum* against "scabros ac corrosos angulos, et intensos pruritus, milphoses et prurigines."[4] Scribonius Largus describes the composition of a collyrium *Psoricum* made from the metallic compound of the same name (facit hoc collyrium bene quod *psoricum* dicitur), and fitted to remove blindness, granulations, and xerophthalmia.[5] Marcellus Empiricus credulously invests the collyrium *Psoricum* with signal powers for various eye diseases, but particularly for old-standing blindness (antiquam coecitatem). For if (says he) we may credit the author of the remedy, it has, at the end of twenty days, restored sight to a person who had been blind for twelve years (nam ut auctori ejus remedii de experimento, duodecim annorum coeco intra dies viginti visum restituisse se dicit).[6]

On the Jena medicine-stamp the *Diapsoricum opobalsamatum* is entered as efficacious for the clearing of the sight (ad claritatem); and in the proposed restoration of the reading of the present English stamp, I have added to it the same therapeutic indication, as

[1] Kuehn's Edit., vol. xiv., p. 767.
[2] Galen in Kuehn's Edit., vol. xii., p. 717.
[3] De Methodo Medendi (Lib. vi.), p. 305, in Princ. Art Medicæ.
[4] Tetrabiblos, pp. 434, 435. [5] De Compositione Medicamentorum, p. 199.
[6] De Medicamentis Liber, p. 274 and 275.

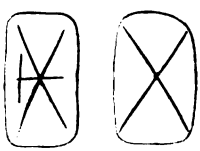
Nº VIII



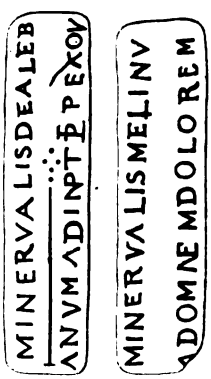
Nº IX

Nº X

Nº XI



Nº XII



one not unlikely to have originally filled up the part that is now deficient in this line of the stamp.

2. Lucii Iulii Ivenis Diasmyrnes bis Lippitudinis (?) impetu ex ovo.—*The myrrh collyrium of L. J. Ivenis, to be used twice a-day, mixed with an egg, at the commencement of Ophthalmy.*

Already we have considered the composition, &c., of the Collyrium Diasmyrnes (see No. VII.) It is entered as efficacious in attacks of Lippitudo on the medicine-stamps of Jena, Nais, &c. In the Jena stamp it is, as in the present instance, ordered to be used mixed with an egg.[1] The word *bis* denotes, in all probability the frequency with which it was to be used daily. Occasionally the ancient authors state in the same way in their works the frequency with which a special collyrium was to be used. Thus, for example, Paulus Aegineta, after describing the composition of the brown collyrium (collyrium fuscum), adds that it is to be applied thrice a-day (illinitur *ter* in die, . . ex ovo aut lacte, &c.)[2]

3. The third side is engraved, as already observed, by a different and far more inexperienced hand than the other two sides. The letters are very roughly and rudely formed. The inscription indicates the name of another oculist,—of one who probably became the possessor of the stamp after Ivenis. The new proprietor's name is F., or probably Fl., *Secundus;* and the inscription reads, F. Sekundi at Albas, *the collyrium* or *preparation* of *F. Secundus against Albugines.* In reading it, I suppose the at to be a misspelling for ad,—a mistake of which there are not wanting other examples in the illiterate and careless engravings sometimes found upon these medicine-stamps.[3] And I have interpreted the albas as signifying *albas cicatrices,* white cicatrices, or, in other words, *albugines* of the cornea,—a suggestion for which I am indebted to M. Sichel. Already I have quoted the expression of Aetius to the effect that all cicatrices of the cornea are "*Albæ;*" and the nouns by which such an eye-cicatrix is sometimes designated, both by the Greek and Roman physicians, namely λευκωμα and *albugo,* are derived from, and intended to signify, the white colour of these lesions.

No. VIII. An anonymous correspondent, C. D., sent to the "Gentleman's Magazine" in 1772, a sketch and notice of what, no doubt, is a Roman medicine-stamp, but both the sketch given of it and the description are excessively meagre. The correspondent dates his letter from Southwell, in Nottinghamshire. He says, "The in-

---

[1] "Diasmyrnes post impetum lippitudinis ex ovo."—Tochon, p. 66.

[2] Cornarius' Edit.; or Dr Adam's Edit., vol. iii., p. 550.

[3] For example, in the stamp found at Maestricht, and described by Saxe, there occurs the inscription *C. Lucci Alexandri Crocodes* at *aspritudines,* instead of ad *aspritudines.* See Tochon's Dissertation, p. 68.

scribed stone was found lately by casting up the ground, in the neighbourhood of Littleborough, in this county. The stone is oblong, about two inches long, and one broad. It contains inscriptions on the edges or rim of the two ends, and on one of its oblong sides, but not on the other."

"It is," says the correspondent, "supposed to be a Tessera or kind of tally, such being, as we are told, a little flat square piece of stone, and having a particular inscription, and was used in the Roman armies, by being on certain occasions delivered to each of the soldiers, to distinguish them from the enemy, and also in setting their nightly guard, by being given from one centurion to another, quite through the army, till it returned to the tribune who first delivered it. Upon the receipt of this, the guard was set immediately." "But," he continues, "as the inscription on the above drawing cannot be made out to satisfaction, many of you will be glad to know whether it has been such a Tessera as is above supposed; or what else it may have been, or also an explanation of its legend, by some of your antiquarian correspondents."

The inscription on one of the long sides of the stone, appears to be the name of the proprietor of the stamp; but the published copy of it presents such irregular lettering, as to defy any certain deciphering of what the name is. (See Plate III., No. VIII.) On the other two sides the inscriptions are as follows:—

1. B DIΛSORICV.
2. STΛTVS.

These two words evidently are mis-spellings, either on the original stamp, or (what is equally probable) on its published copy, for the Collyria termed *Diapsoricum* and *Stactum*. But I have already, in reference to previous inscriptions, discussed the signification of the two terms at such length as not to require to revert to them. (See under Stamps No. V., and No. IV.)

No. IX. This seal is remarkable both from its inscription, and from its round form. In this last respect it is, I believe, as yet unique,—no other specimen of a medicine-stamp of the same circular figure having, as far as I know, been hitherto described. The stone is about seven-eighths of an inch in diameter, and a quarter of an inch thick. Its form and inscription are seen in Plate III., No. IX., where the upper figure shows the stamp presenting the usual incuse and reversed inscription; and the second or lower figure shows the impression left by the stamp upon wax.[1]

This curious medicine-stamp was found, in 1808, by a person ploughing in a field near the Roman wall at Wroxeter (the ancient Uriconium), Shropshire. It was first figured and very briefly

[1] These figures of the Wroxeter stamp are copies of those originally published of it by Mr Parkes in the "Gentleman's Magazine" for 1810, vol. LXXX., p. 617.

noticed by Mr Parkes in the "Gentleman's Magazine" for 1810, p. 617. "Several (observes Mr Parkes) have attempted to decipher the legend, but no one has as yet been able to give a satisfactory reading." Mr Nightingale (1813) in his account of Shropshire in the "Beauties of England and Wales"[1] mentions the stamp; and Mr Hartshorne in his "Salopia Antiqua"[2] (1841) has given an embellished and consequently less accurate copy of the inscription than that originally published by Mr Parkes. Mr Hartshorne describes it as "an amuletal seal," and adds, "it has hitherto baffled the endeavours of those who have attempted to explain it." Lastly Mr Albert Way has lately correctly published it as a specimen of a Roman medicine-stamp, and has interpreted the second and fifth lines, leaving the others still undetermined. But the whole appears capable of being deciphered. The inscription runs thus:—

IBCLM
DIA LBA
AD OM
NE Δ VN
O EXO

DIALIBAnum J (*ulii?*) B (*assi?*) CLEMentis AD OMNEM Διαθεσιν (Diathesim) VNO EX OVO.—*The Dialibanum or Incense collyrium of Julius Bassus Clemens, for every eye-disease; to be used mixed with an egg.*

The name of the practitioner or proprietor, given in the first line of the seal, offers the principal difficulty in reading the inscription. But the CLM is in all probability a contraction,—as I have ventured to interpret it for CLEMENS,—a common cognomen or family name among the Romans. The B as an initial could stand for many of the gens names which begin with this letter, Balbus, Betutius, &c. I have conjecturally given it as Bassus, merely because on an old monumental tablet, discovered at Leyden,[3] the cognomen of CLEMENS is preceded by the nomen gentilicium of BASSUS,—showing the combination in question not to have been unknown among the Roman colonists formerly scattered over Western Europe.

The nature and composition of the Collyrium *Dialibanum* we have already had occasion to consider under a former head. (See Stamp No. VI.)

I have also formerly shown that the Greek term Διαθεσις was used as a general term for eye-disease (see No. I.); and no doubt its initial letter Δ stands in the present inscription under this signification.

[1] Beauties of England and Wales (1813), vol. xiii., p. 190.
[2] Salopia Antiqua, p. 126.
[3] See a copy of it in Gruter's Inscriptiones Antiquæ Orbis Romanæ. Tom. xii. p. 896., No. iii.

No. X.—In the Journal of the British Archæological Association for 1849, Mr Roach Smith has described a medicine-stamp found at Kenchester, in Herefordshire, and communicated to him by Mr Johnson. I myself am indebted to the kindness of Mr Johnson for a model of this medicine-stamp, which is quadrilateral, and engraved on its four sides. It has, besides, the word SENIOR inscribed on one of its flat surfaces; and the three first letters of the same word are repeated on the opposite surface. I shall afterwards have occasion to re-advert to this curious point.

The inscriptions on the four sides of this stamp are as follows:—

1. F. VINDAC. ARIO
   VISTIANICET

2. FVINDACIAR
   OVISTNARD

3. VINDAC. ARI
   OVISTI CHLORON

4. T. VINDAC . ARIO
   VISTI . . . . . N

The name of the oculist or proprietor T. VINDAC ARIOVISTUS, *Titus* (?) *Vindacius* or *Vindex Ariovistus*, is singular; the name *Ariovistus* being the same as that of the celebrated German general that plays so interesting and important a part in the Commentaries of Cæsar.

On this stamp there are no names of any specific eye diseases given; but the three sides contain the designation of three collyria that we have not yet met with on any of the previous medicine-stamps which we have had occasion to describe.

1. F. VINDAC*ii* ARIOVISTI ANICETUM.—*The Anicetum or infallible Collyrium of F. Vindacius Ariovistus.*

The collyrium *Anicetum*, or Ανικητον, is, as far as I know, described by Oribasius alone. It was composed of red copper, combined with henbane, hemlock, spikenard, frankincense, &c. Oribasius enters it as a collyrium "ad carbunculos aptum."[1]

Mr Roach Smith supposes that the *Anicetum* derives its name from being a preparation containing aniseed. But the formula given by Oribasius does not present this ingredient; and the origin of the term is very different. Galen presents us with a clue to its true meaning, when discussing the subject of plasters, in the sixth book of his work, De Compositione Medicamentorum. One, bearing the name of *Anicetum*, is (he observes) called so in consequence of its many and wonderful effects (vocatum est insuperabilis (Ανικητον)

[1] Synopsis, Liber iii., p. 51.

propter miranda et multa ipsius opera).[1] The term itself is no doubt derived from the Greek verb νικάω, "I conquer," with the prefix of the privative *a*. Among his own list of collyria, Galen enters the one known in his time under the name of "Collyrium Aster,"[2] as "*inexsuperabilis* Ανικητος."

2. F. VINDAC : ARIOVISTI NARDINUM. *The Nardinum* or *Spikenard Collyrium of F. Vindacius Ariovistus.*

The ancient authorities in Materia Medica describe several kinds of spikenard, according to the localities in which it is procured, as the Indian, Syrian, Celtic, &c. It was used by the Romans in many of their ointments and perfumes, and sometimes added to their wines. Some kinds of it were very costly. Horace (Carm. lib. iv. xii.) alludes to its high price—Nardi parvus onyx eliciet cadum.

It was used principally to perfume various medicines, &c. But high medicinal properties were also attributed to it in various diseases, and amongst others in diseases of the eye.—See Dioscorides, lib. i., cap. vi. It entered into the composition of many of the ancient collyria, and several were named from it in consequence of its forming a leading ingredient. Aetius has a long chapter on formulæ for "Collyria Nardina et Theodotia,"[3] and bestows the most extravagant praises upon some varieties of the spikenard eye applications. Speaking of one of them, he observes, "It is not easy to relate the powers and efficiency of this medicine; indeed my readers would scarcely credit it, for under the most desperate affections it recalls the eye to its natural state."[4]

3. F. VINDAC: ARIOVISTI CHLORON.—*The Chloron or green Collyrium of F. Vindax Ariovistus.*

Already I have had occasion to allude to the collyrium *Chloron*, as one of those which derive their particular appellation from the tint or colour of the preparation. The green collyrium, or *Chloron*, is mentioned in many of the old treatises upon affections of the eye. For example, Galen gives three such collyria in succession. See vol xii., p. 763, viz., the "*Chloron* ad diatheses;" and again, two forms of *Chloron* used by Zoilus the oculist.

The name of the collyrium on the fourth side of the Kenchester stone is so destroyed, that only the terminal letter N remains; and judging, from the model which I owe to the kindness of Mr Johnson, even this letter is doubtful.

No. XI. In the beautiful work on the Roman remains of Cirencester, published last year by Professor Buckman and Mr New-

[1] Kuehn's Edit. of Galen, Vol. xiii., p. 877 and 879.
[2] Ibid., Vol. xii., p. 761.
[3] Tetrabiblos, Sermo III., cap. cxiii., p. 436–8.
[4] Hujus auxilii actiones ac efficaciam dicere non est facile. Audientes enim vix crediderint. Nam desperatas affectiones ad naturalem statum revocat.—Ibid., p. 438.

march, a Roman medicine-stamp is described.[1] In 1818, it was found in the Leauses garden at Cirencester, deposited in a fictile urn.

This stamp is of the form of a parallelogram, and is inscribed on two of its sides. Plate III., No. IX., shows the lettering of these two inscriptions, as well as the size of the sides and two ends of the stone. The inscriptions are as follows:—

1. MINERVALIS DIALEB
ANUM AD IMPT LIPP EX OVO

2. MINERVALIS MELINU
AD OMNEM DOLOREM

Messrs Buckman and Newmarch read MINERVALIS as signifying "pertaining to Minerva;" but it is no doubt the name, as in other specimens, of the oculist who was the proprietor of the stamp. And from the inscriptions left us upon Roman tombs, we know that Minervalis was a Roman cognomen.[2] The two inscriptions are easily read; they are as follows:—

1. MINERVALIS DIALEBANUM AD IMPETum LIPPitudinus EX OVO. —*The frankincense Collyrium of Minervalis, for attacks of Ophthalmy, to be used with an egg.*

We have already had occasion to discuss the nature of the Collyrium *Dialibanum*;—see stamp No. VI., and it is unnecessary to recur to it. On a previous occasion, also, the signification of the common expression, *ex ovo*, was adverted to.

2. MINERVALIS MELINUm AD OMNEM DOLOREM.—*The yellow Collyrium of Minervalis, for every pain or disease of the eye.*

More than once we have had occasion to allude to the Collyrium *Melinum*. (See stamps Nos. IV. and V.) The only singularity is, that we have here the Melinum offered as a panacea for every painful affection to which the eyes of the colonists and natives of Cirencester might be subject, at the time that Minervalis practised amongst them. One of the forms of the Collyrium Melinum given by Galen is professed by him to be efficacious "ad omnem oculorum hebetudinem."—(Tom. xii., p. 786.)

No. XII.—A Roman medicine-stamp has lately turned up in these islands, in a locality in which its presence could be little expected, viz., in the county of Tipperary, in Ireland. It has been described by Mr Albert Way in an interesting paper (published since the first part of these notices appeared).[3] Dr Dowsley, of Clonmel, who now possesses this stone, has kindly furnished me with a wax impression of its inscription, and with the following note relative to the locality in which it was discovered:—"It was found (he

[1] Illustrations of the Remains of Roman Art in Cirencester, the site of the Antient Corinium. P. 117.

[2] See, for example, Gruter's Inscriptiones Romanæ. Vol. ii., p. DCCLXXXIII. 2; CIL. 3; MXXVII. 4.

[3] Notice of a stamp used by a Roman oculist or empiric, discovered in Ireland. Archæological Journal, p. 354.

says) near the village of Golden, parish of Relig-Murry, in the county of Tipperary, in a field near the ruins of an old hospital, or at least what was supposed to be such; but it was built at so remote a period, that there is now no record of what the building was for, nor of the founder of it, and so little of the walls are at present standing, that even the style of architecture cannot be known. The seal was discovered by a labourer when digging. There was no pottery nor coin found; but near it was a human skeleton much decayed, the position of which in the ground was not noticed. The soil in this field is peculiarly rich and very deep; it is frequently carted away for manure; most likely it was an ancient burial ground. The village of Golden is about a mile from the old abbey of Athassel."

It is unnecessary to discuss here, how such a Roman relic reached this part of Ireland, and whether it was conveyed there or not when the Romans were colonising Britain; or, what is probable, at a later period. But I may merely remark, there can be no doubt, that Roman civilisation and Roman practices spread in the earlier centuries of the Christian era to parts beyond the precise line of Roman conquest. Other Roman relics have been found in Ireland, though Ireland was never subject to the Roman arms.

Pl. III., No. XII., shows the figure of this Irish medicine-stamp. It is engraved only on one side, and the inscription runs as follows:—

M IUVEN TUTIANI
DIAMYSUS AD VET CIC

M(*arci?*) JUVEN*tii* TUTIANI DIAMYSUS AD VET*eres* CIC*atrices*.—*The Diamysus of Marcus Juventius Tutianus, for old cicatrices.*

At the end of the first line there is a small cut in the inscription (see Plate), which, in all probability, is not a letter, but a mark or ornament intended to fill up that space.

In speaking of the Bath stone, I have already taken occasion to state, that this same inscription of *Diamysus ad veteres cicatrices*, has now been found on various Roman medicine-stamps discovered in different parts of France.

The collyrium DIAMYSOS derived its designation from containing as its principal ingredient the *Misy*, a metallic vitriolic preparation, used to a considerable extent as a stimulant and escharotic among the ancients; and it was retained even to a comparatively late period in the London pharmacopeia.[1] It appears to be still used medicinally in the East.[2]

The chemical nature, however, of *Misy*, has given rise to some considerable doubt and discussion. It was usually found, and generally described, along with two other cognate fossils, *Sori* and *Chalcitis*. And Galen, who enters into an elaborate description of them,

[1] See the Pharmacopeia Londinensis for 1662, p. 48.

[2] See Ainslie's Materia Indica, vol. i. p. 513, and Royle's Antiquities of Hindoo Medicine, p. 102.

visited the copper mines of Cyprus, with a view of determining the precise nature of these three mineral substances.[1]

Dr Adams,[2] who has examined this question with all his well-known great learning and care, believes that these three minerals were merely varieties of *chalcanthum* or copperas. In his opinion, the *Chalcitis* was probably a kind of pure sulphate of copper which had contracted an efflorescence from age; the *Sori* was sulphate of copper combined with zinc or other impurities; and the *Misy* was a combination of sulphate of copper with sulphate of iron, the predominance of the chalybeate salt giving to the fossil its peculiar colour. For the Misy, says Dioscorides, is "of a golden appearance, hard, shining like gold when broken, and glancing like stars."

In his notes on the *Misy*, Dioscorides speaks of the analogy of its caustic power with those of Chalcitis; but the only diseases that he referred to as having the *Misy* used in their treatment, are the diseases of the eye. And he does so in telling us that the Egyptian kind of *Misy* is quite inferior to the Cyprian in forming eye medicines (ocularia medicamenta).[3]

In speaking of its medical powers, Galen,[4] Oribasius,[5] and Paulus Aegineta,[6] describe the *Misy* as escharotic, and astringent. In giving his list of eye medicines, Galen places the *Misy*, *Sory*, &c., amongst those local applications which have a detergent effect.[7] Paulus Aegineta enters the *Misy* in his list of "detergents of foul ulcers" of the eye, (vol. iii., p. 548). Pliny, in describing the properties of Misy, states that "extenuat scabrities oculorum."[8] Celsus in his work repeatedly alludes to the *Misy* and its effects.[9] One of the collyria which he describes when treating of granular ophthalmia, contains the *Misy* (see page 294). And he adds, that with the exception of those affections which require mild applications, this special collyrium is adapted to every kind of disorder of the eye (adversus omne genus oculorum valetudinis idoneum est). Galen

[1] I shall quote Galen's own graphic account of his personal visit and observations:—"At the mine in Cyprus, in the mountains of the Soli, there was a great cave dug in the mountain, at the right side of which, that is to say, on our left hand as we entered, there was a passage into the mine, in which I saw certain specimens of the three substances stretched upon one another like zones, the lowest being that of *sori*, upon it *chalcitis*, and then that of *misy*. In process of time the chalcitis changes into misy by degrees, and the sori can change into chalcitis, but requires a much longer space of time. So that it is no wonder that these three substances should be possessed of homogeneous (similar) powers, as differing from one another in tenuity and density of their parts—the grossest being the sori, and the finest the misy, whereas chalcitis possesses an intermediate power. When burnt, they become more attenuant, but less styptic."—*Adams' Paulus Aegineta*, Note, p. 400. (Kuehn's Galen, vol. xii., p. 226).

[2] See his Edition of Paulus Aegineta. Notes in pp. 253, 400, and 402.

[3] Opera, lib. v., cap. 117, p. 370. [4] Kuehn's Edit. of Galen, vol. xii., p. 228.

[5] Opera, lib. xv., p. 515, and lib. xiv., p. 483.

[6] Dr Adams' Sydenham Society Edition, vol. iii., p. 253.

[7] Kuehn's Ed., v. xii., p. 701. [8] Hist. Nat. lib. xxxiv., c. xii., v. iii., p. 399.

[9] See Milligan's Edit., p. 194, Misy sanguinem suppremit; p. 197, rodit; p. 199, crustas inducit, &c.

(vol. xii. p. 736) and Paulus Aegineta (vol. iii. p. 556) both give formulæ for the collyrium—Panchrestos of Erasistratus, which contained *Misy* as its leading ingredient. "It has," says Paulus "wonderful efficacy in diseases of the eyes." The *Misy* (valedissimum medicamentum) enters into the composition of several of the collyria described by Actuarius.[1]

In a previous page I have already taken occasion to state that Marcellus Empiricus gives a formula for a collyrium under the name inscribed upon the stone of the collyrium DIAMYSOS; and he describes it as calculated "ad aspritudines oculorum tollendas et ad lachrymas substringendas."

The collyrium *Diamysos* of Marcellus Empiricus consists of Misy burnt till it becomes red, and then combined with spikenard, saffron, cadmia, calcined copper, opium, myrrh, Cyprian scales, and gum, with all which it was to be rubbed down in the best wine, shaken and filtered. But he gives also the alternative of adding to the *Diamysos* another ingredient, which was long an article in the materia medica, viz., vipers. For some (he observes) add to the collyrium *Diamysos* "a viper, dried and baked well in the sun, as if it were salted" (quidam adjiciunt huic collyrio viperam siccam et arefactam bene in sole tanquam si sit selita). He goes on, however, still further to explain that prayers and incantations must be used in making this addition to the Diamysos. For (he observes) if you thus wish to add the dried viper, you must first extract its bones, roll it up in linen, and then pour over it the wine of the collyrium,[2] previously charming the viper (sed prius eam praecantabis) as follows, lest it cause tears and produce harm, saying, "As thou dost not see, even so may thy juice, when tasted, hurt no one, but I pray that with the purpose for which thou hast been added, thou mayest further the cure (quomodo tu non vides, sic et tuus succus gustatus nulli noceat, sed ob rem propter quam adjecta es proficias bene curationi precor)."

The preceding list contains notices of all the Roman medicine-stamps that have, as far as I know, hitherto been discovered in the British Islands. The account of them has extended to such an unexpected length, that I must retain for a concluding communication all generalisations relative to the nature, use, &c., of these stamps.[3]

---

[1] See De Methodo Medendi, lib. vi. p. 305-308.

[2] Viper wine (*Vinum Viperinum*) and viper broth (*Jus Viperinum*), had long a place in the London Pharmacopœia, and still longer the vipers were retained in it as an ingredient in the celebrated but multifarious Theriaca Andromachi, which, with its discordant farago of seventy and odd ingredients, was only expelled about a hundred years ago from the British Pharmacopœias. (See Alston's Materia Medica, vol. ii. p. 517; Hill's Materia Medica, p. 829; Quincy's Dispensatory, p. 400; Mead's Essay on the Viper, 1745, &c.)

[3] In the "Gentleman's Magazine" for the year 1778, vol. 48, p. 510, it is mentioned that a Roman medicine-stamp had been found at St Albans, but I believe no kind of description of it has, as yet, been anywhere published.

Part Second.

REVIEWS.

Report of the General Board of Health on the Epidemic Cholera of 1848 *and* 1849. London. 8vo. Pp. 159. 1850.

Appendix (A) *to the Report of the General Board of Health on the Epidemic Cholera of* 1848 *and* 1849. Report by Dr SUTHERLAND. London. 8vo. Pp. 164. 1850.

Appendix (B) *to the Report of the General Board of Health on the Epidemic Cholera of* 1848 *and* 1849. Report by Mr GRAINGER. London. 8vo. Pp. 201. 1850.

Statistics of Cholera. By Assistant-Surgeon EDWARD BALFOUR, of the Madras Army. Madras. 8vo. Pp. 70. 1849.

The Cholera: What has it Taught Us? &c. By WILLIAM J. COX, M.R.C.S., &c. London. 8vo. Pp. 26. 1850.

Cholera and its Cures: An Historical Sketch. By J. STEVENSON BUSHNAN, M.D. London. 8vo. Pp. 169. 1850.

THE labours of the General Board of Health have been conducted with an energy of benevolent purpose, which would of itself command respect, even had their sole fruits been disappointment. The great object which they have striven to illustrate is, that all pestilences, whether plague, fever, or cholera, are the necessary consequences of infringements of what some modern philosophers style, the organic laws,—those stern conditions of existence which prescribe deadly penalties against filth, over-crowding, the respiring of an impure atmosphere, the use of bad or insufficient food, and of unwholesome water,—against intemperance and kindred vice,—and, in short, against all the evils inseparable from poverty. Although this doctrine, when first announced by the General Board of Health, in their famous "Report on Quarantine," was not tacitly subscribed to by the medical press; and although, even at the present day, it cannot, without considerable modification, be held as representing the views of any very large section of the profession, its promulgation, by such high authority, has directed inquiries into the social position of the poor of our large cities, and has strengthened the hands of sanitary reformers in preparing remedies for certain great public errors. For even those who refuse assent to the whole doctrines of the Board of Health, cannot deny that the condition of the poor of our cities seems to render them but too prone to become the victims of pestilence, when it has been introduced or breaks out among them; while all modern experience tends to prove, that districts in

which sanitary improvements, such as draining, cleaning, ventilation, and adequate water-supply are introduced, and in which the comforts of the population are increased, become more salubrious. But in these times, although the scavenger is abroad—and we wish all success to him and his labours—we doubt whether the most vigorous exercise of his functions will suffice to sweep epidemic disease out of the kingdom, and prevent its return. Filth is a very ancient institution, in clinging to which, as their indefeasible birthright, the lowest classes of society are most impracticably conservative. If a grand national purification is to be effected, we must first enlist their sympathies for soap, overcome their time-honoured hydrophobia, provide them with abundance of water, and see that they wash and scrub like thorough sanitary reformers, repeating the lustration as often as the polish wears off. This essential preliminary arranged, they must be provided with new and airy dwellings, wherein the most approved systems of ventilation, heating, lighting, draining, and cleaning must be carefully applied. Round these "happy lands" all nuisances must at once and for ever cease; there is no reason why the dead should be permitted to poison the living, therefore the churchyard must give place to the suburban cemetery; pigs must take lodgings in the country, knackers, tanners, tallow-chandlers, fiddle-string makers, and other objectionable neighbours, must choose less odorous vocations, remove along with the pigs, or find means of consuming their own stench. That the regenerated community may enjoy the full benefits of sanitary improvements, they must be well employed,—and well paid, in order that they may be well fed. In the midst of them must dwell a staff of inspectors, with the eye of the lynx and nose of the pointer, swift to detect, and prompt to denounce, offences against the "organic laws." Alas, what a work is before us!

In the "Report on Cholera," and in its "Appendices," much space is devoted to the development of the views of certain members of the Board, as to the influence of the sanitary errors above indicated in inducing the localisation of cholera. We have already stated, that these errors undoubtedly favour the spread of pestilence among our poor; and that the humanity and expediency of removing the more obvious of these predisposing causes are unquestionable; but the influence of nuisances, over-crowding, &c., upon public health, having been recognised from time immemorial, it is, we think, treated of with unnecessary prolixity in these "Reports."

The progress of the late epidemic is sketched as follows:—

"It thus appears that the first great epidemic outbreak, proceeding from Caboul and the north-western provinces of Hindostan, as from a centre, swept over Affghanistan, Persia, and the south-eastern portion of Asiatic Turkey, until it was arrested by the winter of 1846, in its progress towards Europe. It had, up to this date, become localised in the north-eastern parts of Asia Minor, from whence, in the spring of 1847, it again commenced its career, spreading in all directions; striking, on the one hand, the cities of Asia Minor, Persia, Arabia,

and Egypt; and, on the other, Georgia, Circassia, and the southern provinces of the Russian empire. The northern branch of this great outbreak continued its progress until nearly the whole of the governments of European Russia were affected; after which, one portion of it advanced into Finland and Sweden, where it apparently terminated its destructive course; while another branch, after sweeping round the north-eastern shores of the Black Sea, and nearly decimating the cities and towns of the lower Danube, advanced through Austria into Germany and Hanover; and, at the same time, attacked the capital of the Turkish empire. Some idea of the geographical extent of the pestilence may be formed by the circumstance, that it ravaged Constantinople, Berlin, St Petersburg, and Cairo in the same month. Hamburg was attacked on the 7th September; and within three weeks afterwards the epidemic reached the shores of Britain, where, as has been stated, it first appeared at Edinburgh in the beginning of October 1848."—Pp. 8, 9.

In regard to this neighbourhood we find it stated, that—

"In Edinburgh, in the beginning of October 1848, two cases occurred simultaneously; one in the underground flat of a house at the top of Leith Walk; and another in Leith, in a lodging-house behind King Street. Then two other cases occurred in the same room in the house in Leith Walk, in which the first took place. In a day or two it struck two other points in Edinburgh; and at the same time appeared in the fishing village of Newhaven, about two miles distant. Subsequently, it attacked, by isolated cases, a number of new and distant localities, and advanced very slowly before it became epidemic."—P. 15.

From the passage first quoted, it appears that Edinburgh had the unenviable notoriety of being the first spot in the United Kingdom in which the cholera of 1848 manifested itself. But, in a subsequent part of the "Report" (p 13), we find it more than hinted, that, as the earlier cases of the disease have been usually concealed, or their true nature unwillingly acknowledged, it is likely that isolated cases of cholera occurred in London as early as July 1848. We are well aware of the difficulty of obtaining correct information in such circumstances,—a difficulty which must be all but insuperable in localities where the population amounts to one or two millions, as in London. Even in Edinburgh and Leith, with the more moderate population of about two hundred thousand, the truth regarding the first reported and unequivocal cases of the disease was long unknown; and it appears that even the Board of Health is yet in ignorance regarding them. Systems of registration and coroners' inquests are alike unknown in Scotland; hence Dr Sutherland, the inspector employed in Edinburgh by the Board, was unable, in the course of his inquiries here, to avail himself of sources of information, which in the towns of England are deemed sufficiently authentic. Cholera had prevailed for many months in Edinburgh before the whole particulars relative to the following cases were ascertained by the personal inquiries of certain members of the cholera committee of the Colleges of Physicians and Surgeons, who, at that time, happened to be engaged in investigating a connection supposed to have existed between the earliest cases of the disease in Edinburgh and in Leith.

HISTORY OF EARLY CASES.—(*Abridged from Edinburgh Cholera Reports.*)

I. The first case of cholera reported is that of Mr S., spirit-dealer, in Canal Street, who was taken ill on the morning of the 27th August. The following particulars of the case were furnished by Dr M'Cowan, the medical attendant, in a letter to Dr Alison:—"I found him in great agony, from spasms in abdomen and extremities. I could compare the muscular spasms to nothing more similar than the action of the uterus after the expulsion of the placenta. His skin was of a leaden colour. Rice-water vomiting and purging had been present about an hour before I saw him (2 A.M.). The pulse was very feeble, and there was no secretion of urine. I immediately administered about ʒiij. of brandy, along with fully two teaspoonfuls of laudanum, and applied a kitchen towel, covered with mustard, around his loins and abdomen. He was to take a pill containing gr. j. of opium every half hour; after taking two he fell asleep; and I may say that he was cured within three hours, the subsequent weakness of course excepted."

This case, had it presented itself two months later, would undoubtedly have been regarded as one of Asiatic cholera. The rapidity of the attack, the rice-water vomiting and purging, the symptoms of collapse, and suppression of urine, all favour this view of the case. Still there are circumstances which render it not improbable, that the patient may have suffered only from British or sporadic cholera, induced, possibly, by some error in diet. The chief of these are, the rapidity with which full doses of opium induced sleep and convalescence; the fact, that there was no consecutive fever, and that no other case of cholera was reported till six weeks afterwards. It is not uncommon, in the course of ordinary British cholera, or of colic, to find the secretion of urine suspended for a time.

The next cases which presented themselves were seen by Dr Henderson, of Leith; and the following abridged account of them is drawn up from information supplied to the committee by that gentleman.

"September 7th, at 2 A.M.—II. A poor man, named Johnstone, æt. 54, residing in Elbe Court, Leith, on the ground flat, was, without premonitory symptoms, seized with vomiting, purging, spasms, and collapse, which proved fatal within eight hours. He was by trade a cooper, had for years been in good health, was irregular in his habits, and lived in a dirty damp house, in a filthy ill-ventilated locality. He had had no communication with the sick; and his case *is believed to have been the first* which was seen in Leith.

"September 8th, 4 A.M.—III. Mrs Cooper, æt. 50, residing in Elbe Street, Leith, on the ground floor, after a day or two of premonitory diarrhœa, was seized with vomiting, purging, cramps, and collapse, with consecutive fever, from which she ultimately recovered. She was a healthy sober person, was not known to have been in a cholera locality, or to have been in communication with the sick, and lived in a clean house in a tolerably clean street. It is stated that a common sewer passes her door, and that the drainage of the neighbourhood is far from perfect.

"September 11th.—IV. Mrs Forrest, æt. 35, residing in a house on the ground floor, in the Tontines, Leith, was, without premonitory symptoms, seized in the night time with the usual symptoms of cholera, and in three days recovered. She was a healthy woman, of tolerably regular habits, had not been in direct communication with the former cases, but lived very near them, in a damp house, situated in a populous, and not over-cleanly locality.

"September 13th.—V. Bryce, æt. 24, a stoker in the 'Martello' steamer, which had just returned from the infected port of Hamburg, and residing in Weir's Close, Kirkgate, Leith, had premonitory diarrhœa, followed by an attack of cholera, from which he recovered. He was a healthy sober man, and lived in a clean house, situated, however, in a very filthy locality.

"VI. and VII. occurred on the 20th and 28th September, and were both reported by Dr Henderson. One of these patients was a labourer, residing in Burns Street, Leith; the other lived in St Andrew Street, Leith."

Our object in introducing these particulars, is to illustrate the uncertainties with which those must contend who seek to trace the mode in which cholera is introduced into large communities. We have shown that at least *six* unequivocal cases manifested themselves in this neighbourhood before the period specified in the "Reports" of the Board and of Dr Sutherland; and that accounts of some of these cases were only obtained many months after their occurrence, and then with trouble and difficulty.[1] It is more than probable that other and still earlier cases remain unreported—cases which might either connect the infection of certain localities in Leith with the importation of disease from some of the continental ports, or leave the subject of the introduction of the pestilence mysterious as at present.

Enough has been said to prove that it is not in our large cities that evidence for or against the contagion theory can be best collected. Of this the Board seem to have been well aware, when the following remarkable passage was inserted in the "Report:"—

"During the epidemic of 1831-2, it is stated that numerous instances occurred in which an infected individual came into a healthy locality, and that the disease soon afterwards attacked other persons in the house or immediate neighbourhood in which the infected individual resided, and spread from thence as from a centre.

"No instance of this kind has been brought under our notice in the progress of the late epidemic. In all cases where the facts have been carefully observed on the first appearance of the disease in a new locality, which is the only period when the observation can be made, it has appeared to attack and spread epidemically, and not by the contact of the sick with the healthy."—P. 32.

The species of evidence here indicated, in the first paragraph, is indeed by far the most satisfactory which can be offered by the contagionists, and is obviously to be derived from inquiries in villages and other small localities, where the health and circumstances of each member of the population are known to all the rest. But in denying that any evidence of this kind was brought under their notice, in connection with the recent epidemic, the authors of the "Report" show an amount of wilful ignorance hardly less excusable than want of candour. Cases of the kind appeared in shoals in the pages of the English and Irish periodicals, and not a few were published in the 9th volume of this Journal. These were not, however, obtruded upon the notice of the Board, and would probably have been deemed all too unworthy the attention of so enlightened a body. Yet it was undoubtedly their duty, before presuming to report, to have sought for information so important and so easily accessible. There is a variety of obstinacy, too often mistaken for and miscalled *energy of character*, which induces a man to shut his

[1] It is right to mention, that the delay and trouble here alluded to, are by no means attributable to Dr Henderson or Dr M'Cowan, who, on the contrary, took great pains to contribute information, but to the impossibility of making arrangements for securing accurate returns of cases immediately on their occurrence.

ears and eyes against evidence fatal to a darling crotchet or rashly-expressed opinion. Although this unenviable mental qualification is not unfrequently the attribute of an individual, it happily is seldom found to localise itself in an entire body of well-educated men associated for scientific inquiry, or the public good.

In illustration of the nature of the evidence which the Board declines to notice, we append three from a large series of reports, communicated to the Edinburgh cholera committee. Abstracts of the first two have already appeared in the Monthly Journal (vol. ix., pp. 857 and 787); the third, it is believed, has not hitherto been published.

For the accuracy with which these reports are abridged from the original documents, and for the whole of the accompanying commentary, Dr Robertson, a member of the committee, is alone responsible.

CHOLERA AT GLASGOW—STRAITON—CARRINGTON.

I. Mr J. J. took cholera at his residence in Glasgow, on the 17th December 1848, and died on the 18th December. His father and mother, who resided in the farm-house of Straiton near Lasswade, went to Glasgow upon hearing of his illness, arrived shortly before his death, and returned to Straiton on the 19th. The funeral of Mr J. J. took place on the 20th December. The coffin, containing the body, was conveyed by railway from Glasgow to Edinburgh, and thence by hearse to Lasswade churchyard; it was at no time within a 100 yards of the house of Straiton. Some clothes were brought on the 19th December from the deceased's residence in Glasgow, to the house of Straiton by Mr and Mrs J. On the 22d December, Mr A. J., who was connected in business with his deceased brother, went from Straiton to Glasgow, and returned to Straiton on the 23d December. Mr and Mrs M., relatives of the deceased, lived in another flat of the same house in Glasgow, attended Mr J. J. while ill, and on the 26th December came to reside with the family at Straiton. Three children accompanied them, and they likewise brought from Glasgow a cloak (which had been washed), and other clothes belonging to the deceased. Thus on three separate occasions, on the 19th, 23d, and 26th December, direct communication is known to have taken place between Straiton and the infected locality in Glasgow, and on two occasions, the 19th and 26th, clothes were brought from the infected house.

"II. Mr T. J., a brother of the deceased, residing with his father at Straiton, took cholera on the 1st January 1849, and in a few days recovered.

"III. Mr A. J., the brother who had come from Glasgow on the 23d December, took cholera on the 5th January, and died.

"IV. Miss G. J., an unmarried sister, after a few days of premonitory diarrhœa, took cholera on the 5th January, and eventually recovered.

"V. Janet Inglis, a servant in the house of Straiton, had *premonitory symptoms* on the 7th January, and on the 8th *went to live with her father* at Carrington. Immediately on her arrival, she showed unequivocal symptoms of cholera, was nursed by her stepmother, and recovered.

"VI. The stepmother took ill in the same house at Carrington, on the 14th January, and died after 16 hours' illness.

"VII. Mrs Lyall, a neighbour who attended case VI. while ill, took cholera on the evening of the 14th, and died.

"VIII. Thomas Law, a neighbour who visited Mrs Lyall's house during her illness, took ill on the 18th January, and died.

"IX. Charles M'Innes, who lodged with Thomas Law, upon returning to his lodgings on the 18th, and seeing Law unwell, grew frightened, and fled the

house; but finding himself growing ill in his new habitation, he was compelled to return to his lodgings, and lay up. He eventually recovered.

"X. Mrs Law, wife of case VIII., took cholera on the 21st January, and recovered.

"XI. Robert Inglis, father of case V., took cholera on the 20th January, and recovered.

In regard to the four Straiton cases it must be remarked, that the patients were all in comfortable circumstances, and that their residence was a large farm-house, about 100 yards from the junction of the roads to Roslin and Lasswade, on the summit of the hill to the south of the village of Burdiehouse. Loan-head is about a mile from Straiton, and as is well known was severely visited by cholera; cases had, however, ceased to occur there for several weeks before the J—s. suffered from the disease. Mr Edwards, surgeon in Lasswade, who attended some of the Straiton cases, obliged the committee by furnishing the foregoing particulars, and the following special report upon the locality of Carrington:—"Carrington is situated in a healthy locality, and is a remarkably clean village. The dwellings are not crowded, every cottage having two apartments. The inhabitants are temperate. Inglis, Law, and M'Innes (Cases VIII., IX., and XI.), were all labourers, and were neither poor nor ill fed. No circumstance in their habits, nor in the locality of Carrington, was in my opinion likely to predispose them to cholera. Mrs Inglis (VI.), was not by any means a robust person; Mrs Lyall (VII.), had for a considerable time been subject to attacks of diarrhœa; and Thomas Law and his wife (VIII. and X.), were both aged and infirm. Three houses only were visited by cholera in Carrington. The houses of Inglis and Lyall were separated merely by an entry of a few feet. Thomas Law's house was about 200 yards distant."

No other cases occurred either at Straiton or Carrington up to the 1st April; no cause of insalubrity is known to exist in either locality; nothing to determine the localisation of an epidemic atmosphere. But we find a group of ten cases, in each of which we can trace previous communication with others labouring under the same disease; and except this communication, we know of no circumstance common to the whole *ten*, which was not also common to a large number of neighbours, who, however, totally escaped cholera. Here, then, probability points strongly to the efficacy of contagion. * * * * * * * It is sufficient for our present purpose to have shown, that cholera was introduced into Carrington by case V., and that *six* other cases occurred, within a few days, among those of the villagers who had been much in the houses of the sick, while not a single case occurred among the remainder of the population. The total number of inhabitants in Carrington is about 161, of whom, on a very liberal computation, we may suppose 30 to have had intercourse with the sick. Of these 30, 6 contracted the disease, *i. e.*, 20 per cent. suffered, while of the remaining 131 all escaped. It seems unreasonable to maintain, that a number of cases should have succeeded each other in so remarkable a manner, and that the succession should have been accidental. How improbable, if the efficacy of contagion be denied, that cholera should have made its appearance in the isolated and comfortable house at Straiton while the neighbourhood was healthy; that the first case should have *chanced* to occur within five days from the arrival of the M.s from an infected locality; that the girl Inglis, who went, while ill of cholera, to the healthy village of Carrington, should happen to arrive at the same house where the disease was about to localise itself; that her stepmother should nurse her, and be herself the next victim; that communication with the sick should in like manner be followed by attacks of cholera, in five other instances in the same locality; that all the inhabitants of the village, who had no intercourse with the sick, should escape the influence of the 'epidemic atmosphere;' and that out of a population of 161, *six* only should be predisposed to the reception of the choleraic poison, and should all *six* happen to have been, shortly before their seizure, in the company of the sick."

CASES AT KIRKLISTON.—IMPORTATION.

An Irish labourer arrived with his wife on the 14th February 1849 at Kirkliston. They had travelled from Ambleside, through Roxburghshire, and (although the woman denied it) there is no doubt that they slept in Kelso on the 13th Feb. A great number of cases were occurring about this time in Kelso.

Case. I. Mrs M'Goukan, the labourer's wife, took ill on the road from Kelso, and seems to have concealed her complaints for a few hours after her arrival at Kirkliston. She died on the 16th February. This is known to have been the first case in Kirkliston.

II. Ronald Gillies, a labourer, living in a "hut" next door but one to the M'Goukan's, had occasion to pass their door frequently during the woman's illness, and must have been in communication with her, at least *indirectly*. He was seized with cholera on the 16th February, and died next day.

III. John M'Goukan attended his wife while ill, and was at her funeral on the 17th Feb. In the evening he presented himself at the cholera hospital in Surgeon Square, Edinburgh, and after a smart attack of the disease recovered.

IV. and V. Mr B., the surgeon in Kirkliston, who attended I. and II., felt unwell on the 17th February, and passed some time in company with his sister, just after leaving the bedside of Ronald Gillies. Miss B. died of cholera on the night of the 18th, her brother became seriously ill on the 19th, and died on the 20th February.

VI. Mrs Grindlay, who nursed IV. and V., and who lived in a distant quarter of the village, became ill on the 21st, and died.

VII. Mary Kerr, living next door to I. and III., stated that Mr B., while attending case I., came into her hut to rest, and that she was aware of a *peculiar heavy smell* about his person, which nauseated her. After a few days of premonitory diarrhœa, she had on the 22d an attack of cholera, from which she recovered.

VIII. John Gillies, who lived with his brother (II.), had shortly afterwards premonitory diarrhœa, followed by a decided attack of cholera.

"During the month of February," it is stated by Dr Fleming, "there were few or no bowel complaints in Kirkliston. Diarrhœa had prevailed there epidemically in the months of October and November 1848."

IX. Agnes Martin, who was employed in attending cholera patients in Kirkliston, took diarrhœa there on the 2d March. She returned to Edinburgh on the evening of 3d March, and was admitted into the cholera hospital on account of vomiting, purging, and spasms. She was bled to ʒviij., and had a mustard emetic, followed by calomel and opium. Recovered.

The above account of the Kirkliston cases is drawn up chiefly from information furnished by Dr A. Fleming, the medical attendant. That the disease was imported by case I. is tolerably clear; but where the patient had herself contracted the infection is not positively ascertained. She denied that she had slept in Kelso on the 13th, but allowed that she had been at Roxburgh, about three miles to the west of Kelso. There might, however, have been a motive for concealment, as she must have well known that she would not meet with a very cordial welcome at Kirkliston, if it were ascertained that she had slept a day before in an infected locality. The statement of the husband, made in the cholera hospital, is we think more to be depended upon, both because he could have no object in departing from the truth, and because the ordinary routine questions were put to him by a gentleman who, at the time, knew nothing of the Kirkliston cases. In his answers, he distinctly allowed that he had slept with his wife in Kelso on the 13th. Of their subsequent proceedings his account was rather confused; but it is all but certain that they must have made their way by Roxburgh and St Boswells to Melrose or Galashiels, and thence travelled by railway into Edinburgh, where it is certain that they were for some time on the 14th. The wife was, according to the husband, unwell on the journey, but seems to have concealed her indisposition upon arriving at Kirkliston. Whether the woman became infected at Kelso, at Rox-

burgh, at Melrose, or at Edinburgh, the main facts of the case remain unquestionable, viz., that she was ill before she reached the village of Kirkliston, that she was the first cholera patient in that locality, and that her illness was, within fourteen days, followed by seven other cases of cholera among the attendants and neighbours.

CHOLERA AT FINDHORN.—(*From information furnished by Dr Brand of Forres.*)

On Saturday the 7th July 1849, the schooner Swift arrived at Findhorn from London, having on board a Mrs B. of Cromarty. She had been sick and unwell during the passage; came on shore and went into lodgings on the 7th, and on the same night and next day complained of vomiting and purging. On Monday the 9th she went out and sat down on a seat at the end of the house of a fisherman, named John Masson. This man was at the time seated on the same bench, mending his nets. They remained for some time in conversation, and Mrs B. becoming ill, is said to have been assisted to her lodgings by the fisherman. On the night of the 9th, *John Masson* went to the salmon fishing at the mouth of the Findhorn, and was taken ill in the boat about midnight with severe vomiting and purging. On the 10th, at 3 A.M., he was brought home, and about 6 A.M. was visited by Dr Brand of Forres, who found him in a state of collapse, which proved fatal at 12 o'clock of the same day. Dr Brand could not pronounce this a decided case of Asiatic cholera, both because the man had some years before suffered from dysentery, and because he had not an opportunity of examining the evacuations. In his report to the committee he mentions that he considered the case suspicious, from the suddenness and severity of the attack, the violent vomiting and purging which at first were present, the sinking of the features, the blueness of the hands and nails, the violence of the cramps, the "vox cholerica," the rapidity of the symptoms, the coldness of the body, and absence of pulse. *Mrs B.*, already alluded to, after going home on the 9th, became much worse, and died on the morning of the 10th. *Widow Matheson*, who lived in the same house with Mrs B., and had been attending her, went to work in the fields about two miles from Findhorn, was taken ill on the 10th about 3 P.M.; endeavouring to return home she fell down on the road, was carried to her house, and died next morning. She was not seen while ill by any medical man; but Dr Brand reports "that he saw the body at 3 P.M., that the features were shrunk, and the hands and nails blue." He adds that the poor woman was a temperate and hard-working person, of about sixty years of age. *Mrs Masson*, wife of John Masson, was taken ill on the night of the 11th, was in the collapsed state at 7 A.M. of the 12th, and died at half-past 5 P.M. *Widow Main*, a near relative of John Masson, had visited him (while ill?), and on the 11th was seized with vomiting and purging. She ultimately recovered. *Mrs Main*, sister-in-law of John Masson, visited him while on his death-bed. On the 12th she was attacked with violent pains in the stomach and bowels, and had three copious alvine evacuations. Dr Brand, who saw her, administered laudanum and brandy, and the patient recovered. A travelling tinker, named Lindsay, of intemperate habits, had passed the day of the 11th July in Findhorn, and slept that night in a shed near Matheson's house. On the morning of the 12th he and his wife both went into Matheson's house, where the body of widow Matheson was lying, to light his pipe. They then left Findhorn, fording the river about a mile below Forres, and with their two children took up their abode among a parcel of whins. The tinker soon became affected with vomiting, purging, and cramps, and died about 12 P.M. of the 12th. *Mrs Matheson's girl* had been engaged on the 11th in washing her mother's bed-clothes. She was seized on the evening of the same day with vomiting, purging, and cramps, but after the operation of an emetic, followed by a dose of laudanum and ol carui, seemed restored to health. On the 16th she assisted another person to whitewash and clean the house; and on the 17th, about 9 A.M., was suddenly attacked with such profuse vomiting and purging that she fell down on the floor and required to be lifted into bed. She was actively treated by Dr Grigor, R.N., and recovered. *Mrs Lindsay*, wife of the tinker,

was taken ill on the morning of the 14th, with vomiting, purging, clammy perspirations, and reduction of temperature of surface, but no cramps. She recovered after taking a mixture of sp. vini, ol carui, and syrup, together with a drachm of laudanum. *Bell Kinnaird*, sister of Mrs John Masson, who died on the 12th, was in constant attendance on her sister and brother-in-law during their illness. She was seized with the rice-water vomiting and purging on the morning of the 15th, followed by cramps and collapse, and notwithstanding the most judicious treatment by Dr Brand, Dr Innes, and Mr Dempster, she died at noon of the 16th. *Jane Smith*, daughter of a carpenter in Findhorn, had been in communication with the Mathesons, and attributed her illness to having had a tubful of water poured about her feet, in which the bed-clothes of Mrs B. (deceased) had been washed. This girl took cholera on the 17th and died on the 20th. *Mrs Anderson*, aged 56, who keeps a public-house on the road-side, in the hamlet of Kinloss, about two miles from Findhorn, began, on the 18th, to suffer from ineffectual calls to go to stool. On the 20th, she had unequivocal symptoms of cholera; and on the 21st (the date of Dr Brand's report), was still in a critical state. Mrs Anderson had no direct communication with the sick in Findhorn, but several people who had been attending them, went into her house after the funerals, took drams, and conversed with her about the sufferings of their friends. *James Smith*, father of Jane Smith, showed symptoms of cholera on the morning of the 21st, and at half-past 10 was in the collapsed condition. *James Smith*, his child, aged 18 months, had been suffering from choleroid (?) vomiting and purging for two days previous to the 21st.

We are not aware that any case of cholera had occurred within a hundred miles of Findhorn, for three months prior to its outbreak there, and are positively assured by Dr Brand, that "before the arrival of the Swift from London, Findhorn may be said to have been more than usually healthy; the few cases of typhus fever, of a mild nature, which had lingered during the winter and spring, had completely disappeared. There were no bowel-complaints, nor any disease, except a few cases of hooping-cough among the children. The population of Findhorn is from 800 to 1000. The town is situated on a sort of peninsula, formed of dry sand and gravel, about a mile from the mouth of the river Findhorn, which at full tide forms a bay or lake, extending from Findhorn to Kinloss, south—a distance of two miles—from Kinloss to Kincorth, west—a distance of three miles—whilst to the point, opposite Findhorn, at full tide, the distance is only about a quarter of a mile. The inhabitants are generally employed at the fisheries, and are rather a respectable, moral, industrious, comfortable, and cleanly people; a few of them are addicted to drink, but in general they are sober. Since 1818 the town may be said to have been remarkably healthy, but in that year typhus fever of a most malignant nature was introduced into it by vessels from Sunderland and Ireland. The fevers from these places were completely different from each other, and retained their distinctive marks during the prevalence of the disease. Nearly two-thirds of the inhabitants were at that time infected, and the disease was only ultimately subdued by causing the diseased secretions or evacuations from the stomach and bowels of the sick, to be instantly *buried*. For as the houses in Findhorn have, for the most part, a pit or dunghill near their doors, where the garbage of the fish, and 'fulzie' of the family are thrown out, it may readily be supposed that if any infectious disease gets in among them, the diseased excretions thrown out into the dunghill will contaminate the air, and render the disease more virulent, and its spread more rapid than usual. In the course of a forty years' practice, I have witnessed many fatal instances of diseases being propagated in this way."

The fourteen cases reported by Dr Brand, include all, whether of cholera or choleroid disease, which had occurred in Findhorn up to the 21st July. There is strong reason for believing that the seeds of the disease were brought in the schooner Swift, and probably in the person of Mrs B. The vessel sailed from an infected port, and Mrs B. was known to have been ill before landing. Unfor-

tunately one link in the chain of evidence is wanting, viz., the precise nature of her complaints while at sea. But even if we admit that Mrs B. contracted cholera on shore at Findhorn, how curious is the succession of cases which follows. Can it be seriously maintained that the appearance of the disease, in other thirteen instances, was totally independent of the communication which they are all known to have had with the sick? Why should the disease, in selecting victims in an infected house, pitch more particularly upon individuals who attended the sick, who cleaned the house, who washed the clothes? Why should it move from house to house in the train of those who had visited the infected localities? and why should it spread from such individuals to their families? But why, above all, if the pestilence were generated under the combined agency of "epidemic atmosphere" and foul dunghills, do we find but four houses invaded out of at least a hundred similarly circumstanced? Knowing something of the character and prejudices of the north country fishermen, we are satisfied that the first alarm of cholera in Findhorn must have been followed by considerable circumspection on the part of the inhabitants, in avoiding communication with infected houses. There is, therefore, abundant reason for believing that the exciting cause of the first fourteen cases in Findhorn, was contagion, or the communication of a morbific influence from the sick to the sound; and it is at least probable that the immunity enjoyed by the rest of the population was connected with their avoidance of the sources of contagion. If we apply a calculation, similar to that already used in remarking on the cases of cholera at Carrington, we shall find that, in Findhorn, those who attended or visited the sick suffered (within a week) in the ratio of 14 per 100, while the ratio of attacks among the rest of the population was *nil*.

But if, from this admittedly theoretical part of the subject, we pass to matters of a more practical nature, we shall find, in the reports of the Board, and of their inspectors, Dr Sutherland and Mr Grainger, many valuable inferences drawn from recent experience, with which we cordially agree, and which, in the prospect of future cholera epidemics, should not be forgotten. And first we are struck with the assertion, which our own experience amply confirms, that when the stage of collapse is fully formed, treatment is, in a large proportion of cases, unavailing; or, at least, that no one of the many forms of treatment hitherto adopted at this period of the disease has succeeded in saving a fair proportion of lives. And let it not be esteemed an *opprobrium medicinæ* that such should be the case. When gallons of fluid have, in a few hours, passed from the system, and the density of the blood has been suddenly raised from 1052 to 1070 and upwards, when the agonies of sickness and cramps have been endured, till the vital spark is all but extinguished, what human measures can we depend upon to revive the powers of life, or avert the fatal accidents of the reaction stage? Let the frightful mortality of cholera hospitals, where every rational expedient has been successively and carefully tried, and found wanting, be the answer to the question.

But, happily, in the disease as it has prevailed in this country, there is usually—nay, in almost every case—a premonitory stage of diarrhœa; and it is to the detection and treatment of this stage that the labours of medical men, during cholera epidemics, can be most profitably applied. There can be no question of the fact, that an individual, residing in a cholera locality, who becomes affected

with diarrhœa, is in extreme danger; and more especially if the purging assume the rice-water character, it is probable that symptoms of confirmed cholera will speedily supervene. That at this time the power of medicine can be interposed with every reasonable prospect of success, is perfectly ascertained, and cannot be better illustrated than by the statistics of the cholera in Glasgow and Barony parish, during 1848-9, given at pp. 80 and 150-8 of the interesting and laborious "Report" of Dr Sutherland. From these we learn, that of 13,089 premonitory cases, including 979 of rice-water purging, detected and subjected to treatment, only 27—*i. e.* about ·2 per 100, passed into cholera; while, during the same period, no less than 2234 cases of malignant cholera occurred, in which it is inferred, that the premonitory stage either did not exist, or was not treated at all.

An excellent measure, for the introduction of which the Board of Health deserves great credit, was the system of house-visitation which they directed in many localities, for the purpose of detecting those cases of premonitory diarrhœa which were so often neglected or concealed. But for the adoption of this humane and rational measure, it is most likely that of 6970 cases of premonitory diarrhœa *discovered in Glasgow alone by the visitors*, a very large proportion would have succumbed to the pestilence. Should cholera again invade us, we trust the house-to-house visitation will be practised with becoming diligence.

The institution of houses of refuge for the reception of individuals from infected localities, while their ordinary dwellings were being cleaned and surrounding nuisances removed, was another wise arrangement, which, if it did not originate with the Board of Health, at least received their approval and sanction. On this head, we with pleasure quote the words of Dr Sutherland.

"The only escape from the fatal effects of permanent causes of disease, which cannot at once be removed, is to be found in the second of the methods indicated above—to wit, the removal and dispersion of the people. This practice was found to be very successful at Edinburgh during the epidemic of 1832; and it was made matter of special regulation by the General Board of Health in all the parishes affected during the late outbreak of the disease. Large roomy buildings in healthy localities were sometimes made use of; at other times, it was found necessary to erect suitable wooden sheds, and in several instances tents were used. * * * * By referring to the examples of these outbursts already given, it will be found that no fewer than 87 per cent. of the cases, and 61 per cent. of the deaths, took place in houses where more than one person had suffered from cholera. Even in these instances, however, the danger does not in general continue long. If the people be removed, and kept away for a week or ten days, and if their houses be lime-washed during their absence, they may return home with comparative safety; while the whole number of attacks and deaths of persons removed to the houses of refuge is very much below what it would have been had they remained at home.

"All the persons admitted into them were taken from houses where the disease had actually appeared, or from their immediate vicinity. That many were powerfully under the influence of the poison of cholera is proved by the fact, that a large proportion were seized with severe choleraic diarrhœa, either before or within a

day or two of the time of admission; but as all the inmates were inspected by the medical officer twice, or oftener, during the day, very few, even of these severe cases, passed into cholera. The mortality from the epidemic has varied from 1 per cent. to 3, 4, and even 7 per cent. on the entire population of towns. These proportions include not only those in localities more immediately affected, but the unaffected population also. *The parties removed to houses of refuge were all taken, as has been stated, from infected houses or localities; and yet the table exhibits a proportion of deaths to the inmates of less than* 0·6 *per cent.* It was observed that the general health of the people was materially improved during their stay in the refuge, in some degree, no doubt, from the better diet provided for them; but mainly, I conceive, from their having been withdrawn from infected localities.

"It is very much to be regretted that this system was so inefficiently carried out in many of the affected parishes. I found almost everywhere a want of intelligence in appreciating its importance; and I hardly know an instance, except in a few of the Scotch towns, in which a house of refuge was prepared before the disease made its appearance. Even after hundreds of persons had died, I have occasionally experienced great difficulty in inducing Boards of Guardians to provide the needful accommodation. This has arisen partly from the obstacles which popular prejudice has thrown in the way of obtaining suitable premises—one of the necessary fruits of the doctrine of contagion—and partly from the fear that pauperism might be increased. The marked beneficial results which have been observed wherever a house of refuge has been properly worked, warrant me in stating, that a great many lives have been sacrificed all over the country from want of attention to the orders and notifications of the General Board of Health in regard to this matter."—Pp. 44, 45.

It is undoubtedly to the old Edinburgh Board of Health, that the merit belongs, of first pointing out the expediency of such modified quarantine restrictions, and of indicating the mode in which their advantages could be made most apparent to those for whose benefit they were intended. On referring to the Medical and Surgical Journal, Supplement, February 1833, p. 267 *et seq.*, it will be found, in Dr Christison's account of the preparatory arrangements made by the Board, in the prospect of the arrival of Cholera, that not only these refuges, but the house visitation, the removal of nuisances, and in fact every step which subsequent experience has sanctioned as humane and judicious, in dealing with the pestilence in large communities, were organised and recommended. These measures were advised as applicable, "whatever theoretical opinion might be held as to the question of contagion;" and were kindly enforced by means of "persuasion, gentle bribery, and a mere show of compulsion." In 1832, and again in 1848-49, the wisdom of such arrangements has been unequivocally demonstrated.

To other important information contained in these "Reports," we cannot, in the ordinary compass of a review, direct our readers' attention. Neither have we space at present for analysis of the many elaborate statistical tables, for which we are indebted to the labours of Dr Sutherland and Mr Grainger.

The little work of Mr Balfour, published on the recommendation of the Madras Medical Board, by authority of the Governor, is a statistical essay of great merit, constructed of materials drawn from

the best available sources, and likely to prove useful to the military surgeon, in directing sanitary measures among our armies in the East.

The pamphlet of Mr Cox is chiefly occupied with a defence of the mode of treating cholera by frequently-repeated small doses of calomel,—a practice which was powerfully advocated during the late epidemic, by Dr Ayre, and by various writers in the "Lancet." Our own experience and convictions, with regard to the general inefficacy of this treatment, correspond perfectly with those of Dr Hughes, which were made public in 1849.

Dr Bushnan's work is, as we gather from its preface, the result of a sense of public duty, stimulated by the view of an immense mass of documents connected with cholera, which circumstances threw in the author's way. He labours hard to persuade her Majesty's ministers to inquire into the merits of Dr Stevens' saline treatment of Asiatic cholera; but, as the Queen's speech has since appeared without notice of any such intention on the part of Government, we conclude that Dr Bushnan has shed his ink in vain.

Part Third.

CLINICAL REPORTS, LECTURES, ETC.

CLINICAL MEDICINE.—PROFESSOR BENNETT.

REPORT OF CASES OF NERVOUS DISEASE IN THE CLINICAL WARDS OF THE ROYAL INFIRMARY, DURING THE WINTER SESSION 1850-51.

Condensed from Lectures.

During the months of December, January, and February, there have been admitted into the clinical wards of the Infirmary several cases of disease of the nervous system, all of which have been examined with great care, while some of them are of unusual interest. Before proceeding, however, to comment on each in detail, I must remark that the diagnosis of these disorders is dependent on a kind of knowledge altogether different from that appertaining to the consideration of cutaneous, pulmonary, or cardiac affections. In these last, as we have seen, a direct appeal to the senses enables us to arrive at conclusions with tolerable accuracy. An arbitrary classification of skin diseases once established, with clear definitions, we have only to apply these to the appearances observed to ascertain the disorder. Once master the practical difficulty of distinguishing with exactitude moist from dry râles, whether a murmur replace the first or second sound of the heart, and what is its position, and we possess a key which, with the aid of percussion, will enable us to arrive at the certain diagnosis of pulmonary and cardiac affections. But, with regard to nervous diseases, no such exactitude is attainable in the present state of the science or art of medicine. The encephalon is an aggregation of various parts, more or less connected together, the functions of which are by no means

determined. In health these act in harmony, but in disease they are so irregularly disordered that, while the action of one is excited, that of another may be perverted or annihilated. Thus, nothing is more common than to observe some of the most fatal nervous diseases, such as hydrophobia, leaving after death no lesion detectable by the most careful histological examination, whilst on other occasions tumours and extensive destruction of the cerebral mass may exist, without producing any effects whatever. And yet, notwithstanding the obvious difficulties which oppose themselves to exactitude of diagnosis, careful observation, conjoined with a knowledge of physiology and pathology, will enable us to approximate closely towards, if not actually reach, a correct opinion in the great majority of cases. Seeing, then, the necessity of possessing a knowledge of the general principles arrived at by physiologists and pathologists, in order that we may form a correct diagnosis at the bedside, I propose, in the first instance, placing these shortly before you.

I. *Structure and Arrangement of the Nervous System.*—To the eye, the nervous system appears to be composed of two structures,—the gray or ganglionic, and the white or fibrous. The ganglionic, when examined under high powers, may be seen to be composed of nucleated corpuscles, varying greatly in size and shape, mingled with a greater or less number of nerve tubes, also varying in calibre. The important fact, with regard to these, is, that many of the corpuscles may be demonstrated to throw out prolongations, which are in direct communication with, or constitute, the central band or axis of Remak and Purkinje within the tubes. The fibrous structure may be shown to consist of minute tubes, which are smallest towards the periphery of the cerebrum, larger towards its base, and largest in the nerves. They are of three kinds,—1st, Cylindrical, as observed in the optic and auditory nerves; 2d, Varicose, as seen in the white substance of the cerebral lobes and of the spinal cord; and 3d, Of regular size throughout, as seen in the nerves. There are also bundles of gelatinous or flat fibres, the nature of which is much disputed, very common in the olfactory nerve and sympathetic system of nerves. There can be no doubt that some nerve tubes run into the ganglionic corpuscles, whilst others originate from them. (Wagner, Kölliker.) It is even possible that the same ganglionic corpuscle may receive and give off nerve tubes, each having distinct properties, the one of conveying the influence of impressions to, and the other of conveying influences from, the nervous centres. The peripheral termination of the nerves is in loops or arcs.

The general arrangement of the two kinds of structures should be known. By cerebrum, or brain proper, ought to be understood that part of the encephalon constituting the cerebral lobes, situated above and outside the corpus callosum; by the spinal cord, all the parts situated below this great commissure, consisting of corpora striata, optic thalami, corpora quadrigemina, cerebellum, pons varolii, medulla oblongata, and medulla spinalis. In this way, we have a cranial and a vertebral portion of the spinal cord.

In the cerebrum, or brain proper, the ganglionic or corpuscular structure is external to the fibrous or tubular. It presents on the surface numerous anfractuosities, whereby a large quantity of matter is capable of being contained in a small space. This crumpled up sheet of gray substance has been appropriately called the hemispherical ganglion. (Solly.) In the cranial portion of the spinal cord, the gray matter exists in masses, constituting a chain of ganglia at the base of the encephalon, more or less connected with each other and with the white matter of the brain proper above, and the vertebral portion of the cord below. In this last part of the nervous system the gray matter is internal to the white, and assumes the form of the letter x, having two posterior and two anterior cornua,—an arrangement which allows the latter to be distributed in the form of nerve tubes to all parts of the frame.

The white tubular structure of the vertebral portion of the cord is divided by the anterior and posterior horns of gray matter, together with the anterior and posterior sulci, into three divisions or columns on each side. On tracing

these upwards into the medulla oblongata, the anterior and middle ones may be seen to decussate with each other, whilst the posterior columns do not decussate. On tracing these up into the cerebral lobes, we observe that the anterior columns, or pyramidal tracts, send off a bundle of fibres, which passes below the olivary body, and is lost in the cerebellum—(*Arciform band* of Solly). The principal portion of the tract passes through the corpus striatum, and anterior portion of the optic thalamus, and is ultimately lost in the white substance of the cerebral hemispheres. The middle column, or olivary tract, may be traced through the substance of the optic thalamus and corpora quadrigemina, to be in like manner lost in the cerebral hemispheres. The posterior column, or restiform tract, passes almost entirely to the cerebellum. In addition to the diverging fibres in the cerebral hemispheres which may be thus traced from below upwards, connecting the hemispherical ganglion with the structures below, the brain proper also possesses bands of transverse fibres, constituting the commissures connecting the two hemispheres of the brain together, as well as longitudinal fibres connecting the anterior with the posterior lobes.

II. *Functions of the Nervous System.*—The great difference in structure existing between the gray and white matter of the nervous system, would *à priori* lead to the supposition that they performed separate functions. The theory at present entertained on this point is, that, while the gray matter eliminates or evolves nervous power, the white matter simply conducts to and from this ganglionic structure the influences which are sent or originate there.

The brain proper furnishes the conditions necessary for the manifestation of the intellectual faculties properly so called, of the emotions and passions, of volition, and is essential to sensation. That the evolution of the power especially connected with mind is dependent on the hemispherical ganglion is rendered probable by the following facts:—1. In the animal kingdom generally, a correspondence is observed between the quantity of gray matter, depth of convolutions, and the sagacity of the animal. 2. At birth, the gray matter of the cerebrum is very defective, so much so, indeed, that the convolutions are, as it were, in the first stage of their formation, being only marked out by superficial fissures almost confined to the surface of the brain. As the cineritious substance increases, the intelligence becomes developed. 3. The results of experiments by Flourens, Rolando, Hertwig, and others, have shown that, on slicing away the brain, the animal becomes more dull and stupid in proportion to the quantity of cortical substance removed. 4. Clinical observation points out, that in those cases in which the disease has been afterwards found to commence at the circumference of the brain and proceed towards the centre, that the mental faculties are affected *first*; whereas in those diseases which commence at the central parts of the organ and proceed towards the circumference, they are affected *last*.

The white tubular matter of the brain proper serves, by means of the diverging fibres, to conduct the influences originating in the hemispherical ganglion to the nerves of the head and trunk, whilst they also conduct the influence of impressions made on the trunk, in an inverse manner, up to the cerebral convolutions. The other transverse and longitudinal fibres which connect together the two hemispheres, and various parts of the hemispherical ganglion, are probably subservient to that combination of the mental faculties which characterises thought.

The spinal cord, both in its cranial and vertebral portions, furnishes the conditions necessary for combined movements; and that the nervous power necessary for this purpose depends upon the gray matter, is rendered probable by the following facts:—1st, Its universal connection with all motor nerves. 2d, Its increased quantity in those portions of the spinal cord from whence issue large nervous trunks. 3d, Its collection in masses at the origin of such nerves in the lower animals as furnish peculiar organs requiring a large quantity of nervous power, as in the triglia volitans, raia torpedo, silurus,

&c. 4th, Clinical observation points out that, in cases where the central portion of the cord is affected previous to the external portion, an individual retains the sensibility of, and power of moving, the limbs, but wants the power to stand, walk, or keep himself erect, when the eyes are shut; whereas, when diseases commence in the meninges of the cord or externally, pain, twitchings, spasms, numbness, or paralysis, are the symptoms present, dependent on lesion of the white conducting matter.

The white matter of the cord acts as a conductor, in the same manner that it does in the brain proper, and there can be no doubt that the influence arising from impressions is carried along the tracts, formerly noticed, which connect the brain and two portions of the spinal cord together. It is now also determined, that many of the fibres in the nerves may be traced directly into the gray substance of the cord,—a fact originally stated by Grainger, but confirmed by Budge and Kölliker.

The various nerves of the body consist for the most part of nerve tubes, running in parallel lines. Yet some contain ganglionic corpuscles, as the olfactory, and the expansion of the optic nerve constituting the retina, whilst the sympathetic nerve contains in various places, not only ganglia, but gelatinous flat fibres. The posterior roots of the spinal nerves possess a ganglion, the function of which is quite unknown. These roots are connected with the posterior horn of gray matter in the cord, while the anterior roots are connected with the anterior horns. As regards function, the nerves may be considered as —1st, Nerves of special sensation, such as the olfactory, optic, auditory, part of the glosso-pharyngeal and lingual branch of the fifth. 2d, Nerves of common sensation, such as the greater portion of the fifth, and part of the glosso-pharyngeal. 3d, Nerves of motion, such as the third, fourth, lesser division of the fifth, sixth, facial or portio dura of the seventh, and the hypo-glossal. 4th, Senso-motory or mixed nerves, such as the pneumo-gastric, the accessory, and the spinal nerves. 5th, Sympathetic nerves, including the numerous ganglionic nerves of the head, thorax, and abdomen,—the exact function of which has not been determined.

All nerves are endowed with a peculiar vital property, called sensibility, inherent in their structure, by virtue of which they may be excited on the application of appropriate stimuli, so as to transmit the influence of the impressions they receive to or from the brain, spinal cord, or certain ganglia, which may be considered as nervous centres. The nerves of special sensation convey *to* their nervous centres the influence of impressions caused by odoriferous bodies, by light, sound, and by sapid substances. The nerves of common sensation convey the influence of impressions *to* their nervous centres, caused by mechanical or chemical substances. The nerves of motion carry *from* the nervous centres the influence of impressions whether psychical or physical. (Todd.) The mixed nerves carry the influence of stimuli both to and from, combining in themselves the functions of common sensation and of motion. Although the sympathetic nerves also undoubtedly carry the influences of impressions, the direction of these cannot be ascertained, from their numerous anastomosis, as well as from the ganglia scattered over them, all of which act as minute nervous centres. But there *are* cases where certain psychical stimuli (as the emotions) act on organs through these nerves, and where certain diseases (as colic, gall, stones, &c.) excite through them sensations of pain.

Sensation may be defined to be *the consciousness of an impression*, and that it may take place, it is necessary,—1st, That a stimulus should be applied to a sensitive nerve, which produces an impression; 2d, That, as the result of this impression, a something should be generated, which we call an influence, which influence is conducted along the nerve to the hemispherical ganglion; 3d, On arriving there, it calls into action that faculty of the mind called consciousness or perception, and sensation is the result. It follows that sensation may be lost by any circumstance which destroys the sensibility of the nerve to impressions,

which impedes the process of conducting the influence generated by these impressions, or, lastly, which renders the mind unconscious of them. Illustrations of how sensation may be affected in all these ways must be familiar to you, from circumstances influencing the ultimate extremity of a nerve, as on exposing the foot to cold,—from injury to the spinal cord, by which the communication with the brain is cut off, or from the mind being inattentive, excited, or suspended.

The independent endowment of nerves is remarkably well illustrated by the fact, that whatever be the stimulus which calls their sensibility into action, the same result is occasioned. Mechanical, chemical, galvanic, or other *physical* stimuli, when applied to the course or the extremities of a nerve, cause the very same results as may originate from suggestive ideas, perverted imagination, or other *psychical* stimuli. Thus a chemical irritant, galvanism, or pricking and pinching a nerve of motion, will cause convulsion and spasms of the muscles to which it is distributed. The same stimuli applied to a nerve of common sensation will cause pain, to the optic nerve flashes of light, to the auditory nerve ringing sounds, and to the tip of the tongue peculiar tastes. Again, we have lately had abundant opportunities of seeing that suggestive ideas, or stimuli arising in the mind, may induce peculiar effects on the muscles, give rise to pain or insensibility, and cause perversion of all the special senses.

Motion is accomplished through the agency of muscles, which are endowed with a peculiar vital property, called contractility, in the same way that nerve is endowed with the property of sensibility. Contractility may be called into action altogether independent of the nerves (Haller), as by stimulating an isolated muscular fasciculus directly. (Weber.) It may also be excited by physical or psychical stimuli, operating through the nerves. Physical stimuli applied to the extremities or course of a nerve, may cause convulsions of the parts to which the motor filaments are distributed directly, or they may induce combined movements in other parts of the body *diastaltically* (Marshall Hall),—that is, through the spinal cord. In this latter case the following series of actions take place:—1st. The influence of the impression is conducted to the spinal cord by the afferent or *esodic* filaments which enter the gray matter. 2d. A motor influence is transmitted outwards by one or more efferent or *exodic* nerves. 3d. This stimulates the contractility of the muscles to which the latter are distributed, and motion is the result. Lastly, contractility may be called into action by psychical stimuli or mental acts—such as by the will and by certain emotions. Integrity of the muscular structure is necessary for contractile movements; of the spinal cord, for diastaltic or reflex movements; and of the brain proper, for voluntary or emotional movements.

Thus, then, we may consider that the brain acting alone furnishes the conditions necessary for intelligence; the spinal cord acting alone furnishes the conditions essential for the co-ordinate movements necessary to the vital functions; and the brain and spinal cord acting together furnish the conditions necessary for voluntary motion and sensation.

An account of the various cerebral, spinal, and cerebro-spinal functions, as they are performed separately or conjointly, belongs to the course of the Institutes of Medicine, and with these you are supposed to be familiar. It is important, however, that we dwell more at length on

III. *The Pathological Laws which Regulate Diseased Functions of the Nervous System.*—For the purposes of diagnosis and treatment, it is a matter of great importance to attend to the following generalisations:—

(1.) *The amount of fluids within the cranium must always be the same so long as its osseous walls are capable of resisting the pressure of the atmosphere.* There are few principles in medicine of greater practical importance than the one we are about to consider,—the more so, as many able practitioners have lately abandoned their former opinions on this head, and on what I consider to be

very insufficient grounds. On this point, therefore, I cannot do better than condense and endeavour to put clearly before you the forcible arguments of the late Dr John Reid, with such other considerations as have occurred to myself.

That the circulation within the cranium is different from that in other parts of the body, was first pointed out by the second Monro. It was tested experimentally by Dr Kellie of Leith, ably illustrated by Dr Abercrombie, and successfully defended by Dr John Reid. The views adopted by these distinguished men were, that the cranium forms a spherical bony case, capable of resisting the atmospheric pressure, the only openings into it being the different foramina by which the vessels, nerves, and spinal cord pass. The encephalon, its membranes and blood vessels, with perhaps a small portion of the cerebro-spinal fluid, completely fill up the interior of the cranium, so that no substance can be dislodged from it without some equivalent in bulk taking its place. Dr Monro used to point out, that a jar, or any other vessel similar to the cranium, with unyielding walls, if filled with any substance, cannot be emptied without air or some other substance taking its place. To use the illustration of Dr Watson, the contents of the cranium are like beer in a barrel, which will not flow out of one opening, unless provision be made at the same time that air rushes in. The same kind of reasoning applies to the spinal canal, which, with the interior of the cranium, may be said to constitute one large cavity, incompressible by the atmospheric air.

Before proceeding further, we must draw a distinction between pressure on, and compression of, an organ. Many bodies are capable of undergoing a great amount of pressure without undergoing any sensible decrease in bulk. By compression must be understood, that a substance occupies less space from the application of external force, as when we squeeze a sponge, or compress a bladder filled with air. Fluids generally are not absolutely incompressible, yet it requires the weight of one atmosphere, or fifteen pounds in the square inch, to produce a diminution equal to $\frac{1}{20.000}$th part of the whole. Now this is so exceedingly small a charge upon a mass equal in bulk to the brain, as not to be appreciable to our senses. Besides, the pressure on the internal surface of the blood vessels never exceeds ten or twelve pounds on the square inch, during the most violent exertion, so that, under no possible circumstances, can the contents of the cranium be diminished even the $\frac{1}{20.000}$th part. When the brain is taken out of the cranium it may, like a sponge, be compressed, by squeezing fluid out of the blood vessels; but during life, surrounded, as it is, by unyielding walls, this is impossible. For let us, with Abercrombie, say, that the whole quantity of blood circulating within the cranium is equal to 10—5 in the veins, and 5 in the arteries; if one of these be increased to 6, the other must be diminished to 4, so that the same amount, 10, is always preserved. It follows, that when fluids are effused, blood extravasated, or tumours grow, a corresponding amount of fluid must be pressed out, or of brain absorbed, from the physical impossibility of the cranium holding more matter. At the same time, it must be evident that an increased or diminished amount of pressure may be exerted *on* the brain, proportioned to the power of the heart's contraction, the effect of which will be, not to alter the amount of fluids within the cranium, but to cause, using the words of Abercrombie, "a change of circulation" there.

Dr Kellie performed numerous experiments on cats and dogs, in order to elucidate this subject. Some of these animals were bled to death by opening the carotid or femoral arteries, others by opening the jugular veins. In some the carotids were first tied, to diminish the quantity of blood sent to the brain, and the jugulars were then opened, with the view of emptying the vessels of the brain to the greatest possible extent; while, in others, the jugulars were first secured, to prevent as much as possible the return of the blood from the brain, and one of the carotids was then opened. He inferred, from the whole inquiry, which was conducted with extreme care, "That we cannot, in fact, lessen, to any considerable extent, the quantity of blood within the cranium by

arteriotomy or venesection; and that when, by profuse hemorrhages destructive of life, we do succeed in draining the vessels within the cranium of any sensible portion of red blood, there is commonly found an equivalent to this spoliation in the increased circulation or effusion of serum, serving to maintain the plenitude of the cranium."

Dr Kellie made other experiments upon the effects of position immediately after death from strangulation or hanging. He also removed a portion of the unyielding walls of the cranium in some animals, by means of a trephine, and then bled them to death; and the differences between the appearances of the brain in these cases, and in those where the cranium was entire, were very great. One of the most remarkable of these differences was its shrunk appearance, in those animals in which a portion of the skull was removed, and the air allowed to gravitate upon its inner surface. He says:—"The brain was sensibly depressed below the cranium, and a space left, which was found capable of containing a teaspoonful of water."

It results from these inquiries, that there must always be the same amount of fluids within the cranium so long as it is uninjured. In morbid conditions these fluids may be blood, serum, or pus; but in health, as blood is almost the only fluid present (the cerebro-spinal fluid being very trifling), its quantity can undergo only very slight alterations. There are many circumstances, however, which occasion local congestions in the brain, and consequently unequal pressure on its structure, in which case another portion of its substance must contain less blood, so that the amount of the whole, as to quantity, is always preserved. These circumstances are mental emotions, hemorrhages, effusions of serum, and morbid growths. Such congestions, or local hyperhemias, in themselves constitute morbid conditions; and nature has, to a great extent, provided against their occurrence under ordinary circumstances, by the tortuosity of the arteries and the cerebro-spinal fluid, described by Magendie.

The views now detailed had been very extensively admitted into pathology, when Dr Burrows, of St Bartholomew's Hospital, endeavoured to controvert them, first in the Lumleian lectures of 1843, and subsequently in a work published in 1846, entitled, "On Disorders of the Cerebral Circulation, and on the Connection between Affections of the Brain and Diseases of the Heart." Dr Burrows, however, evidently formed the most confused notions of the doctrine we are advocating; for, instead of stating it as propounded by its authors, he *actually misrepresented it*, as Dr Reid pointed out. Thus, he is always combating the idea that blood-letting, position, strangulation, &c., cannot affect the *blood in the brain;* whereas the real proposition is, that they cannot alter the *fluids within the cranium*. By thus confounding blood with fluid, and brain with cranium, he has only contrived to overthrow a theory of his own creation.

Dr Burrows has brought forward several observations and experiments, which he considers opposed to the theory now advocated. His facts are perfectly correct. I myself have repeated his experiments on rabbits, and can confirm his descriptions. It is the inferences he draws from them that are erroneous. For the paleness which results from hemorrhage, and the difference observable in the colour of the brain, when animals, immediately after death, are suspended by their ears or by their heels, is explicable by the diminished number of coloured blood particles in the one case, and by their gravitation downwards in the other. That the amount of fluid within the cranium was in no way affected, is proved by the plump appearance of the brains figured by Dr Burrows, and the total absence of that shrunken appearance so well described by Dr Kellie.

Neither does our observation of what occurs in asphyxia or apnœa, oppose the doctrine in question, as Dr Burrows imagines, but rather confirms it. On this point the following observations by Dr John Reid are valuable. He says: "If any circumstance could produce congestion of the vessels within the cranium, it would be that of death by hanging; for then the vessels going to and

coming from the brain are, with the exception of the vertebral arteries, compressed and then obstructed. These two arteries, which are protected by the peculiarity of their course through the foramina of the transverse processes of the cervical vertebræ, must continue for a time to force their blood upon the brain, while a comparatively small quantity only can escape by the veins. Indeed, the greater quantity of blood carried to the encephalon by the vertebrals returns by the internal jugulars, and not by the vertebral veins, which are supplied from the occipital veins of the spinal cord; and the anastomoses, between the cranial and vertebral sinuses, could carry off a small quantity of the blood only, transmitted along such large arteries as the vertebrals. And yet it is well known that there is no congestion of the vessels within the cranium after death by hanging, however gorged the external parts of the head may be by blood and serum." This is admitted by Dr Burrows, although he endeavours to get rid of so troublesome a fact by a gratuitous hypothesis, which will not bear a moment's examination, but for the refutation of which I must refer to the works of Dr Reid.[1]

On the whole, whether we adopt the expressions of local congestion, of change of circulation within the cranium (Abercrombie), or of unequal pressure (Burrows), our explanation of the *pathological* phenomena may be made equally correct, because each term implies pretty much the same thing. But if we imagine that venesection will enable us to diminish the amount of blood in the cerebral vessels, the theory points out that this is impossible, and that the effects of bleeding are explained by the influence produced on the heart, the altered pressure on the brain, exercised by its diminished contractions, and the change of circulation within the cranium thereby occasioned.

I have entered somewhat fully into this theory, because, independent of its vast importance in a practical point of view, it is one which originated in, and has always been maintained by, the Edinburgh School of Medicine. Singular to say, notwithstanding the obvious errors and fallacies in Dr Burrows' work, no sooner did it appear than the whole medical press of England and Ireland adopted its conclusions, and even Dr Watson, in the last edition of his excellent work, also abandoned the theory of Monro, Kelly, and Abercrombie. But so far is this theory concerning the circulation within the cranium from being shaken by the attack of Dr Burrows, that it may be said now to stand on a firmer basis than ever, owing to that attack having drawn forth the convincing reasoning and unanswerable arguments of so sound an anatomist, physiologist, and pathologist as the late Dr John Reid.

(2.) *All the functions of the nervous system may be increased, perverted, or destroyed, according to the degree of stimulus or disease operating on its various parts.* Thus, as a general rule, it may be said, that a slight stimulus produces increased or perverted action; whilst the same stimulus, long continued or much augmented, causes loss of function. All the various stimuli, whether mechanical, chemical, electrical, or psychical, produce the same effects, and in different degrees. Circumstances influencing the heart's action, stimulating drinks or food, act in a like manner. Thus, if we take the effects of alcoholic drink, for the purpose of illustration, we observe that, as regards combined movements, a slight amount causes increased vigour and activity in the muscular system. As the stimulus augments in intensity, we see irregular movements occasioned, staggering, and inability of directing the limbs. Lastly, when the stimulus is excessive, there is complete inability to move, and the power of doing so is temporarily annihilated. With regard to sensibility and sensation, we observe cephalalgia, tingling, and heat of skin, tinnitus aurium, confusion of vision, muscæ volitantes, double sight, and lastly, complete insensibility and coma. As regards intelligence, we observe at first rapid flow of ideas, then confusion of

[1] Monthly Journal, August 1846. Physiological, Anatomical, and Pathological Researches, No. XXV.

mind, delirium, and lastly sopor and perfect unconsciousness. In the same manner pressure, mechanical irritation, and the various organic diseases, produce augmented, perverted, or diminished function, according to the intensity of the stimulus applied, or amount of structure destroyed.

Thus it has been shown, that excess or diminution of stimulus, too much or too little blood, very violent or very weak cardiac contractions, and inflammation or extreme exhaustion, will, so far as the nervous functions are concerned, produce similar alterations of motion, sensation, and intelligence. Excessive hemorrhage causes muscular weakness, convulsions, and loss of motor power, perversions of all the sensations, and lastly, unconsciousness from syncope. Hence the general strength of the frame cannot be judged of by the nervous symptoms, although the treatment of these will be altogether different, according as the individual is robust or weak, has a full or small pulse, &c. These similar effects on the nervous centres from apparently such opposite exciting causes, can, it seems to me, only be explained by the peculiarity of the circulation previously noticed. A change of circulation within the cranium takes place, and whether arterial or venous congestion occurs, pressure on the organ is equally the result. The importance of paying attention to this point in the treatment must be obvious.

(3.) *The seat of the disease in the nervous system influences the nature of the phenomena or symptoms produced.* It is a matter of very great importance to ascertain how far certitude in diagnosis may be arrived at, and the seat of the disease ascertained. On this subject it may be affirmed that, although clinical observation combined with pathology have done much, more requires to be accomplished. As a general rule, it may be stated, that disease or injury of one side of the encephalon, above the decussation in the medulla oblongata, especially influences the opposite side of the body; whilst, if the spinal cord be affected below the decussation, the influence produced is not crossed, but direct. It is said that some very striking exceptions have occurred to this rule, but these at any rate are remarkably rare. Besides, it has always appeared to me probable that, inasmuch as extensive organic disease, if occurring slowly, may exist without producing symptoms, whilst it is certain most important symptoms may be occasioned without organic disease, even these few exceptional cases are really not opposed to the general law. Then, as a general rule, it may be said that diseases of the brain proper are more especially connected with perversion and alteration of the intelligence; whilst disease of the cranial portion of the spinal cord and base of the cranium, are more particularly evinced by alterations of sensation and motion. In the vertebral portion of the cord, the intensity of pain and of spasm, or want of conducting power, necessary to sensation and voluntary motion, indicates the amount to which the motor and sensitive columns are affected. Further than this we can scarcely generalise with prudence, although there are some cases, as we shall subsequently see, where careful observation has enabled us to arrive at more positive results.

The fatality of lesions affecting various parts of the nervous centres varies greatly. Thus the hemispheres may be extensively diseased, often without injury to life, or even permanent alteration of function. Convulsions and paralysis are the common results of disease of the ganglia, in the cranial portion of the cord. The same results from lesion of the pons varolii. But this, if it affect the medulla oblongata, where the eighth pair originates, or injury to this centre itself, is almost always immediately fatal.

(4.) *The rapidity or slowness with which the lesion occurs influences the phenomena or symptoms produced.* It may be said as a general rule, that a small lesion, for instance a small hemorrhagic extravasation, occurring suddenly, and with force, produces, even in the same situation, more violent effects than a very extensive organic disease which comes on slowly. Here, however, much will depend upon the seat of the lesion. Very extraordinary cases are on record, where large portions of the nervous centres have been much disorganised, without producing anything like such violent symptoms as have been occa-

sioned at other times by a small extravasation in the same place. Here again the nature of the circulation within the cranium offers the only explanation, for the encephalon must undergo a certain amount of pressure, if no time be allowed for it to adapt itself to a foreign body; whereas any lesion coming on slowly enables the amount of blood in the vessels to be diminished according to circumstances, whereby pressure is avoided.

(5.) *The various lesions and injuries of the nervous system produce phenomena similar in kind.* The injuries which may be inflicted on the nervous system, as well as the morbid appearances discovered after death, are various. For instance, there may be an extravasation of blood, exudation of lymph, a softening, a cancerous tumour, or tubercular deposit, and yet they give rise to the same phenomena, and are modified only by the circumstances formerly mentioned, of degree, seat, suddenness, &c. Certain nervous phenomena also are of a paroxysmal character, whilst the lesions supposed to occasion them are stationary or slowly increasing. It follows, that the effects cannot be explained by the nature of the lesions, but to something which they all have in common; and this, it appears to me, may consist of,—1st, Pressure with or without organic change; 2d, More or less destruction or disorganisation of nervous texture. Further, when we consider that the same nervous symptoms arise from irregularities in the circulation from increased as well as diminished action, sometimes when no appreciable change is found, as well as when disorganisation has occurred, the theory of local congestions in the nervous centres seems to me the most consistent with known facts. That such local congestions do frequently occur during life, without leaving traces detectable after death, is certain; whilst the occurrence of molecular changes, or other hypothetical conditions which have been supposed to exist, have never yet been shown to take place under any circumstances.

CASE I.—*Apoplexy, followed by Hemiplegia of Left Side.—Progressing Recovery.*

Andrew Bryce, æt. 72, tailor, admitted into the clinical wards Jan. 29, 1851. He states that on the 9th October last, while walking up Dundas Street, he suddenly felt very dizzy, and was obliged to sit down. He lost his senses completely for several minutes, and when he again became conscious he found he could not walk. He was assisted home, and on the afternoon of the same day admitted into No. 10 of the Royal Infirmary. He was freely purged, and has been confined to bed ever since. Since his admission he has been gradually gaining power in the paralysed side. At present he complains of occasional frontal headach, increased during cold weather. His intelligence is unaffected. The special senses also normal. He cannot raise himself into a sitting posture. He can move the left hand, but is unable to direct it steadily to a distant object; neither can he place it on the top of his head. While in bed the left leg moves as perfectly as the right, but he cannot stand upon it. The sensibility of the left leg appears to be at present normal, but that of the left arm is slightly impaired. There is also a little rigidity of the left arm. The tongue is protruded in the median line. On laughing, the mouth is drawn slightly towards the right side. Sensibility of the face unimpaired. The pulse 88—is of natural strength. The urine is not so copious as formerly, and is made in a smaller stream and more frequently. He has sometimes passed gravel, and observed his urine of a high colour—sp. gr. 1023—non-albuminous—a copious deposit of phosphates. This man remained a few days in the ward, during which no change occurred in the symptoms, and on Feb. 7th he returned to No. 10.

CASE II.—*Apoplexy followed by Hemiplegia of the Right Side.—Progressing Recovery.*

Anthony Caron, hairdresser, a Frenchman, admitted to the clinical wards Feb. 6, 1851, in a state of coma, in which condition he was found lying on the Granton road, and was immediately brought to the Infirmary. When

first examined, he appeared to be about fifty years of age, and of robust habit of body. He was perfectly unconscious of surrounding objects, and as if in a deep sleep—the surface pale, the countenance calm, and respirations natural. The mouth was slightly drawn to the left side. The right arm, allowed to fall down after being raised, sunk rapidly like an inert mass—the left, similarly treated, fell slowly. Pricking the right leg and foot caused no shrinking of the limbs; but the left limbs when pricked were rapidly withdrawn. Pulse 60, rather full, but, considering his habit of body, probably of natural strength. On returning to him an hour after the first examination, consciousness had to a considerable extent returned. He opened the mouth when asked to do so, and the tongue was ascertained to be covered with a white fur. He also raised the left arm and leg readily, but could not move the limbs on the right side. The pulse remained the same. In consequence of this change in his condition, the full bleeding which was previously determined on from this arm, was changed into the removal of ℥xij. of blood, by means of cupping, from the nape of the neck. He was ordered a pill containing Ol. crotonis M. i., and ext. colocynth. gr. v. to be taken every four hours until the bowels were freely moved. Cold was to be applied to the head, and the most perfect quietude enjoined.

The following day he was much better—consciousness evidently more alive—and he has been gradually improving since (Feb. 24.)

Commentary.—The term apoplexy has been used in two senses. By the older writers and clinical observers, it was used to denote a sudden loss of consciousness and volition, independent of the various morbid lesions which may occasion this apoplectic state. By the followers of the French school of pathology, the same word has been applied to an extravasation of blood into an organ; and hence the terms apoplexy of the lung, apoplexy of the spinal cord, apoplexy of the kidney, &c. Ecchymosis of the skin, the result of a contusion—such as is present in what is commonly called a "black eye,"—is in this sense an apoplexy. I believe it better to adhere to the meaning of our ancestors, the more so as it is not always possible to determine when a cerebral hemorrhage is present. Hence the two cases which have been given, characterised by sudden loss of consciousness and volition, coming on spontaneously, are called apoplexy. But should such cases prove fatal, and we are enabled to speak positively as to the cause of the apoplexy from post-mortem examination, then we may denominate them with more propriety cerebral hemorrhage, softening, &c. &c., according to circumstances.

In the two cases before us there is great similitude,—both were seized suddenly with apoplexy while walking. In Case I., however, the apoplectic state was of a few minutes' duration; in Case II. it continued several hours. The one case occurred between three and four months before it came under your observation; in the other, you had an opportunity of seeing him in the apoplectic condition, or immediately after he emerged from it. In both there was perfect hemiplegia; but in the one case there has been complete recovery of sensation and partial recovery of motion, as regards the affected side. In the other, both sensation and motion are still completely lost.

The treatment of such cases is, to the rational practitioner, a subject of anxious consideration. The empirical practitioner has little difficulty,—venesection to a large extent being the established routine remedy. The advance of pathological knowledge, however, must have made it apparent, that the same proceeding is not likely to be beneficial in all cases where the nervous centres are similarly affected. We may have sudden loss of consciousness and volition from syncope, as well as from coma, the only difference between the two being, that the same nervous phenomena commence in the heart, with a weak pulse, in the one case; whilst they originate in the brain, and have a strong pulse, in the other. Careful observation, however, has sufficiently proved that there are many cases of even true hemorrhagic apoplexy which are closely allied to syncope, and which have recovered under the use of stimulants, rather

than of depletions. Indeed it seems to me very probable that many of those individuals who died under what Abercrombie called simple apoplexy, and in whom no trace of disease could be found in the brain after death, were really the victims of one form of fatty degeneration of the heart—an affection in his day altogether unknown. The best rule, therefore, I can give you, is to judge from all the circumstances of the case. Whenever the individual is of vigorous frame of body, if the face be flushed, and the pulse strong and full, a copious bleeding will be beneficial. The extent must be influenced by its effect on the heart's action; for as we have seen, the object of this measure is not to draw blood from the brain, which is impossible, but to diminish the pressure on that organ, by lessening the force with which the heart propels the blood through the carotid and vertebral arteries. On the other hand, if the individual be of spare habit, the face pale, and the pulse weak and irregular, wine, brandy, and stimulants, and restoratives generally, are demanded. But it frequently happens that when you are called in, neither one nor the other indications present themselves. It will be most prudent, under such circumstances, to use local bleeding, apply cold to the head, administer an active purgative, and above all, enjoin quietude.

On first examining Case II., you will remember I determined to bleed, but the patient shortly after exhibited signs of coming out of the apoplectic condition. It was clear, therefore, that the pressure on the brain was spontaneously diminishing; the pulse was steady and of natural strength. How could a large bleeding have benefited him? The chief point here, therefore, was not to interfere with the operations of nature, but assist her as much as possible in the operation she had next to perform,—that is, causing absorption of the clot. This I presume to be best accomplished by quietude, by the avoidance of all circumstances which could excite the heart's action, regularity of the excretions, and moderate diet. These are, therefore, the measures which have since been adopted, and his recovery, though slow, is satisfactory.

CASE III.—*Chronic Cerebral Meningitis; Inflammatory Induration surrounded by Softening of a Portion of the Left Cerebral Hemisphere.*

Mrs Swan, æt. 35, wife of a coach-builder, admitted into the Clinical Ward, December 8, 1850. She has always enjoyed good health up to four years ago, when, having contracted syphilis, and having taken a large quantity of mercury, she began to complain of headach, indigestion, and constipation. About six months ago, she had a fit, from which she recovered in the course of half an hour. She suffered from similar attacks afterwards, at intervals of from two to three weeks. These attacks were ushered in by severe headach, tinnitus aurium, vertigo, and dimness of vision, and they were followed by great muscular debility. During the paroxysms, which lasted for various lengths of time, she was insensible; there was frothing at the mouth, and twitchings of the muscles of the limbs, especially of the right arm. The last fit occurred two months ago. Four weeks ago, she experienced, without any accompanying fit or insensibility, a twitching of the muscles of the right arm, along with a feeling of numbness of the fingers of the right hand. After the shaking of the arm had ceased, she found she had less power over it than previously. The right leg was not affected by any shaking; it only felt benumbed.

On admission, she appears debilitated and considerably emaciated. There are marks of old ulcers on the right leg. There is great mental confusion, and she often wanders. There is difficulty and slowness of articulation. The right side of the face is slightly paralysed. The tongue when protruded, is slightly turned to the right side. There is no diminution of sensibility. The power of motion in the right arm is diminished; she cannot close the hand, or hold anything firmly. Sensibility is unimpared. The right leg is not affected with any diminution of muscular power, though there is a feeling of dragging when the limb is moved. She complains of intense pain in the head.

There is, however, no flushing of the face, or congestion of the eyes, and no delirium; pulse is regular and of good strength; she has no cough; complains of loss of appetite; tongue is moist, white; no vomiting or sickness. Bowels constipated; menstruation is irregular, and the discharge scanty; last menstruated six weeks ago. Urine muddy, of 1023 sp. gr.; becomes clear on heating.

From this period until the 4th of January 1851, she remained pretty much in the same condition, on some days the confusion of intellect and difficulty of speech being somewhat less than on others. The treatment consisted of the occasional application of leeches, and latterly of a blister to the nape of the neck, and purgatives. On the day mentioned, however, she was found comatose—does not answer questions, though she seems to know that she is addressed—pupils are moderately dilated—respiration is stertorous. There is slight twitching of the muscles of the right side of the face. The right arm is rigidly flexed, and may be felt to offer great resistance when an effort is made to extend it. *January* 5.—To-day appears better. No stupor. Expression not so drowsy. No stertorous breathing. Has spoken a little to-day. Has no sickness or vomiting. There are still occasional twitchings of the muscles of the right side of face. Right arm not so rigidly flexed. Ordered a purgative enema immediately. *January* 6.—Has again relapsed into a state of coma. Breathing easy. Twitching of the muscles of the right side of the face, of the right arm, and occasionally of the right leg, have again presented themselves. Pulse rather full, and slow. Bowels freely opened by the enema. Sensibility in affected parts still unimpaired. *January* 7.—Continues in much the same condition. Does not seem conscious when spoken to. Sensibility still unimpaired. Pulse frequent, and smaller than yesterday. Increased rigidity of the right arm and leg, with occasional twitchings. *January* 8.—Pulse frequent and very small. Breathing not stertorous. Lies on the left side; and the muscles of the neck are so rigid that the head is quite immoveable. Apparently sensible, though she can neither hear, speak, nor protrude the tongue. Twitchings still occasionally occur in the right side of face, right arm, and right leg. Right arm rigidly contracted. Died early on the morning of the 9th.

Sectio Cadaveris.—January 10.—Rigor mortis well marked.

Head.—There were strong adhesions between the calvarium and dura mater over the vertex, at which place the latter membrane was considerably thickened. The cerebral arachnoid membrane covering the posterior half of the left cerebral hemisphere was thickened, dense and opaque, closely adherent to the pia mater below. This thickening and adhesion existed to its greatest extent over a space about the size of half-a-crown, situated about two inches external to the falx, and at the anterior portion of the middle third of the hemisphere. Here the arachnoid membrane, united with the pia mater, was one-eighth of an inch thick; and the dense layer being carefully dissected off, exposed a discoloured spot in the cerebral convolutions measuring an inch and a half from before backwards, and one inch transversely. The centre of this spot was indurated to the feel, whilst its circumference was soft and pulpy. In the centre there was observed a hard deposit, the size of a pea, of a bright yellow colour, surrounded by a purple areola, passing into a pink colour, and disappearing gradually towards the margin of the spot alluded to. On making sections through this diseased portion, the discoloration was found to extend inwards and occupy a space about the size of a walnut. It contained imbedded in its substance five other indurated masses, varying in size from a millet seed to that of a pea, and similar to the one formerly noticed. The boundaries of this diseased mass internally presented the same colour and consistence as were noticed on the surface, with the exception perhaps that the disappearance of colour was more gradual internally, and passed into a pulpy white softening of the cerebral hemisphere, which extended from it in a straight line until it terminated in the external portion of the left optic thalamus. The two lateral

ventricles contained each about half a drachm of slightly sanguinolent fluid, and, in the left one, a vesicle the size of a pea, containing amber-coloured matter, sprung from the choroid plexus. Other portions of the encephalon were healthy.

Chest.—Heart healthy. Valves normal. No adhesion of the pleuræ. The bronchi, when cut, poured out a sero-sanguinolent fluid. Left lung throughout spongy and crepitant, with much pigmentary matter scattered through it. Right lung was non-crepitant and engorged posteriorly and inferiorly, presenting a mottled appearance when cut, from a number of minute granulations scattered throughout. All the other viscera were quite healthy.

Microscopic Examination.—The yellow indurated masses described as scattered throughout the diseased portion of the left cerebral lobe consisted of a dense aggregation of molecules and granules, without tubercle, pus, or any kind of corpuscle. The cerebral structure surrounding these masses was loaded with innumerable compound granular cells and masses, which existed throughout the whole discoloured portion of the brain, but became less and less numerous in the internal white softening as it approached the left optic thalamus. Indeed the most internal portion of the white softening near the optic thalamus contained none of them.

Commentary.—This woman, when she entered the hospital, presented in a most characteristic manner the general aspect and symptoms of inflammatory softening of the brain. The dulness and confusion of intellect, without loss of volition and sensation—the weakness of the right side of the body, and contraction of the right arm—latterly the rigidity of this extremity and the coma, could leave little doubt as to the nature of the lesion, and its seat in the left hemisphere. From the account received of her history, which, however, was not entirely to be depended on, it appeared that for four years previously she had been subject to "fits" of an epileptic character, at all events involving loss of the mental functions, and convulsive movements of the limbs, especially on the right side. This account was confirmed by the post-mortem examination, which exhibited a very chronic thickening and adhesion to the brain of the meninges on the left side, in addition to an inflammatory circumscribed softening, commencing in the circumference of the same hemisphere, and extending inwards to the optic thalamus of the same side. The yellow masses described were evidently a very chronic form of exudation, and it is very possible that these may have originated the meningitis, rather than followed it. Certainly they occasioned the surrounding discoloration and exudation, which had extended inwards to the central portions of the encephalon.

As regards the connection of the symptoms with the post-mortem appearances, we can have little difficulty in ascribing the "fits" to the meningitis, which increasing in intensity, and causing pressure downwards on the cranial portion of the cord, occasioned the convulsions. The same lesion, conjoined with the external softening and corresponding change of circulation within the cranium, was the cause of the confusion of intellect and stupidity lately observed, whilst the continued irritation originating in the local cerebral inflammation, also operating downwards through the anterior portion of the optic thalamus, and perhaps a portion of the corpus striatum, caused the contraction and rigidity observable in the right arm. It is of course impossible to determine the amount of pressure and its direction, which any lesion may occasion, except from its effect. But it seems to me that this case is an illustration of the correctness of the pathological laws formerly given. The first symptoms are those of excitation, and are paroxysmal; these pass into more permanent symptoms; and as the organic disease proceeds from the circumference to the centre, we observe the intelligence affected most, motion secondarily, and sensibility not at all.

CASE IV.—*Chronic Inflammatory Softening of Right Corpus Striatum; Hemorrhage into the Pons Varolii; Cardiac Hypertrophy; Hemorrhage into the Lungs.*

Mrs Macpherson, æt. 34, admitted December 22, of intemperate habits. She has been troubled for the last four years more or less with cough. Five years ago she had an attack of paralysis affecting the left side of whole body. Her speech was thick. The left cheek appeared more prominent than natural; there were twitchings also of the left arm. Leg not affected. Intellect unimpaired. From this she recovered perfectly in two or three months. She continued, however, her intemperate habits, and was addicted to taking laudanum. Four months ago, after taking a drachm of laudanum, she was suddenly seized with violent convulsions, sprang a little distance, and fell on her face. She was quite unconscious at the time, a condition from which she gradually emerged, but her mind has ever since been affected, and the power over the left side is much impaired. Since this second attack, she has been subject to violent and sudden fits of coughing, lasting for hours without intermission, which have latterly increased.

On admission, she leans to the right side when sitting. Countenance anxious. Motions of chest rapid, with much elevation of thorax during inspiration. Dyspnœa urgent. Cough constant and paroxysmal. Expectoration copious. On percussion the anterior surface of the chest sounds resonant. There is dulness over the infra-scapular region of left side. On auscultation, the inspiration is short, and the expiration much prolonged, and accompanied with sibilant and sonorous râles over the whole anterior surface of both sides; loud crepitating and mucous râles over the inferior portion of left back, with distinct crepitation also inferiorly in right back. Vocal resonance is increased over left infra-clavicular region. Heart's sounds normal, distant. Tongue of a brown colour, moist. Appetite bad. Bowels regular. Catamenia regular. Has no pain in head or any part of her body. Skin hot and moist. She was treated with various anodyne expectorant mixtures, sulphuric and nitric ether, ipecacuan wine, chloroform, morphia, &c. *December* 30.—Is weaker to-day. Countenance sunk and anxious. Tossing about of arms. Breathing short and rapid. Cough almost ceased. Expectoration greatly diminished. Pulse weak, scarcely perceptible. Ordered spirit. communis, ℥ij.; porter 1 pint. *January* 1.—Exhaustion still greater. With difficulty roused to answer questions; incoherent in her conversation; sleeps little; breathing rapid, short, and laboured; paroxysmal cough. The râles formerly noticed still continue, dulness over the left back more extensive and complete. Pulse small. ℞. Spirit. commun. ℥iv. *January* 2.—Since yesterday there have been coldness and lividity of face, with stupor gradually increasing. Dyspnœa very urgent. Expectoration scanty. These symptoms increasing, she died at 3 A.M., January 3.

Sectio Cadaveris.—January 3.—*Head.*—The dura matter and arachnoid membrane were healthy in structure, but the sub-arachnoid cavity contained superiorly a small quantity of serum between the sulci. Both lateral ventricles contained about a drachm of fluid, but that on the right side was opaque, of a grayish colour like dirty milk, while that on the left side was colourless and clear. Three-fourths of the right corpus striatum posteriorly was reduced to a fawn-coloured diffluent pulp, from which a turbid gray fluid flowed out on puncture, similar to what had tinged that in the ventricle. The white substance external to the corpus striatum was not affected, the lesion being limited to a space about the size of an almond nut. On cutting through the softened texture, a few bright yellow patches were observable, about the size of a millet seed, closely resembling in appearance the reticulum often seen in soft cancer. On slicing the pons varolii, there was observed near its centre, a little to the right of the median line, a hemorrhagic extravasation the size of a

small pea, the centre of a dark red, and the circumference passing into a rusty brown. Other portions of the encephalon were healthy.

Chest.—The left ventricle of the heart was somewhat hypertrophied, the apex rounded, the mitral orifice was smaller than usual,—just admitting the thumb,—but there was no thickening or disease of the lining membrane. Lining membrane of the heart and large vessels stained of a claret colour—blood fluid. Both lungs anteriorly emphysematous. The lining membrane of the bronchi of dark mahogany colour, and more or less filled with sanguinolent mucus. Inferior lobe of left lung greatly engorged, containing coagulated masses of extravasated blood, varying in size from a pea to a moderate-sized orange. Inferior lobe of right lung also engorged, with similar masses of blood, but not so numerous or so large as on the opposite side. Abdominal organs healthy

Microscopic Examination.—The turbid fluid in the right ventricle contained numerous floating compound granular cells and masses. The softened portion of the corpus striatum was infiltrated with them throughout, and the bright yellow masses were composed of an aggregation of the same cells and masses mingled with innumerable molecules and granules. The clot in the pons varolii contained several round and oval cells, varying in size from the four to the six hundreth of an inch in diameter, crowded with blood corpuscles. No compound granular cells were anywhere visible in its neighbourhood.

Commentary.—The history of what occurred to this woman previous to her admission, involving the account of the two paralytic seizures, was obtained after her death from the husband, who attended the post-mortem examination. During the period she was under treatment, the pulmonary symptoms alone were those that excited attention. The weakness stated to exist on the left side of the body was certainly very slight, as, in the frequent examinations which occurred, it was observed that she sat up when desired to do so, presented either hand when bid to have the pulse felt, and frequently got out of bed without assistance. The crepitating and mucous rales, with the dulness of percussion and great prostration of the patient, however, left little hopes from the first of her recovery ; and of these symptoms she alone complained, never speaking of a former or a present palsy. These facts in themselves are very curious, when compared with those narrated when the brain was examined, although here it must be confessed that the investigation of the nervous phenomena, from the dislike of closely interrogating a woman evidently dying, was not very minute. Indeed, it was the husband's account of the two paralytic seizures which even led to the careful examination of the encephalon ; and there can be little doubt that the first attack was owing to disease (perhaps a hemorrhage) into the right corpus striatum five years previously, and the second, four months before admission, to the limited hemorrhage into the pons varolii. The histological facts ascertained in connection with the hemorrhagic clot are important, and I shall again allude to them, in connection with the charges observed during the disintegration of blood, extravasated into the nervous centres.

CASE V.—*Hemorrhage into the Right Crus Cerebri.—Meningitis at the base of the Encephalon.—Serous Effusion into the Lateral Ventricles.*

George Crichton, æt. 28, brewer, admitted Jan. 31, 1851. For the last six months he has suffered from a short dry cough, and has sweated profusely at night. His appetite has been good, and he considered himself in good health. Three weeks ago he felt pain in his head, which gradually increased in intensity, although he continued at his work. On the 26th, feeling the headach very severe, he applied eight leeches, but without relief. On the following day (last Monday), as he was going to his work, he had a severe fit of coughing, and expectorated a teaspoonful of florid blood. Immediately after he felt giddy and stupid, being obliged to support himself against a wall. He says he never lost his recollection, recovered himself in a few minutes, and

walked home, but with difficulty; afterwards he felt weak, but had perfect command over all his muscles. On Tuesday evening he felt drowsy, and on awakening from one of his short sleeps, he discovered that the power of moving the left arm was much diminished. The left leg was unaffected.

On admission, appears weak and emaciated; complains of pain in the forehead; most severe on the right side; has no pain elsewhere. His intelligence seems but little affected. He speaks slowly, reluctantly, and with an effort. The special senses are unimpaired. The power of motion in his left arm and leg is almost entirely gone. He can neither stretch nor flex his arm or leg. His leg has become much more useless within the last twenty-four hours. Sensibility of the parts is unimpaired, and he feels impressions made upon them. His mouth is very slightly twisted to the right side; tongue protruded straight. During the examination before the class, he was seized with spasmodic movements of the lower jaw, lasting for a minute and a half, unattended with pain. This was first observed on Wednesday,—when it occurred nine times,—and has returned at irregular periods since. Pulse 60, not increased in strength. Cardiac sounds normal. Appetite good. Tongue clean in centre; covered with a white fur at edges. Bowels generally costive; not opened by medicine last night. Urine 1027 sp. gr.—deposits a copious sediment of mucus and phosphates. Has occasional slight cough; there is dulness on percussion under left clavicle, with harsh inspiration, and great increase of vocal resonance. He was ordered ice to the head; quietness to be maintained.

Feb. 2.—To-day headach is abated, he complains of weakness in the right eye, which he cannot keep open without an effort. On frowning, the corrugations are more distinct over the left eye. The right pupil is less contracted than the left. *Feb.* 3.—Slept ill last night, had a good deal of convulsive twitching of the unaffected side. Is more confused. Tongue has a dense white fur over it. Bowels costive. ℞ *Olei Ricini*, ℥iss. to be followed by laxative enema, if required. *Feb.* 4.—Bowels moved after administration of the injection. Has had a good deal of muttering delirium,—passes his urine in bed. Appears to know he is addressed, if spoken to in a loud voice, but gives no answer. Pulse 60. Small and weak. Twitchings more distinct and decided. Breathing not laboured. ℞ *Carb. Ammon. gr. xij.; Mist. Camph.* ℥ *vj.; M. Signa, Two table spoonfuls every 3d hour; App. Vesicat.* 3 + 4 *ad Nuchæ.—Cold to head. Feb* 5.—Muttering delirium last night; does not seem to feel a prick on his left leg; is restless when spoken to, but never speaks. Pulse 60, still small, but increasing; has some dysphagia. *Feb.* 6.—Quiet during the night; had a slight attack of general convulsions; bowels opened by enema. Pulse 75, of good strength. *Feb.* 7.—Was more restless during the night; picking the bedclothes; no muttering; respiration is more hurried and laboured; there is puffing of the left cheek during expiration; left pupil more contracted than right; jactitation of the right arm. *Feb.* 8.—Dysphagia is increasing. Pulse 80. Twelve leeches were applied to the temples. To have one drop of croton oil every four hours. *Feb.* 9.—Breathing more hurried and laboured. Pulse 116, small and weak. Bowels freely opened by the croton oil. Appeared to feel the bites of the leeches. This morning he had a return of the general convulsions, more severe, and lasting for a longer period than the former, accompanied with frothing at the mouth. *Feb.* 10.—Last night, about 8 P.M., he began to moan and cry out, but no convulsions. The respiration became more laboured, and accompanied by a tracheal râle. At 12 P.M., he died comatose.

Sectio Cadaveris.—Feb. 12.—*Head.*—On removing the calvarium, the sinuses of the dura mater were found almost empty, the longitudinal one contained a small decolorised coagulum. The cerebral arachnoid was very dry, the surfaces of the hemispheres flattened, and the convolutions pressed together. The ventricles were distended by 13 drachms of colourless limpid serum, and freely communicated with each other by means of the foramen of Monro, which was much enlarged. The fornix, septum lucidum, floor of the fourth ventricle, and corpus callosum, were of pultaceous consistence, and readily broke down under

the fingers. On removing the brain, a semi-opaque exudation of yellowish white colour was seen in the subarachnoid space at the base of the brain, extending to the sylvian fissures laterally, surrounding the chiasm of the optic nerves anteriorly, and stretching as far back as the fifth pair posteriorly. Here, however, the coagulated exudation was very thin and soft, whereas immediately behind the optic commissure, it was one-eigth of an inch thick, and of considerable density. On slicing the optic thalamus from above downwards on the right side, there was discovered below that ganglion, in the crus cerebri, a clot of dark red blood the size of a pea, surrounded by several smaller red spots, the result of capillary hemorrhage. The cerebral substance surrounding it was softened to the extent of a quarter of an inch all around. In the pons varolii, two masses, the largest the size of a millet seed, of yellowish indurated chronic exudation, were discovered.

Chest.—Pleuræ of both lungs were adherent at the apex, especially on the left side. The lining membrane of the bronchi appeared congested and of a reddish colour. The bronchial glands were loaded with pigment. A cavity was broken into, when separating the dense adhesions at apex of left lung, and a dirty grayish white, tolerably tenacious fluid, escaped. This cavity was capable of holding a hen's egg. Walls were irregular, and lined by no distinct membrane. The surrounding texture was of a deep red colour, and displayed on section numerous yellowish hard miliary tubercles. These were also found scattered over the lower part of the left lung. The right lung was crepitant throughout, and displayed here and there on section the same bodies as above described.

Abdomen.—Numerous yellowish miliary tubercles were found in the cortical and tubular portions of both kidneys. Other viscera healthy.

Microscopic Examination.—The exudation at the base of the brain was composed of bands of molecular fibres, mingled with curled and spiral elastic filaments. In the softer parts of the exudation, the delicate molecular fibres at irregular intervals contained nuclei, most of which were oval, and a few fusiform. The centre of the clot in the crus cerebri was composed of numerous blood corpuscles, and the surrounding softened cerebral substance contained numerous granules and granular cells. The serum in the ventricles was structureless, and the cerebral softening of the white substance contained no granular cells, the normal structure being only more easily separated and capable of being broken up when crushed between glasses.

Commentary.—This is an instructive characteristic case of that form of apoplexy which has been called *ingravescent*, commencing with headach, followed by temporary loss of consciousness and voluntary motion, their recovery, and after a period varying from a few hours to several days, gradual return of the coma, almost always followed by death. Such return of coma is usually the result of gradually increasing pressure on the brain, but the pathological cause of that pressure it is not always easy to determine. Most commonly it is the result of a hemorrhage slowly increasing, and at length forming a large coagulum. Occasionally it is caused by an effusion of serum into the ventricles, and a few cases have been observed where it was the effect of a congestion which either might or might not leave traces after death. In the present case we found four lesions of the nervous structure,—1st, Chronic exudation at the base of the brain; 2d, A hemorrhagic clot in the right crus cerebri; 3d, Accumulation of serum in the lateral ventricles; 4th, Softening of the central structures of the brain. Of these lesions the three first doubtless united in producing the symptoms, whilst the last was post-mortem, dependent on imbibition of the serum after death. Here it is important to observe, that the exudation of the base was chronic, for in structure it was firm and fibrous, characters which I have never seen in recent exudations into the sub-arachnoid cavity, which are generally purulent. It is exceedingly probable, therefore, that the headach and premonitory symptoms were occasioned by the meningitis; whilst the subsequent twitchings and convulsions were attributable to the

presence of the exudation, more especially the pressure and irritation occasioned at the base of the encephalon by the subsequent changes it passed through. Then the apoplectic attack on the 27th, was entirely owing to the hemorrhage into the right crus cerebri. This hemorrhage was small in amount, and the apoplectic condition was momentary. It probably, however, increased somewhat afterwards, and broke up the nervous structure of the crus; and the result was interruption of the conducting power between the brain and left side of the body,—in other words, hemiplegia. Lastly, the exudation and clot combined, must have exercised pressure on the veins, producing dropsy of, or effusion into, the lateral ventricles, owing to which, pressure on the whole organ, gradually augmenting, was produced, occasioning the ingravescent coma. It may be a question how far the spasms of the jaw were occasioned by the clot in the crus cerebri irritating the deep origin of the motor branch of the fifth, or by the exudation surrounding its superficial origin from the pons varolii. I am inclined to think the first theory the true one, because both divisions of the fifth were alike surrounded by the exudation at the base, and yet spasm only was caused, and no pain.

The treatment of this case caused me very anxious consideration, the more so as it was the evident opinion of the examining class, and of the clerks, that it should be treated actively by bloodletting. The student and young practitioner is generally an advocate for active treatment; and this was certainly a case in which a difference of opinion might be expected to exist even among the most experienced. The circumstances, however, which deterred me from general bleeding, were the condition of his pulse, which, though of good strength, was never full or hard, the paleness of his countenance, and his general habit of body, which was far from robust. The existence of phthisis did not influence me at the time; but I think it supports the correctness of the conclusion I arrived at. Many years ago, when studying the subject, nothing struck me more in carefully analysing the cases of Abercrombie, in reference to this question, than the fact, that notwithstanding he waited until the circulation rallied, and the pulse rose, the almost constant statement is, that a full bloodletting produced "no benefit," or "no relief," "not the smallest benefit," and so on. And in such cases, when pressure is caused by a solid coagulum or local obstruction to some part of the venous system, inducing effusion, how can bleeding lessen the pressure *when the heart's action is not increased?* We have already seen that the idea of diminishing the amount of fluids within the cranium by bleeding is visionary, and experience fully proves its uselessness, even in the hands of men who inculcate the practice. All agree, however, that you can only relieve pressure on the brain by influencing the force of the heart's contractions. But in the present case so far were these contractions from being increased, that they were normal when he was first admitted, and exhibited a tendency to diminish in force. Indeed so low was the pulse on 4th February, that I administered stimulants, *under which he rallied.* Looking then retrospectively at this case, it appears to me certain that bleeding, by diminishing the force of the general circulation, would have increased the tendency to effusion in the lateral ventricles, and would have hastened rather than retarded the fatal result.

(To be continued.)

CLINICAL SURGERY.—PROFESSOR SYME.

16th January 1851.

NECROSIS OF LOWER JAW.

THIS girl, nine years of age, has suffered for the last six months from disease of the right side of her lower jaw. You see that her face is swelled, so as to give a peculiar expression, very characteristic of necrosis affecting this bone. There are two openings in the skin,—one at the angle of the jaw, and the other on the same horizontal line with it, but farther forward, opposite the canine tooth. Through both a probe reaches the bone, and can be passed along its surface to a considerable extent. There is also an opening into the mouth, between the gum and cheek, allowing matter to pass and the dead bone to be felt. From the length of time that has elapsed since the patient began to complain, I conclude that the exfoliation must be detached, and that the substitute bone is sufficiently advanced in its formation to permit extraction without any bad effect.

I first enlarge the posterior opening over the angle of the jaw, by introducing a probe-pointed bistoury, following it with my finger, so as to ascertain the extent of exfoliation. I find that it reaches upwards to the condyle and forwards along the base of the bone. In order to facilitate extraction of the dead portion, I seize it with strong forceps at the angle, and break it into two. I then readily draw out the half which belongs to the ramus, but cannot disengage the other, which is locked within its osseous shell. Though I hoped that division of the facial artery might have been avoided, it is now plain that the incision must be extended forwards to the anterior opening, and I therefore do so. The blood gushes forth from both orifices of the vessel, and we must lose no time in securing them both, to prevent unnecessary hemorrhage, and also to protect the patient from the risk of suffocation, through the blood passing into the mouth during the profound sleep that has been induced by chloroform. Now that both orifices of the artery have been tied, I introduce the cutting pliers, and divide the osseous shell covering the exfoliation, which is then readily extracted, and accounts by its form for the difficulty previously experienced. As there is still some oozing of blood from the upper part of the cavity, I insert a piece of lint, and support it by the gentle pressure of a bandage. The child now begins to awake, and, as often happens in young people from the use of chloroform, rejects the contents of her stomach, which, you may remark, are mixed with blood, and show that our apprehension was not without reason.

It may now be asked,—Since the condyle has been detached, will the jaw be of any use for articulation or mastication? and I am happy to say, from the observation of similar cases, that in neither of these respects will there be any permanent defect. The articulation of the lower jaw, you are aware, is double, having two synovial membranes separated by an inter-articular cartilage, and I suppose that the upper cavity, immediately connected with the glenoid cavity of the temporal bone, remains unaffected, while the lower one alone is concerned in the disease, and, together with the periosteum, probably forms the matrix of a new condyle.

INTERNAL EXFOLIATION OF THE TIBIA.

This boy, fifteen years of age, has, you observe, a very large swelling of the tibia, engaging the shaft of this bone, but not extending to its extremities at either the knee or ankle, both of which joints are perfectly sound. There are two small openings over the skin, about an inch apart, in a transverse line, which allow the probe to enter a cavity, and detect an exfoliation. I cannot, in this way, ascertain that it is loose, but conclude that it must be so, from the disease having commenced two years and a half ago, and therefore proceed to its removal.

The patient being now sound asleep, through the influence of chloroform, I make a longitudinal incision, about three inches in length, at an equal distance

between the two openings, and reflect the integuments together with the periosteum, from the bone, the apertures in which I next unite by means of cutting pliers, but find that they converge so as to meet a little way below the surface, and then lead down to the central cavity, through an osseous shell, far too thick for the efficient action of any instrument except a saw. I therefore apply the trephine, which would not be easy, as I cannot take the assistance of a centre-pin, were the patient not so entirely passive as he is. I use the instrument, now that a groove has been formed, not with the caution requisite in perforating the cranium, but freely, and without intermission, until I feel it pass through the whole thickness. In withdrawing it, I find the circular portion enclosed within the saw, and, on inserting my finger into the hollow of the tibia, feel that the exfoliation is loose. I seize it with forceps, but cannot effect extraction, on account of its size, and therefore divide it by the cutting pliers, when removal is readily effected. It consists, you will observe, of the central portion of the bone, and measures two inches in length, and two-thirds of an inch in breadth, so that the opening made by the trephine was just large enough for it to escape.

It is seldom that the removal of an exfoliation occasions so much difficulty as you have just witnessed, since the shell which encloses it is usually formed, not by expansion of the old bone, but by the deposition of osseous substance in the periosteum, and whenever this membrane has been destroyed by the disease, there is a corresponding deficiency in the new bone. These "*cloacæ*," as they are named in surgical language, become smaller as the osseous shell increases in thickness and solidity, but at any period of the process admit of enlargement by the action of cutting pliers, so as to facilitate extraction of the sequestrum. But when, as in the case you have seen to-day, the exfoliation is from the interior of a thick bone like the tibia, the walls of the cavity resist any means of attack less powerful than a saw. It is seldom necessary, however, to repeat the application of a trephine, since the portions of bone that exfoliate from the interior are generally of a small size, readily passing through the aperture thus obtained.

10th February.

At our last meeting I expressed my regret for not having been able to lecture the day before, on account of being suddenly called to a distant part of Cumberland; and, as some compensation for my absence, mentioned the circumstances of the case for which my assistance had been required, as they seemed of considerable importance in a practical point of view. The patient, you will recollect, was a gentleman about thirty years of age, who, a week before, had fatigued himself in shooting, and imprudently sat down upon a wall, in the open air. He had felt unwell in going home, and for several days following been chiefly in bed. On the fourth and fifth days he had made water with difficulty, and on the sixth his medical attendant, finding a large tumour in the hypogastric region, had passed a catheter, with the effect of drawing off two or three ounces of urine, but no perceptible diminution of the swelling. It was then resolved to send for me. In the evening, the medical attendant introduced a long flexible catheter, and drew off two quarts of water, with great relief to the patient, who had previously been in an alarming state of restlessness, and tendency to delirium. When I arrived on the evening of the seventh day, it seemed as if nothing was required but a little time for convalescence. But, on considering all the circumstances, I felt an uneasy suspicion that there must be some mischief still remaining, for the patient was not a likely person to have retention induced by cold, and had not suffered from this condition until several days after the exposure to which it was attributed. I therefore feared that the retention was only a symptom of some other effect, and, from the observation of many similar cases, thought it was probable that a collection of matter had taken place. On examination, I found the scrotum and perineum free from any swelling or sign of derangement, but, upon putting my finger into the rectum, felt an obscure fluctuation. I therefore introduced a straight bistoury at the verge of the anus

and, thrusting it upwards between the bowel and prostate, evacuated between seven and eight ounces of purulent fluid. Some of you have since asked me to explain how it happened that the catheter, when first introduced, brought off a few ounces of urine merely, without emptying the bladder. This request tends to confirm my persuasion that, in the present day, solid professional information is not so much in fashion, as pursuits which must be regarded as more curious than useful. If you spent less time in reading the ephemeral trash of modern manuals, and devoted more attention to the imperishable records of their experience which have been left by practical surgeons, you would not require me to tell you that M. Deschamps, long ago, distinctly explained how this deceptive effect is produced,—namely, by the distended bladder elongating its neck, and forming a sort of pouch at the lower part. Had I suspected that you were not all well acquainted with this important fact, I should have endeavoured to explain it, and now that there seems occasion for doing so, I may read what the excellent authority just mentioned has said upon the subject.

TUMOUR OF THE SOFT PALATE.

I lately showed you a fibrous tumour, about the size of a small marble, which I removed from the neighbourhood of the uvula, in a young female patient, who came from the country to the hospital for this operation. It was performed easily, without perforation of the velum, or hemorrhage; but a few days afterwards was followed by bleeding to the extent of a few ounces. She has suffered no inconvenience since, and went home on Saturday last.

OBSTRUCTION OF THE HUMERAL ARTERY FROM VIOLENCE.

It is now three weeks since I called your attention to the case of a young man, who had sustained an injury of his arm, by falling from a ladder, or rather folding-steps, on which he was standing while cleaning a lamp that hung in the staircase of a house in which he was one of the servants. He struck the inner side of his arm, at the elbow, on an iron rail, with so much force as to bend it, and was recommended to my care by Dr Simson, on account of the swelling and discolouration which soon appeared, together with the want of pulsation which he remarked at the wrist. I stated to you my first impression, that the internal coats of the humeral artery had been ruptured, and expressed my expectation that there would be simply obliteration of the vessel, without the formation of an aneurism. In both respects the result has verified these views, and the patient was dismissed yesterday with no remaining inconvenience, except a hardness still perceptible in the situation of the artery at the injured part.

HYDROCELE.

The young man who now presents himself was lately treated with success by injection for hydrocele of the left side, and now suffers from a collection of fluid, together with enlargement of the right testicle. I have not found a moderate degree of chronic swelling in the testicle prevent the good effects of injection with iodine, but in this case fear that there is rather more of such an obstacle than could be reasonably expected to yield under the absorbent influence. Therefore, before proceeding to treat the hydrocele, I will now pass a full-sized bougie, which has certainly a remarkable power in removing simple swelling of the testicle,—with reference to which I beg to recommend your perusal of the late Mr Ramsden's, of Bartholomew's Hospital, treatise on hydrocele, &c. The case of hydrocele which was last under treatment, and which, you may recollect, was rendered somewhat remarkable by the circumstance of having been tapped sixteen times previously, very speedily recovered after injection, and was dismissed on the seventh day. I met the other day with a case in private, which may be mentioned. The patient came from the country, for the purpose of having injection performed for a hydrocele of ordinary size and form. I tapped it in the usual way, and evacuated several ounces of straw-coloured fluid; but found that a swelling of equal size to the one removed still remained, and therefore punctured it, after

injecting the other, with the effect of drawing off a similar quantity of fluid, perfectly colourless and slightly turbid. By these characters I recognised a spermatocele, and found this diagnosis confirmed by the microscope, which exposed to view a multitude of spermatozoa. Nevertheless, the injection was performed, with a satisfactory result.

The distinction of spermatocele from hydrocele on the ground of spermatozoa exhibited by microscopic examination, affords an example of refinement in pathological research leading, not to improvement, but to an error of practice. It was established upon this ground, as a principle of practice, that the treatment of hydrocele should not be extended to that of spermatocele; and I taught this principle until, by accident, having treated what turned out to be a spermatocele as a hydrocele, I found that it yielded to the same means of remedy. In many cases since then I have not scrupled to undertake the radical cure, and in every instance with success.

The operation for hydrocele, which you see practised here, is, I think, fairly entitled to the character of perfection in regard to simplicity, facility, and efficiency. It is executed by injecting a teaspoonful of the tincture of iodine, by means of a common sixpenny pewter syringe, which answers the purpose not only as well, but, in my opinion, much better, than the expensive and fragile toy that would be put into your hands, if you were to seek the assistance of a London instrument-maker. To appreciate the advantages of this most simple procedure, you should be acquainted with the newest fashion of Parisian practice, promulgated about six weeks ago, not by any obscure or doubtful authority, but by the medical inspector-general of the French army, surgeon of the great hospital of the Val de Grâce—M. Baudens.[1]

This distinguished gentleman begins the process by preparing his patient, through means of suitable diet, baths, and purgation. He then employs a sort of pasteboard telescope, and candle, to ascertain the position of the testicle, and next punctures the hydrocele with a lancet, to let a sufficient quantity of fluid escape for rendering the bag so flaccid that a portion of it may be compressed between the operator's finger and thumb. Through this he thrusts the trocar, bringing its point fairly out on the side opposite to that on which it enters. He then takes out the trocar, and leaves the canula, which, having a hole in its side, allows the remaining fluid to escape. The canula being secured in its place, air is blown into the cavity of the tunica vaginalis, and retained by a plug. This process is repeated three or four times, with the interval of as many hours. Next day, another series of injections of water takes place,—each being preceded by an insufflation of air, to clear the passage. On the day following, a third course of the same kind is executed, with the difference of wine, or a solution of nitrate of silver, being the agent employed; and so on until the requisite degree of excitement is induced. In one case, gravely related by the author, air, water, wine, and nitrate of silver, were injected during the first four days, and an injection of tincture of iodine, in the ordinary way, on the fifth, at which period the patients treated here are usually dismissed from farther restraint. If this proposal were merely puerile and absurd, or useless and inexpedient, it would hardly deserve notice; but being, as I believe, not free from the danger of serious consequences, if applied to the irritable systems of patients treated in private practice, it merits, in my opinion, the most unqualified condemnation. You should beware of supposing that that which is newest is also the best, and may see, from what I have just stated, that deference to mere date or name is an unsound foundation for professional opinions. It used to be thought essential for the completion of medical education, that a season should be spent in Paris. I am not sure how far an abstract desire of professional information may have always led to the representations for this purpose, but think it right to tell you, in order that your present opportunities may not be misspent, what sort of improvements you are to expect, in the event of going abroad in search of them.

[1] Gazette Médicale de Paris, December 28, 1850.

LECTURE ON THE PROGRESS RECENTLY MADE IN THE ANATOMY OF THE CIRCULATING SYSTEM,—DELIVERED IN THE HALL OF THE EDINBURGH COLLEGE OF PHYSICIANS, ON 8TH FEBRUARY 1851, BY PROFESSOR GOODSIR.

MR PRESIDENT,—When requested by you to read a lecture "On the Progress which has recently been made in the Anatomy of the Vascular System," I undertook the duty with much pleasure, not only because the subject proposed indicated, on the part of the College of Physicians, a continued interest in the progress of that science to which many of its Fellows have so largely contributed, but also because I felt that it would afford me an opportunity of bringing under your notice the important share which the labours of a former distinguished member of your body has had in the advancement of the particular department of anatomy which I should have to discuss.

I am afraid, however, that this duty, which was undertaken with pleasure, may not be so satisfactorily performed; and that the mass of materials at my disposal would require a more practised historian to do them justice. Believing, nevertheless, that the history of a science, like the history of a country, or the record of a life, should not consist of a mere detail of facts and opinions, but should, if attempted at all, aim at the elucidation of the influences which have encouraged, the causes which have induced, and the principles which have regulated, each step in its progress, I shall endeavour to make such a use of my materials.

The history of the anatomy of the vascular system in animal bodies divides itself into three periods, the third of which is now in progress.

The first period is the epoch of the opinions of the ancient anatomists.

The second period is the epoch of Harvey, whose discovery was developed by Malpighi and his successors.

The third period is the epoch of John Hunter, whose doctrine is now rapidly extending through the labours of Milne Edwards and his school.

During the first period, the vessels and the blood were supposed to derive their origin from certain glands,—from the liver or the lungs,—and to be distributed from thence to the different parts of the body; the blood passing from the extremities of the vessels into the areolæ of the "parenchyma," of which the textures and the viscera were conceived to be principally composed.

With these opinions were conjoined the notion of the vital spirit derived from the lungs, or from the right side of the heart, through the septum; and conveyed from the left ventricle into the arteries.

In reviewing these vague and disconnected conceptions, we cannot but be struck with the felicitous guesses with which they are interspersed. So true is it, that a conception, sufficiently correct to be the basis of successful research, may remain barren for centuries, because the epoch in which it occurred may not be prepared to examine into and develop it.

In the second period, we perceive the influence of that remarkable tendency which impelled the cultivators of science, in the seventeenth century, to rest satisfied with no conception, however ingenious, if it did not at the same time include all the results of experiment or observation in a connected and intelligible form. Under the influence of this peculiar tendency, which must ever continue to guide the true cultivators of science, there resulted at the commencement of the period we are now considering the discovery of the circulation of the blood. Harvey obtained this result principally by including the observations of his predecessors in a hypothesis of his own, which not only admitted what was true, but excluded what was false, and suggested new proofs of the entire theory. This great man made most of his experiments and dissections, after he had satisfied himself of the existence of the circulation. He undertook them for the purpose of convincing others.

Harvey was not, however, acquainted with the mode in which the blood passes from the arteries into the veins. He retained the ancient hypothesis. He sup-

posed that the fluid passed from the one set of vessels into the other through the "parenchyma," permeating its areolæ; or that the arterial blood entered into the composition of the textures, to replace the matter which had been removed from these textures, under the form of venous blood. The conception which throughout guided Harvey was merely a continuous circulation of the blood, induced by the peculiar structure and arrangement of the heart and vessels.

This great discovery characterises the second period in the history of our subject; and the followers of Harvey were occupied during the rest of the period in developing the principle, and following it out in other departments of the science. Of these the most remarkable was Malpighi, by whose microscopic observations the direct passage of the blood from the arteries into the veins was first firmly established, and the ancient hypothesis of the "parenchyma" exploded; until, under a new form, it is again maintained in our own times.

Although Malpighi verified and completed the Harveian discovery, he cannot be considered as having laboured so much for the period in which he himself flourished as for that which succeeded his own. While his cotemporaries, and particularly Ruysch, were busily engaged, by means of injections and other anatomical procedures, in extending the knowledge of the minute vessels, and were falling into various errors in their attempts to explain all the functions of the economy by vascular action, Malpighi, who was a comparative anatomist, and the founder of the science of vegetable anatomy, possessed too philosophical an intellect, and took too extended a view of his subject, to be led astray even by his own discovery. He allotted to the capillary vessels, through which he had seen the blood passing, a secondary function in nutrition and secretion; and by detecting the general structure and action of the glandular acini, anticipated by nearly two centuries the most important departments of modern physiology. Like every investigator who works as the pioneer of a period posterior to his own, his most important results only received the credit they deserved after they had been illustrated by the discoveries of modern observers.

Although the belief in the existence of open-mouthed or exhalent vessels—a remnant of the ancient doctrine of the "parenchyma"—continued more or less throughout this second period, and was maintained by Haller, Hewson, and even by Bichat; yet the influence of the Hunterian school, and the writings of the profound and accurate Prochaska, finally removed all doubt as to the continuity, in the higher animals, of arteries into veins by means of capillaries, without lateral or terminal orifices.

In proceeding to the third period in the history of our subject, it must be borne in mind that Harvey and his followers had discovered, and fully proved, that in the higher animals, at least, the blood circulates continuously in a system of perfectly close tubes. Numerous observations, which have been lately made, satisfactorily prove, however, that although the circulation of the blood takes place in the higher animals in the interior of close vessels, yet in a large proportion of the invertebrate animals, and probably in all of them which possess a vascular system, this system, over more or less of its extent, does not consist of close tubes, but of areolæ or lacunæ, destitute of appreciable walls, and consisting apparently of spaces interposed between or enclosing the organs; and extending, as interstices, among the laminæ, fibres, and cells, which constitute the textures. These lacunæ communicate with one another freely, and receive the blood in one direction from the arteries, and empty themselves in the other into the veins. The organs and textures float in, or are bathed by, the blood.

As the invertebrate animals, in which this kind of vascular system has been discovered, much exceed in number and variety of type those of the vertebrate sub-kingdom, we are led to suspect that a circulatory system of close vessels is the exceptional form, and that it occurs only as an ultimate development in the higher animals.

The investigation of this more generally diffused and lower type of vascular system is now advancing, principally by the exertions and under the guidance of M. Milne Edwards and his pupils.

The discovery was made, however, and the general fact established in two great sections of the invertebrate animals—the insects and crustacea—by Mr Hunter, two-thirds of a century ago.

The observations and dissections of Swammerdam and Malpighi had, indeed, previously established the nature and general structure of the dorsal vessel in the insect, and the remarkable manner in which the external air is conveyed, by means of ramifying tubes, to all parts of the body in this class of animals. They did not, however, recognise the real nature of the vascular system.

Mr Hunter's claims to this discovery are based on a series of injected preparations of the insect, and on a set of highly finished drawings, with descriptions of the organs of circulation in the lobster, preserved in the Museum of the Royal College of Surgeons in London. The preparations from the insect are eight in number, and illustrate the organs of circulation in the silk-worm, larva of the puss-moth, and in the humble bee. Mr Hunter states, that in these animals "the heart consists of one cavity, which is a systemic ventricle, or distributes the blood to the general system." "The circulation of the blood," he observes, "is, in the insect, in itself very simple; yet, from being very familiar with the most complicated,—viz., that of the *Tetracoilia*,—it at first might seem complicated. Insects cannot be said to have either pulmonary arteries or veins, having but one simple circulation. Whatever blood is sent out from the heart of the insect is prepared blood, therefore the arteries serve the purpose of the aorta and the veins, both of the veins in common and of those called pulmonary; for the common veins have the blood prepared in them, serving the purpose of pulmonary veins."

"The veins of the insect would appear to be simply the cellular membrane; but they are regularly formed canals, although not so distinctly cylindrical canals as in the quadruped, &c.,—not branching with that regularity. They would appear to be, or to fill up, the interstices of the flakes of fat, air-cells, muscles, &c., and therefore might be called in some measure the cellular membrane of the parts."

Mr Hunter adds another observation, which would be perfectly unintelligible except by the light of recent discovery, and which proves how clear and steady a conception he had of the vascular system of the insect. He says:—"I conceive that there is a great regularity in the vascular system of the insect, although it is not easily unravelled." He did not commit the mistake, which Cuvier long afterwards fell into, of conceiving the blood as stagnating in the areolæ, the air passing on to the blood, instead of the blood meeting the air; but he understood the manner in which they both circulated, and he felt that the blood must move in a definite and precise course through the lacunæ, as has been, since his time, demonstrated by Cazus and others.

In Mr Hunter's introduction to his description of the venous system of the lobster, he states, "that the veins in this class of animals, as in the winged insect, &c., are principally in the form of large irregular cells, as if the cellular, or investing, membrane of the animal contained the venal blood; and when injected, we find the injection principally in large masses."

This account of the structure of the venous system in the lobster has been confirmed by the subsequent and independent dissections of Lund and Straus Durckheim.

Many years later, Cuvier described the vascular system of the insect as altogether anomalous. He not only considered the blood as stagnant in this class of animals, but conceived it to be so, in consequence of the general diffusion or circulation of air through the trachea. He was, consequently, compelled to deny that the dorsal vessel performed the functions of a heart; and thus increased the obscurity which had hitherto involved this most important subject.

In his great work on the "Anatomy of the Molluscous Animals," Cuvier recorded, however, one curious instance of a disposition of parts resulting from the lacunary vascular system in *aplysia*. In his memoir on the *aplysia*, he described and figured the cardiac extremities of the great systemic veins or branchial arte-

ries, as destitute of continuous walls, communicating by numerous openings with the general cavity of the abdomen. The communication between these vessels and the visceral cavity is so little in accordance with the structure of the higher animals, that Cuvier was long in doubt regarding it; and for some years after he had communicated it to the Institute, he delayed the publication of his memoir, under the impression that he might have been misled. Having afterwards examined many of these animals with great care, he came to the following conclusions:—

1. That there are no other vessels for conveying the blood to the branches but the two venæ cavæ, pierced with numerous holes.

2. That all the veins of the body empty themselves mediately or immediately into these perforated tubes.

3. That the communication between these vessels and the abdominal cavity is evident and palpable; and that, as they convey the blood to the branchiæ, the fluid contained in that cavity must be mixed with the blood which passes into these organs.

4. The veins must perform the office of absorbent vessels.

5. This structure is the first step towards the still more remarkable arrangement in insects in which no vessels exist for the nutrient fluid.

In 1832 Mr Owen published his monograph on the "Pearly Nautilus," which was followed by that of Valenciennes on the same subject, in which these observers announced that the vena cava in the nautilus is perforated, and communicates with the general abdominal cavity.

In 1839 Milne Edwards communicated to the Academy of Sciences a memoir on the composite and social ascidians, in which he described the lacunary vascular system, and the reciprocating circulation of these animals.

He has more lately been engaged in the examination of this system in the mollusca generally, with the following results:—

1. The vascular apparatus is not complete in any mollusk.

2. In certain portions, more or less considerable, the veins are altogether wanting, and are replaced by lacunæ, or by great cavities in the body.

3. That the veins are frequently altogether deficient; and that, in these instances, the blood conveyed to all parts of the body by arteries, returns to the respiratory surface by a system of lacunæ.

4. That in some of these animals the arterial system is also more or less deficient, and presents a lacunary arrangement.

The distinct manner in which the lacunary arrangement of the vascular apparatus is exhibited in the crustaceous insects and mollusks, indicates the morphological relations of corresponding structures in other organised bodies. In the simplest organic form, or one-celled animal or plant, we perceive only the cavity of the cell. In a compound cellular form we have presented to us the cavities of the several constituent cells, as in the first form; and, in addition, such an arrangement of continuous or interrupted intercellular spaces, as the particular collocation of the cells in the given form may exhibit. However complicated the structure of an animal or plant may be, its various cavities, whether they be tubes, sacs, lacunæ, areolæ, communicating or isolated, simple or complicated, are referable to one or other of these two, the cell-cavity or the intercellular space. Cell-cavities are generally isolated, as in gland-cells; or communicating, as in certain ducts and capillary vessels. Intercellular spaces are much more varied and complex in their forms and arrangements, and exhibit every variety of isolation and connection. The most important intercellular space in the animal is the alimentary canal, which, in its most complicated and perfect form, is from mouth to anus a mere vacuity in the midst of the epithelial particles which form its walls, and which branches off in the various gland-ducts which open into it. But the most extensive and complicated intercellular space is the cavity of the vascular system, including that of the lymphatic vessels, which, communicating with one another, are lined by a continuous layer of cellules, the free surfaces of which bound the space which contains the blood and lymph. The cavities of the

respiratory and reproductive organs, the serous, synovial, and bursal sacs, are in like manner spaces of a similar constitution.

As all such intercellular spaces are limited and defined by the approximation of contiguous cells, it is evident that we may conceive their walls as exhibiting more or less distinctly the spaces between these cells; and, therefore, as presenting an arrangement which, by a slight development, would open up a passage into adjoining spaces or cavities.

By applying this principle, Milne Edwards conceives the continuous digestive and vascular systems of the medusæ and other invertebrate animals, to result from a continuity of certain of the intertextural spaces. He explains the separation of these two systems in animals higher in the scale, as in the bryozoa, by an extreme diminution in the size of the certain lacunæ cutting off the communication. The view he takes enables him to see how, in the insects and inferior mollusks, a true vascular system begins to show itself, in the form of a heart or incipient artery, produced by a condensation of the walls of one or more of the lacunæ.

We can understand, in the same manner, how, in the crustacea, arachnida, brachyopoda, and higher mollusks, the arterial and venous system is more or less developed; in all presenting, however, more or less of the lacunary structure, of which it appears to be only a modification.

From this mode of viewing the subject, M. Milne Edwards concludes, that in the vertebrate animals and the human body, in which the capillary vessels are fully developed, this is effected by a process similar to that by which the separation of the vascular from the digestive systems is produced in the lower invertebrata. The cavities of the capillaries are shut off from the textural lacunæ by a diminution of the intermediate lacunæ; so that the vascular system in the higher animals is only relatively closed, allowing the passage of particles of a certain size, through minute lacunary spaces, to and from the textural areolæ.

From these conclusions you will perceive, that M. Milne Edwards has endeavoured to establish the existence of communications between the cavity of the vascular system in the higher animals, and the cellular areolæ or intertextural spaces. But, although it must be admitted in the abstract that, as the coats of the vessels consist of agglutinated cellular elements, and therefore may be conceived as retaining the lines and surfaces of junction of these elements, in the form of fissures or openings; yet no one who has examined these structures with modern instruments, either in the dead or living animal, can for a moment hesitate as to their impermeability, except in a molecular sense. Besides, M. Milne Edwards has overlooked the existence of the lymphatic system in the vertebrate animals,—a most important and remarkable part of their vascular system. No theory on this subject can be satisfactory which does not involve in its scheme the absorbent vessels, and which does not afford a consistent explanation of their peculiar relations to the red blood vessels.

In a paper "On the Supra-Renal, Thymus, and Thyroid Bodies," published in 1846, I made the following statement:—

"During the first stage of the development of the animal ovum, digestion and respiration—the absorption and preparation of nutriment—are carried on by the blastoderma, a structure consisting of nucleated cells and of vessels.

"The cells, of which the blastoderma consists, are the progeny of that previously occupying the germinal spot of the ovum, and are continually reproduced and increased in numbers by the production of others from the nutritive centres, or secondary germinal spots distributed over it.

"Materials for the nutrition of the blastoderma are derived from the subjacent yelk. The matter resulting from the solution of a certain number of the secondary blastodermal cells—that is, of the progeny of the primary blastodermal cells, or *nutritive centres*—is employed as the nutrient matter by the remaining secondary or proper blastodermal cells. In this way 'pabulum' is afforded for two purposes,—the growth of the blastoderma, and the growth of the embryo itself.

"During the early period of the existence of the blastoderma, before the circulation has been established, the product of solution of the elder is at once absorbed by the younger cells. During the later periods, the product of solution drops into the incipient loops of the blood-vessels, and so circulates for purposes of nutrition. This is an instance of primary lymphatic absorption, and differs in no essential particular from the same process in the animal further advanced. We may consider the blastoderma, in fact, during the first period of its circulation, as containing very numerous lymphatic ducts, instead of a few, as in the more advanced animal.

"In the blastoderma, the process by which nutrient matter passes into the circulation, or the act of absorption, as it is usually called, is reduced to its most simple form, being contemporaneous and also identical with the formation of the imperfect capillary network. In the more advanced animal, when the capillary network is consolidated, the product of solution of the textures passes or drops into the intercellular or textural lacunæ, which appear to be the radicals of the lymphatic system,—a system which in the adult communicates with the blood-vessels only at a few places in the neighbourhood of the trunks of the original blastodermal veins."

To put this process in a clearer point of view, permit me to remind you of the mode in which vessels are developed in the germinal area. Certain of the cells of that structure send out processes, which approaching, ultimately communicate, and form a vascular network. The contents of each of these cells pass into the general cavity of the vascular system by an absorption of the wall of the cell, and thus form an addition to the general mass of blood. This is a process which not only involves the formation of blood, but also the development of the vessels through which that blood circulates; and, at the same time, removes from the spot all that matter which is not required as pabulum for the more persistent structures. The process involves, therefore, also the function of the lymphatic system—that is, the act of conveying away, into the general circulating medium, all the matter not required by the texture at the spot. The combination of the functions of circulation of blood, and of lymphatic absorption, must necessarily be discontinued as soon as the capillary network has been completed; and another channel must be provided for the conveyance of the debris of the textures, and of materials for the formation of blood into the general current of the circulation. This appears to me to be effected by a very simple modification of the primary process. The cells which continue to break into the cavity of the vascular system no longer do so in the same manner. They communicate only with one another, and with the venous system, so that their contents passing forwards into the veins, and no longer mixed with blood from the arteries, consists only of lymph—that is, of the debris of cells or textures which no longer finds access to the capillary vessels. If this hypothesis should be found to involve the plan of formation of the lymphatic system of vessels, it indicates the direction in which this system is developed. It points to its growth from the venous extremities of the thoracic ducts backwards; and to the formation of new portions of lymphatic vessels, as fresh masses of texture require additional drainage for their effete materials.

As long as the act of lymphatic absorption continues to be part of the process by which new blood vessels are formed, each cell, as it breaks into the vascular system, unites the remaining portion of its wall to the wall of that system. But as soon as the red blood vessels of a part are completed, the textural elements which are about to pass into the mass of blood, no longer required to afford a portion of their structure for the formation of these vessels, are now made use of in building up the lymphatics. The lymphatics do not appear to possess capillary radicals. The intertextural lacunæ or areolæ would appear to constitute the widely-extended root of this system of vessels. Admitting this to be correct, then the intertextural spaces throughout the body must be considered as an extension of the general cavity of the vascular and lymphatic system. The textural extremities of the lymphatics, as the parts of the vascular system last

formed, and the portions of that system which perform the simplest function, exhibit the nearest approach to primary structure. In the embryo, before the appearance of the blood vessels, in animals, or parts of animals, which do not possess these structures, the nutrient fluid flows in intercellular or textural spaces; and this primary form of vascular cavity appears to constitute the radical portion of the lymphatic system.

The origin of the lymphatic vessels from the intertextural spaces appears to be confirmed by the gradual modifications presented by the system in the descending scale of the vertebrata. These vessels begin to assume a dilated or varicose aspect, covering and surrounding the various parts in a cellular or areolar manner. In the reptiles, but especially in fishes,—as described and represented by the second Monro in his work on fishes,—they break up, or give place to more or less extended sacs or great areolæ, which surround and enclose various parts, which appear to be bathed in the lymph through which they pass. Thus we perceive in the vertebrata an arrangement apparently similar to that which we have already considered in the invertebrata. In the former, the parts are probably bathed in the lymph; in the latter, they are in contact with the fluid which in these animals represents both blood and lymph.

You will now perceive how the explanation I have given of the mode of origin of the lymphatics in the germinal area, and its relations to the arrangement of these vessels in the four classes of vertebrate animals, rectifies that part of M. Edward's theory in which it fails to explain the existence and connection of the absorbent with the red blood-vessel system of the higher animals. You will also observe, that a lacunary system exists in the vertebrate as well as in the invertebrate animals; but that in the latter it is amalgamated with the ordinary vascular system, as in the early stages of the germinal area; while in the former the two systems are connected only by the medium of the thoracic ducts.

The colourless blood of the invertebrate animals must be considered, as Wagner has remarked, only as chyle or lymph. It has been arrested in its development, in harmony with the lower range occupied by the animals in which it circulates. It represents, therefore, potentially the white and coloured nutrient fluids,—the lymph and blood of the vertebrate animals; while the heart, vessels and lacunæ of the invertebrata consist, in like manner, of ordinary vascular and absorbent system of the higher class, not yet distinguished from one another in the development of the series.

Part Fourth.

MEDICAL NEWS.

EDINBURGH MEDICO-CHIRURGICAL SOCIETY.

MEETING III.—*January* 15, 1851.—Dr BEGBIE, President, in the Chair.

On taking the chair for the first time, *Dr Begbie* returned thanks to the Society for having elected him to act as their preses. He briefly indicated the modes by which the objects of the Society could be best promoted, and in which he would, like former presidents, endeavour to co-operate with other members.

Dr W. T. Gairdner read a communication on the terminations of pericarditis, especially adhesion of the pericardium, with illustrative cases. (This communication has appeared in a more complete form in the February Number of the Monthly Journal.)

Dr Begbie read a communication on fatty degeneration of the heart, with reference chiefly to some points in diagnosis. In the course of this communication, he detailed with considerable minuteness all that had been ascertained regarding the cause of sudden death in the cases of the late Dr Abercrombie and of Dr Chalmers. (This communication appears in the present Number.)

Dr Maclagan and *Dr Christison* related cases illustrative of the subject of Dr Begbie's paper. In one of these—a gentleman of seventy-five—sudden death had occurred after about an hour's illness, in the midst of health to all appearance perfect. Dissection revealed rupture of the coronary vein, and other appearances much resembling those recorded by Mr Goodsir, in the case of the late Dr Abercrombie.

Dr Alison expressed his opinion, that the too low regimen to which Dr Abercrombie subjected himself, after the evacuations which his case at one period seemed to demand, had very probably contributed to induce the subsequent fatal degeneration of the heart.

Dr Bennett had observed the fatty heart very frequently in the course of post-mortem examinations, and believed the degeneration was very commonly connected with the causes of death in these cases. Probably some of those cases in which death was attributed to spasm of the heart, admitted of a more satisfactory explanation, if ascribed to this peculiar degeneration of the muscular structure of the organ.

Dr Spittal and *Dr W. T. Gairdner* also joined in the discussion.

Dr Richard J. Mackenzie read a case, in which an operation for the division of stricture of the urethra by external incisions had been followed by death. (This communication appears in the present Number.)

Mr Syme had not scrupled to recommend the division of strictures upon a grooved instrument passed through them, as a surgical procedure equally safe and effectual, and as the best mode of treatment that could be applied when the stricture was found to be very obstinate and contractile. It was to such cases that he had proposed to limit the operation. He had now performed it in University College, London, in Minto House and the Royal Infirmary, Edinburgh, and in private practice, thirty-five times, and that without a single untoward result. A more thorough test of the safety of an operation could hardly be expected. In establishing an improvement in surgery, proposed innovations were not protected by a patent, as was usual when discoveries were made in the arts. The merits of operations must be tested by the experience of the profession. If this was favourable, no unscrupulous efforts to decry the practice which he had originated would be found effectual. As to the unfortunate issue of the case which Dr Mackenzie had very properly brought under the notice of the Society, it must be regarded as quite exceptional. He asked, if the congested spot on the mucous surface of the bladder, which had been mentioned by Dr Mackenzie as probably due to the contact of the catheter, could, in his opinion, be connected with the fatal event?

Dr Mackenzie replied, that at the point in question there was no abrasion; and the amount of congestion was so trifling that he should not have alluded to it, but for his anxiety that every particular regarding the case should become public. He pointed out the spot upon the preparation.

Mr Syme was satisfied that no fault could be found with the operation which Dr Mackenzie had performed. The question which he had asked was suggested by unpleasant consequences which he had witnessed, when a catheter had been suffered to remain in the bladder in an improper position.

Dr Mackenzie stated that, as the urine escaped freely by the catheter at all times after the operation, as the patient suffered no inconvenience from its presence, and as no injury to the bladder could be seen on dissection, there could be no connection between the death of the patient and a faulty position of the catheter.

Mr Goodsir asked, Whether pus was found or looked for among the pelvic

lymphatics? The train of symptoms detailed, and the analogy of uterine phlebitis, rendered it very probable that pus might have existed in these vessels.

Dr W. T. Gairdner had inspected the parts most carefully, squeezing out and examining the juices from all the tissues in the neighbourhood of the bladder, but had seen nothing resembling pus.

ROYAL MEDICAL SOCIETY OF EDINBURGH.

On the 14th February, at a meeting of this Society, which has formed a prominent feature of the medical school of Edinburgh since 1734, a large number of the honorary and extraordinary members, as well as of those on the attendance-roll, were assembled to consider an offer of the managers of the Royal Infirmary to purchase the Society's Hall. Most of the medical professors and extra-academic lecturers, and many of the principal medical practitioners of the city, were present, together with Professor Forbes, one of the honorary members, and Dr Conolly of Hanwell, who had come to Edinburgh, at the request of his friends in England, to sit for his portrait to our eminent artist, Sir John Watson Gordon.

Dr Alexander Christison, the president of the evening, briefly mentioned the steps which had been taken since February of last year, by a committee of the Society, regarding the sale of the hall; and stated that the managers of the Infirmary had now made offer of the sum of L.1700, with possession of the hall by the Society for five years, in case that period might be found necessary for providing a new building; and that the committee recommended unanimously that the Society should accept so liberal an offer.

Professor Christison moved the adoption of the committee's report. He observed that it had become a matter of necessity that the Society should remove to a more eligible situation. Many years ago they had been anxious to do so, but were unable for want of funds. At length they were about to be hemmed in altogether by the Infirmary improvements; the access had become intolerable; and there was no longer space for their admirable library. It was fortunate, in these circumstances, that another body was equally interested in their removal. The managers of the Royal Infirmary, who, since the institution of the Society, had repeatedly befriended them, now made a liberal offer, which, along with other resources, would enable them to build a new hall on a much better site, and with all modern conveniences. No one could doubt that a large sum, in aid of the building fund, might be raised by a subscription among the extraordinary members, now scattered over the whole world, and constituting a great proportion of the practitioners, not merely of this city, but likewise of the country towns, as well as the metropolis of the empire, and who looked upon the Society with affectionate regard as the scene alike of instruction and happiness, when they were students of the University and ordinary members. As the sum, however, which would be thus raised, might not prove sufficient, it would be advisable to make the accumulated fund, now amounting to L.1300, available for the purpose to the extent of L.600. There could be no more appropriate application of a portion of a fund which had been set aside by their predecessors expressly for "repairing, altering, and improving the hall." It was true that the interest of the fund had some years ago maintained the efficiency of the Society, and saved it from debt, when the income from other sources fell short of the ordinary expenditure; and a part of it should still be reserved for annual repairs. But the sum proposed to be made available might be safely withdrawn if found necessary. They must commence with a certainty of being supplied with the requisite building-fund. It was to be hoped that the little capital of the Society might not be seriously encroached on. But although it were to be so, the Society might trust to a considerable accession of new members as soon as the hall was placed in a respectable site, and with full accommodation for the library, now amounting to 14,000 volumes, selected with a care unexampled in any other institution. And should a day of adversity ever again reach it, they ought to trust, as they had

done in former times, and as they might safely do on the present occasion, to the liberality and affection of their extraordinary members.

Dr Christison concluded with moving that the sale be effected in terms of the offer of the managers of the Infirmary; that the committee be instructed to carry this resolution into effect, and empowered to purchase a site for the new hall, and obtain plans for the Society's consideration; that a subscription be set on foot among the members and friends of the Society; and that the accumulated fund be made available for the new hall to the amount of L.600.

Dr Conolly expressed his hope that, as a member of more than thirty years' standing, and one who had taken an active interest in the business of the Society while he was an attending member, and had derived from its debates much valuable instruction, he might be allowed the pleasure of seconding the motion. At the time to which he referred, the Society's hall was surrounded by famous schools of anatomy, and by the hall of the College of Surgeons, which had been all gradually removed. The site and access were now greatly deteriorated; and the state of the hall itself showed that they had not in this respect advanced with the times. Other public institutions had raised edifices of architectural beauty, which everywhere adorned this unrivalled city; and he hoped, if he should be spared to revisit Edinburgh, that he should find them too in a new hall, with more architectural pretensions, and situated in a more desirable locality. As regards a subscription, he was quite sure that the many members now scattered throughout England and over the whole world, would gladly contribute towards the prosperity of a Society which they all looked back to with feelings of attachment, respect, and veneration.

Dr Andrew Wood was quite of opinion with Dr Christison and Dr Conolly, that it was very desirable for the Society to remove from its present hall. But it was much better for it to remain, than to move at the risk of sacrificing its accumulated fund. That fund had been the means of saving the Society from ruin or diminished usefulness at no distant date, and there might again arise years of depression when they might seriously miss so valuable a resource. He would recommend that the managers of the Infirmary, who had already shown great kindness to the Society, should be asked to give them a few months to reconsider the matter; and that, in the interval, the committee should ascertain what amount could be raised by subscription. He therefore moved as an amendment, that the accumulated fund should not be encroached on for the purpose of building a new hall, and that the offer of the managers should not be decided upon until November next; and that the necessary steps be taken for obtaining a subscription and looking out for sites for a new building.

Dr Wm. S. Carmichael seconded the amendment.

Professor Syme urged the immediate acceptance of the offer of the managers. After many delays they had got a definitive offer, which ought to receive a definite answer. All agreed as to the necessity for moving. No one could doubt the liberality of the offer made them. The adjoining house was bought by the managers for L.600. Their own hall was now not worth L.400 to any body except the managers, to whom it was valuable on account of important servitudes which they required to obtain possession of. He deprecated delay, and had no doubt that the requisite funds would be forthcoming.

Professor Balfour felt so strongly that the Society had owed its salvation a few years ago to the accumulated fund, as to be opposed to any measure which would appropriate any material portion of it, and would therefore support the amendment.

Dr Spittal took the same view, saw no occasion for immediate action, and counselled delay.

Dr Alexander Wood thought it was unfortunate that the question of the sale of the hall had been mixed up with that relative to appropriating the accumulated fund. The latter was a more doubtful measure than the former, and not necessarily connected with it. As to the former, they had a distinct and substantive proposition, to which they should return a corresponding answer; and

therefore, if the first part of the motion were put as a separate one, he would heartily concur with it, and vote for acceptance of the managers' offer.

Dr Christison said, in explanation, that he had contemplated nothing else than that the several clauses of his motion should be put separately. The laws of the Society indeed rendered this necessary. They could not sell the hall without a very peculiar form and condition, and then, nevertheless, a simple majority was enough; but they could not touch the accumulated fund without the consent of three-fourths of the meeting.

Dr Douglas Maclagan was averse to any delay, and supported the motion, with the understanding that the several clauses were to be put *seriatim*.

Professor Bennett said there appeared to him to be simply two propositions before the Society,—one of great boldness, and another of extreme caution. He was decidedly in favour of the bold one. He felt assured that all the old members of the Society would gladly come forward with liberality on such an occasion, entertaining, as they did, such grateful recollections of a Society from which they had in their day received so much benefit, and in which he for one could say, he had acquired much of his medical education. He was certain that, if the members went forward with the determination of succeeding, and prepared to encounter difficulties, they would overcome them all, and ultimately prove triumphant.

Mr J. F. Macfarlan, as Treasurer of the Society, had a great regard for the accumulated fund, and when he considered that it had already been a refuge in the day of adversity, and might again be so, he sincerely hoped that the Society would not seriously trench upon it. Indeed he felt confident this would not be necessary. He would, in reliance upon the gratitude and liberality of the extraordinary members, advise the Society to close at once with the liberal offer of the managers. One advantage from that measure would be, that the Society would be in an equally favourable condition with the celebrated conqueror who, on landing in a new country, burnt his ships, so that there might be no retreat.

It was then agreed that the first part of the motion should be put, viz., that the Society approve the committee's report, accept of the offer of the managers, and appoint the following committee to carry the sale into effect:—ORDINARY MEMBERS. *Presidents*,—Mr Wolley, Dr Alexander Christison, Mr Sanderson, Mr Murchison. *Curator*,—Mr Maclagan. *Secretaries*,—Mr Currie, Mr Calder. EXTRAORDINARY MEMBERS,—Professors Christison, Syme, Simpson, Goodsir, Balfour, Bennett; Drs Conolly, Craigie, Simson, Douglas Maclagan, T. Anderson, Newbigging, Alexander Wood, Andrew Wood, Handyside, Donald Mackenzie, J. D. Gillespie, Mackenzie, M'Cowan; Hugh Cleghorn, H.E.I.C.S., — White, H.E.I.C.S.; Messrs Cuningham, Broadbent, Turner; Mr J. F. Macfarlan, *Convener*,—with power to add to their number.

The amendment of Dr Wood being then balloted for, was negatived by a majority of 73 to 20.

The President next intimated that the laws of the Society, relative to alienation of property, required that at least six honorary members, and sixteen ordinary members, of nine months' standing, should concur in such a motion as that of Dr Christison, otherwise the meeting could not agree to it. Thereupon nineteen such ordinary members, and the following honorary members, Dr Alison, Dr Christison, Professor Syme, Dr Simpson, Professor Goodsir, Professor Forbes, and Mr J. F. Macfarlan, declared their concurrence. The ballot was then taken on the motion, which was declared to be carried by a majority of 73 to 10.

The Society then adjourned for public business.

At its next meeting, on February 21, *Dr Christison* moved, and *Dr Andrew Wood* seconded, the second clause—that the same committee be empowered to purchase a site, and to obtain plans and estimates for the consideration of a full meeting of the ordinary and extraordinary members of the Society. This clause was carried unanimously.

Dr Christison then moved, and *Dr Balfour* seconded, the third clause—that a subscription be commenced forthwith among the members and friends of the Society, for an additional fund for building the new hall; and that the same

committee, with Mr J. F. Macfarlan as treasurer, shall superintend the subscription. This motion was also carried unanimously.

The remaining clause, relative to the appropriation of a part of the accumulated fund of L.1300, was postponed, at the mover's request, for consideration at a future meeting of ordinary and extraordinary members, to be summoned before the close of the present session.

VARIETIES.

SCHMIDT'S REMEDY FOR TAPEWORM.

A correspondent, who has noticed the remark in our last number, that we had not been able to discover what was the composition of Schmidt's remedy for tapeworm, recommended by Dr Kerlè, has supplied us with the following account of it. The details are extracted from Most's "Encyklopädie der gesammten Medicinischen and Chirurgischen Praxis" (B. I., p. 992, 2d edition); and both for their own sake, and as presenting a somewhat curious trait in medical history, we give them a place in our pages. The composition of the remedy, our correspondent adds, is also presented, though with less fulness and accuracy, in the "Pharmacopée Universelle" of Jourdan (T. I., p. 313, 2d edition).

By the process of Schmidt, the tapeworm is said to be expelled in from three to five days, without injury to the patient. A trial of it was made, under the authority of government, in the Berlin hospital, when the results were found to be so favourable, that the King of Prussia purchased the secret through a pension of several hundred dollars, bestowed upon the originator. The plan substantiates the purely empirical fact, that vermifuge medicines, when exhibited separately, are less efficacious than when combined in complex formulæ. The patient commences by taking, in the morning, the following mixture:—℞. Pulv. Rad. Valer. min., ʒvi.; Fol. Senn., ʒij.; Aq. ferv., ʒvi.; Infunde et cola. Adde Sal. Glauber. Crystall., ʒiij.; Syr. Mannæ, ʒij.; Elaeosacch. Tanaceti, ʒij., M. S. Two tablespoonfuls every two hours, drinking freely at intervals of sweetened coffee. With this he perseveres till seven in the evening. At noon he takes a thin gruel; and in the afternoon one or two herrings, with the milt. At eight in the evening the patient has a herring salad, with minced raw ham, and oil, sweetened with sugar, plentifully added. Frequently portions of the tapeworm begin now to be discharged. At six in the ensuing morning, he commences the following pills every hour:—℞. Assafœtidæ, Extr. graminis ää, ʒiij.; Gambog., Pulv. Rhei, Jalap. ää, ʒij; Pulv. Ipecac., Digit. Purp., Sulph. Antim. aurat. ää, ℈ss.; Sub. muriat. Hydr., ℈j.; Ol. Tanacet., Æther. Anisi, ää, gt. xv. M. Divide into pills of two grains each, which are to be placed in a well-stoppered bottle; of these, six pills are to be administered every hour, with a teaspoonful of common syrup. Half-an-hour after the first dose, a tablespoonful of castor-oil is also given. Sweetened coffee is again to be taken freely at intervals during the day. At two in the afternoon, the worm is usually wholly expelled, when the pills are to be discontinued. Should only separate portions be voided, however, the remedies are persisted with, and a second dose of castor-oil becomes requisite. Meat soup is allowed at dinner, and in the evening. On the succeeding day, as an additional precaution, six pills are again exhibited at three different times—morning, noon, and evening. Such is the entire and complicated process, the alleged excellent effects of which are held to proceed more from the peculiar combination of well-known remedies, than from the employment of any new agent. Most adds, that this method of cure has been a hundred times successfully tested; and that it presents a warning, especially to young physicians, not to condemn too hastily the complex prescriptions of older practitioners, however much they may appear to offend against scholastic rules, or even the laws of affinity, inasmuch as there is often produced a *tertium quid* of real efficacy, which could not have been developed or found in more simple compositions. We leave it to British practitioners to judge, whether this commendation ought to be sufficient to induce them to give a trial to the singular process—ham and herrings included,

COLLEGE OF PHYSICIANS OF EDINBURGH.—The third of the series of lectures to be delivered in the Hall of the College, will be given by Professor Christison, on the evening of Monday, the 10th March. The subject of lecture will be "Medical Evidence."

KOUSSO.—In a letter addressed to the Editor of the Pharmaceutical Journal, Mr John Savory, of London, states that, having lately received from his correspondent at Aden, a supply of kousso, amply sufficient for testing its virtues, he will be most happy to present quantities of the remedy to physicians of hospitals and charities who may be inclined to give the drug a trial. He wishes its qualities to be fairly ascertained before ordering a larger supply.

ST BARTHOLOMEW'S MEDICINE CHEST.—Between L.200 and L.300 are spent every year for strong sound port wine for the sick poor in St Bartholomew's Hospital. It is bought in pipes, and drawn off as needed. Nearly 2000 pounds weight of castor oil, 200 gallons of spirits of wine, at 17s. a gallon, 12 tons of linseed meal, 1000 pounds weight of senna, and 27 cwt. of salts, are items in the annual account for drugs. The grand total spent upon physic in a twelvemonth is L.2600. 5000 yards of calico are wanted for rollers, for bandaging, to say nothing of the stouter and stiffer fabric used for plasters. More than half a hundredweight of sarsaparilla is used every week—a sign how much the constitution of the patients requires improvement. In a year 29,700 leeches were bought for the use of the establishment—an invasion of foreigners without parallel, till we have the influx of the Great Exhibition—for the leeches brought to bite and die in this London hospital are gathered in France and Poland, in Africa and Spain. A ton and a half of treacle is annually used to make some kinds of syrup; the five casks of hips, which, mixed with a cask of sugar, makes linctus for coughs, have been already mentioned; but one little fact, in addition, respecting it, should not pass unnoticed. This preparation for coughs is red in colour, and looks fruity, and tastes somewhat sweet, having still, however, an acid dash. As winter comes the coughs increase, and the demand upon the stock of linctus becomes heavier and heavier. This is expected, and provided for; but one season it had been larger even than usual. The same children and the same women came again and again, most perseveringly; when, in consequence of some inquiries, it was found that one of the most urgent claimants for the favourite physic lived by selling little sweets and pies to children, in a back street near Smithfield, and that she used the favourite linctus to make fruit tarts of.—*Dickens's "Household Words."*

PUBLICATIONS RECEIVED.

Letter additional to the Second Edition of the Divining Ring or Odometer. By H. Mayo, M.D. Edin. and Lond., 1851.

Introductory Address on Medical Education. By Alexander Fleming, M.D., Professor of Materia Medica, Queen's College, Cork. Dublin, 1850.

The New York Register of Medicine and Pharmacy. Vol. 1, Nos. 1, 2, and 3. 1851.

The Stethoscope and Virginia Medical Gazette. Vol. 1. No. 1. January 1851.

Considerations Addressed to the Friends of Foreign Missions. By Kenneth Macqueen, Esq. Second Series. Edin., 1851.

Report of the Proceedings at a Meeting of the Working Classes of Edinburgh, with Resolutions on the subject of National Education. Edinburgh, 1851.

The Institute. (We accept the exchange.)

London Medical Examiner. February, 1851.

Pharmacopœia Nosocomii in Curam Morborum Cutaneorum. Londini, 1850.

Letter to the Right Hon. Sir George Grey, Bart., M.P., on some of the Social Relations of the Medical Profession. By G. Robinson, M.D., Newcastle-on-Tyne.

General Directions for Clinical Observations on the more important points of Surgery. Designed for the use of the Students of the Queen's College, Birmingham. By Langston Parker, one of the Surgeons to the Hospital, &c. Birmingham, 1850.

Heilart der Asiatischen Brechruhr von Dr Gerhard von Breuning-Zweite Auflage. Wien, 1848.

Zeitschrift der K. K. Gesellschaft der Aerzte zu Wien. (We have just received a complete set from April 1848, to October 1850, together with the January Number for 1851. We hope that the missing Numbers, viz., November and December (eilftes und zwölftes heft) 1850, will be sent shortly, and that this exchange, which we highly value, will in future go on regularly).

TO OUR READERS.—Though our usual limits have been considerably exceeded, we have been this month compelled to omit our Periscope, and abridge the number of our Reviews.

Part First.

ORIGINAL COMMUNICATIONS.

ARTICLE I.—*On the Physiological Properties of Picrotoxin.* By R. M. GLOVER, M.D., F.R.S.E., Lecturer on Materia Medica in the Newcastle-on-Tyne School of Medicine, &c.

(*Some trifling additions have been made to this Paper since it was read to the Medical Society of London, December* 28, 1850.)

A FEW years ago, I made a series of experiments on animals before my class, with several of the active principles of drugs, such as aconitina, conia, atropia, &c. Among others, I tried picrotoxin. My object was partly the instruction of my pupils, partly to ascertain how far the statements made with regard to the properties of these active principles would be confirmed. Some of my results differed from those of previous investigators. For example, I was led to doubt how far atropia should be regarded as the sole active principle of belladonna.

The results which I obtained with picrotoxin were so curious that I was induced to communicate them to the Medical Society of London, about two years ago, through Dr Cogswell. A dog, under the influence of the substance, exhibited some symptoms resembling hydrophobia; and, besides, a singular convulsive movement backwards was observed, shown in a slight degree by convulsive twitches of the head, neck, and shoulders backwards, and in an extreme degree by a regular forced retrogression. The animal would try to mount some steps, and even cling to them with energy; but it would be unable to resist the impulse backwards, and would descend the steps backwards, and continue to move in this direction until it fell down in tetanic convulsions.

During the last summer, I made several experiments with this substance, which I shall shortly relate; but it may be well to premise some brief notice of the nature and chemical properties of the substance.

Picrotoxin is obtained by boiling the berries of *cocculus indicus* in a sufficient quantity of water. The decoction is then filtered, and acetate of lead added as long as a precipitate falls. The liquid is filtered, and cautiously evaporated to the consistence of an extract.

This is to be dissolved in alcohol; and the solution evaporated to dryness. The solutions in alcohol and water must be alternately repeated, until the dry residue is completely soluble both in alcohol and water. This is picrotoxin, with a little colouring matter. By agitation with a little water, the colouring matter, which is very soluble, is removed, and a number of crystals are separated, which are picrotoxin, nearly pure.

This substance crystallises in needles or silky filaments, in transparent plates, in radiating and mammiform masses, or in hard granular crystals. It is soluble in twenty-five times its weight of boiling water, and about 150 times its weight of water of the temperature of 57°. Alcohol, sp. grav. 0·810, dissolves one-third of its weight of it. Sulphuric ether, sp. grav. 0·7, dissolves two-fifths of its weight of it. Its alkaline nature is doubtful. It is very bitter. The formula is, C_{12} H_7 O_5.

The picrotoxin with which I experimented was in beautiful white needles; and I could have no doubt of its purity, both from the sources from which it was obtained, and its possession of the chemical properties ascribed to the substance, and also from the physiological properties observed in its action on animals corresponding to those ascribed to the *cocculus indicus* and to picrotoxin.

The most full, and apparently exact, description which I can find of the toxicological action of the crude drug is contained in Orfila's *Toxicologie Générale.*

Orfila says:—"In comparing the effects of picrotoxin with those of *cocculus indicus*, I have tried to determine the action of these substances." [1]

"*Experiment* 1.—When robust dogs are made to swallow twelve or sixteen grammes of *cocculus indicus*, pulverised as much as possible, and the gullet is tied after the injection of the poisonous substance, we remark that these animals are not long in making efforts to vomit. At the end of twenty, twenty-five, or thirty minutes, their walk and attitude are tottering, their eyes prominent and haggard, their muscles are agitated with a trembling, which is at first slight but is augmented by degrees; soon afterwards their features are affected by convulsive movements of various muscular parts of the face; contortions and horrible grimaces announce a general nervous attack; all at once they make some steps backwards, the fore-legs stiffen, they stop, and with difficulty avoid falling by reposing on the posterior limbs; the head ere long experiences a violent shock, like that caused in frogs by a strong electric discharge; sometimes these motions are sufficiently strong to cause this part to be reversed on the trunk, and to produce a somerset backwards, in which the head strikes the ground with violence, and the body rolls in all directions." He then describes the "temporary recovery of the animals,

[1] Toxicologie Générale, 1843, t. ii., p. 499.

and their death under repeated attacks of the most terrible convulsions."

In the experiments with picrotoxin itself which he relates, the peculiar movements which I am about to describe do not seem to have been so distinct as in my experiments; and the idea does not appear to have occurred to him that the movements produced are exceedingly similar to those which occurred in the experiments of M. Flourens, where sections were made of various parts of the encephalon.

I shall now proceed to relate my experiments, premising that I do not describe every symptom observed, since such a description would swell this paper to an inconvenient bulk.

Experiment 1.—A mongrel dog was forced to swallow twenty grains of the poison. In a few minutes he seemed uneasy. In twenty minutes he vomited freely; salivation followed, and general tremors. In half an hour from the beginning the salivation was profuse. In about three quarters of an hour there were severe convulsions—opisthotonos. He then attempted to move backwards, but fell down with his fore-legs extended. In an hour the breathing became laborious; again another spasm of opisthotonos. The spasms diminished gradually in severity; but the saliva continued to flow. In an hour and twenty minutes he again vomited. In two hours he had partially recovered, and was able to go about, but was still salivated. During the night he made his escape.

Experiment 2.—A bull-dog had thirty grains of the poison inserted under the skin of the *axilla*. In ten minutes tremors came on. In a quarter of an hour he had a stool, and began to run backwards. He then stopped, and commenced to make peculiar movements with his fore-paws as if swimming, and to scratch the ground with his fore-feet. At the twentieth minute he vomited, and seemed to have little power over his limbs; in two minutes more he passed urine and fæces, and again began to scratch the ground. Frequent vomiting and salivation ensued, and he had bloody stools. At about the twenty-seventh minute there were tremors of the whole body, and backward movements of the head. He continued thus frequently passing urine and bloody stools, and occasionally moaning. In an hour and twenty minutes he had a violent spasm of opisthotonos. He had also several of the backward movements. A bloody discharge was constantly flowing from the rectum, and saliva from the mouth. The breathing laborious, and heart's action frequent. The spasms of opisthotonos became absolutely dreadful. In their interval the animal staggered, and occasionally fell down, with dilated pupils and bloodshot eyes. He would sometimes batter the ground with his head, and grind and gnash his teeth, so that the sound could be heard for some distance. The mouth was filled with dust, and bloody. At about the second hour, the symptoms began to abate somewhat, and in the course of the day he got gradually better.

In three days he got comparatively well, but still had constant tremors, was stupid, and had little appetite. I now inserted two scruples under the skin of the groin. The symptoms were very similar to those which occurred in the first experiment. The retrograde movements were occasionally very distinct, and the animal, while lying on the ground, would sometimes work himself round in a circle. At the forty-fifth minute, he got up and rushed forward, a terrific object, the saliva and bloody foam flying in *sheets* from his mouth, and his eyes glaring. He suddenly stopped, ran right backwards, and was seized with a violent fit of tetanus, at the end of which he was dead.

On opening the body, the facts and appearances observed were as follows:—Almost total extinction of the muscular contractility; the heart was irritable—the auricles much more so than the ventricles. Both cavities of the heart were distended with black fluid blood; the lungs, although somewhat darker than natural, were crepitant and collapsed. The stomach and intestines showed considerable marks of irritation. The peristaltic movement went on. The brain and its membranes were greatly congested, especially the lower portion, the *cerebellum*, *corpora quadrigemina*, and upper portion of the spinal cord. There was much bloody serum in the ventricles of the brain.

Experiment 3.—I was desirous of testing the action of the poison on an animal peculiarly likely, I thought, to shew the effects on locomotion. I selected a female ass. Sixty grains were put under the skin, but nothing was observed except tremors, some striking out of the limbs, and exhibition of general uneasiness. On this, after an interval of time, during which the animal had quite recovered—in fact, a delay of weeks occurred—120 grains were put into the mouth, and she was forced to swallow. An hour elapsed, and hardly anything was observed, except some slight tremors. From the little effect of the sixty grains before administered, I was afraid that the quantity was insufficient to affect materially so large an animal, and 100 grains were again introduced into the stomach. The dose had hardly been administered for a few minutes when the animal became salivated; she then began to strike the ground with her fore-feet. The breathing became laborious,—she suddenly ran backwards for at least twenty paces, fell on her side, and expired after a terrible fit of tetanus, which lasted about five minutes.

The *post-mortem* appearances were similar to those of the dog. The stomach was not examined. There was the same want of irritability of the voluntary muscles, but the heart contracted on being irritated. The congestion of the base of the brain could not be mistaken; and that of the vermiform process of the *cerebellum* was very remarkable.

Experiment 4.—A rabbit was the subject of this experiment,—a strong male was chosen. Ten grains of the poison were inserted under the skin of the *axilla*. He moved about the room for about eight

minutes without manifesting any uneasiness, when the muscles of the neck began to be agitated. In eleven minutes defective muscular power of the limbs was observed, and the tremulousness increased. In seventeen minutes the respirations were much quickened, and the whole body agitated backwards. At the twentieth minute he was unwilling to move, lying on his belly. The ears now fell back; the eyelids twinkled, and he appeared as if going to sleep. At the thirtieth minute convulsions began; he ground his teeth, and clashed them together with a loud noise. He exhibited distinctly the backward movement, and uttered cries. He then struck out with the fore and hind legs, and began pawing the air when lying on his side. Fits of opisthotonos, difficulty of breathing, tears and salivation, were observed; the eyelids winked, when an object approached the eye, till shortly before death; pupils greatly dilated. During life the highest temperature that could be observed in the mouth or elsewhere was 102°. After death the temperature of the muscles was 105°. The post-mortem appearances corresponded with those already described.

Experiment 5.—A pigeon had ten grains of picrotoxin put under the skin of the *axilla*. In fourteen minutes convulsive backward movements of the head and neck were observed. At the sixteenth minute the animal vomited from its crop; strange motions of the feathers, very difficult to describe, occurred; it seemed as if they were agitated in all directions. The animal fell on its side; in this position, by movements of one wing, it contrived to turn itself round in a circle, as if rotating on a pivot, and this lasted for some minutes. It exhibited also spasms of opisthotonos, and pecked at the ground with its bill.

After death the same want of muscular contractility was observed, as in previous instances. On cutting the muscles of the chest, they were found to be absolutely *hot*. A thermometer was inserted among them as soon as possible, and it *instantly* rose to 115°, above which point it was not graduated.

Experiment 6.—A frog had five grains of the substance administered to it, one-half inserted beneath the skin of the thigh, the other half given by the mouth. It leaped about vigorously for about fifteen minutes, when it began to have general convulsions; twice it moved about in a small circle, then it had three or four paroxysms of opisthotonos, the body and hind legs forming a continuous curve backwards. It emitted a long plaintive cry. After the convulsions, the animal crawled about, using the extremities alternately, sometimes doing so without making progress. Occasional twitches of the toes of the hind legs. It then gradually appeared to grow torpid, scarcely moving when approached; finally, it hardly moved at all, except when stirred. The experiment was commenced at two minutes to 6 P.M., and at half-past 11 all spontaneous motion had ceased; but cutting the skin of the thigh still caused contraction of the limb. In the morning it was dead.

Experiment 7.—A small gold-fish was put into a dish of water with five grains of picrotoxin. No effect was observed for about half an hour; the fish then seemed excited; it twice sprang out of the glass; it then gradually lost the power of keeping the back and belly vertical, and inclined sideways. In about forty-seven minutes it could be taken out of the water without struggling; it gradually lost the power of motion, and in about six hours was dead.

Other experiments on dogs and rabbits were performed, which, for brevity's sake, I do not detail. In one experiment, on a dog, efforts to make water were observed, and the animal was only able to pass a drop or two at a time; other symptoms similar to those already described; the breathing very frequent, and heart's action feeble; consciousness till near death, in the intervals between the backward movements (which were very distinct) and fits of tetanus; in fact, the animal walked when called by its name. Neither the heart, nor other muscular structures, were irritable by the knife; the kidneys and bladder were much congested.

A rabbit was killed in twenty-four minutes by five grains dissolved in a little ether put into the mouth. This animal had most extraordinary salivation, and attempted to bite everything it encountered, even its own limbs. Another rabbit, which had ten grains inserted under the skin, when the poison began to work, threw a perfect somerset. The post-mortem appearances were pretty similar to those described in the detailed experiments.

The general resemblance between the peculiar movements observed in these experiments, and the phenomena which M. Flourens has described, as produced by his sections of particular parts of the nervous centres, and especially of the *corpora quadrigemina* and *cerebellum*, is very striking. It must be remembered, that in my experiments the locomotive phenomena were less distinct than they would probably have been, but for the fact of several parts of the nervous system having been affected, as well as those which, when operated upon by M. Flourens, gave rise to the very curious results which he obtained.

I quote the following from the report of Cuvier on the experiments of M. Flourens:[1]—"After the effects of the removal of the brain, properly so called, M. Flourens examines those of the extirpation of the *corpora quadrigemina*. The removal of one of the pair (bigeminal), after a convulsive movement, which soon ceases, produces, as a lasting result, blindness of the opposite eye, and an involuntary turning; that of the pair renders the blindness complete, and the turning more violent and more prolonged. Nevertheless, the animal preserves all its faculties, and the iris continues contractile. The complete extirpation of the tubercles (*corpora quadrigemina*) alone paralyses the iris. * * * What then is most curious in the experiments of M. Flourens, is that part which relates to the

[1] Report of M. Cuvier to the Institute on the experiments of M. Flourens.

functions of the *cerebellum*. During the removal of the first layers, there was only a little weakness and want of harmony in the motions. When it came to the middle layers, there was almost general agitation—the animal, still continuing to see and to hear, only executed quick and irregular motions. Its faculty of flying, walking, and keeping itself erect was lost by degrees. When the whole *cerebellum* was removed, this faculty of executing regular movements had entirely disappeared."

I have not space, nor is it necessary, to extract many of the experiments of M. Flourens; but the following bear so directly upon the subject, that I think it necessary to quote them:—"I gradually removed the *cerebellum* of a swallow; this bird was soon reduced to such a state, that it could only fly in the most singular and extraordinary manner; it went back instead of advancing; it rolled on flying; its flight had all the appearance of the most complete drunkenness. With this exception, this animal, like the others, preserved all its perceptive and intellectual faculties." He goes on: "I removed the *cerebellum* of a young and vigorous dog by successive slices. The animal lost more and more the faculty of moving with order and regularity; soon he only walked tottering and by zigzag; he went back when he wanted to advance; when he wanted to turn to the right he turned to the left. As he made great efforts to move, and was no longer able to moderate these efforts, he rushed forward with impetuosity, and finished by falling or by rolling on himself. If he found an object on his road, he could not, whatever design he might have, avoid it. He hit himself right and left. Nevertheless, he heard and saw very well; when irritated he sought to bite, and bit an object when he could meet it; but he could not dispose of his motions with such precision as to meet it often." [1]

Those who witnessed my experiments will agree that this was almost exactly the character of the phenomena which picrotoxin produced.

The congestion at the base of the brain in the animals poisoned in my experiments was ascertained both by the eye and by examining the cerebral matter with the microscope.

I am aware that congestion is often vaguely described, and too much attributed to it; but I could hardly be mistaken in the appearances presented, as I have examined the brains of hundreds of animals after death from poisons. I do not, however, mean to infer that this excessive vascularity of the cerebellum was the *sole cause* of the phenomena.

I drew the following inferences from these experiments:—

1. Picrotoxin has the power of determining peculiar movements and effects, very similar to those described by M. Flourens, as re-

[1] Recherches Expérimentales sur les Propriétes et les Fonctions du Système Nerveux. Paris, 1842. Chap. vii.

sulting from section of the *corpora quadrigemina* and *cerebellum*. I do not think, however, that the animals in my experiments were blind until just before death (if then), as in the cases where Flourens removed the *corpora quadrigemina*. The iris was certainly in some cases contractile until the symptoms became very severe.

2. Picrotoxin also acts powerfully on the spinal cord, and generally is a narcotic acrid poison.

3. Although a most terrible poison, it is not so formidable in small quantities as conia, aconitina, and others of these active principles.

4. The animal temperature was much increased in some of those experiments, forming in this respect a great contrast with some facts I observed several years ago, showing the great diminution of temperature undergone by animals slowly poisoned by the chloride and bromide of olefiant gas, chloroform, bromoform, &c. How far the increase of temperature was owing to excitement of the nervous system I shall not pretend to decide.

It appears to me that these peculiar movements, which are certainly not voluntary, should also be distinguished from reflex movements. It is not easy to see how they tend to the conservation of the individual. As voluntary movements originate from the brain, and reflex motions from the spinal cord; these other movements appear to be connected principally with the cerebellum. My results strengthen so far those of Flourens, which latter can no longer now be ascribed to the mere mutilation of the animal.

Similar movements have been occasionally observed in disease, in animals wounded by shot, and in drunk people.

ARTICLE II.—*On Leucocythemia, or White Cell Blood.* By JOHN HUGHES BENNETT, M.D., F.R.S.E., Professor of the Theory of Physic, and of Clinical Medicine, in the University of Edinburgh. —(*Continued from p.* 38.)

IN the January Number of the Journal I gave an account of all the cases which were known to me up to the time of publication, including those which had fallen under my own notice. Since then I have thought it right, with a view of bringing together all the facts hitherto observed, to request the co-operation of such members of the profession as, I had been informed, had seen examples of the disorder; and I am happy to say that all of them (with one exception) have at once furnished me with the history of their cases. These I now lay before the professional public.

The two following cases were communicated to me by Dr T. K. Chambers. One of them is a more detailed account of Case XI., a

notice of which only formerly appeared, so that we continue the series of observations with another history of

CASE XI.—*Leucocythemia detected during Life and after Death; Hypertrophy of the Spleen.*—(Communicated by Dr T. K. Chambers, of London.)

William Gowin, a cabinetmaker, aged 42, had usually resided near Carnaby market, a close built, but not malarious, part of London. He was never a very strong man, and has occasionally had bleeding from the gums and nose. Towards the end of 1846 he was obliged to give up work, by a cough and pain in the right side. In the early part of 1847 he was a patient under my care at the Chelsea Dispensary, at which time he presented a very pale and cachectic appearance; his gums were spongy, and his skin harsh, and he had a very well-defined tumour in the region of the spleen. On being questioned on this head, he stated it to be of very recent growth, and did not appear to have paid much attention to it. His sputa were occasionally tinged with blood, but I could not discover any stethoscopic signs of pulmonary disease. The tumour grew rapidly; and to provide for his comforts better than could be done at home, and also for facility of observation, he was sent into St George's Hospital, under Dr Nairne, on February 10th, 1847. The tumour then extended two inches over the median line, towards the right side. On the 1st of March he was attacked by violent epistaxis, which was stopped indeed after some difficulty by plugging the nose, but which weakened him so much that he died on the 9th. Immediately on his entrance into the hospital, I examined his blood under the microscope, and found in it a considerable number of large spherical corpuscles, about three or four times the size of the ordinary red globules.

Sectio Cadaveris.—A small quantity, about the size of a bean, of tubercular matter, together with some puckering of the pulmonary tissue, was found at the apex of each lung. The blood was all fluid. The other viscera were healthy, except the spleen, which was greatly enlarged. Its long circumference measured 27½ inches, its broadest circumference 18 inches. Its appearance outside was yellow, mottled, and smooth, and attached to it were some small supplementary spleens, also diseased, one of a singular round form. Its consistence was about that of a raw potato. The section presented a number of spots, as of ecchymosis from the rupture of small vessels. Near the surface, on the posterior part, was a lump of whitish deposit, like those sometimes found in the spleen, in cases of diseased heart.

Remarks.—In addition to the pale cachectic appearance noticed in previous cases, this man had also violent epistaxis, of which it seems he died. Owing to the kindness of Dr Chambers, I have had an opportunity of examining the drawing made from the spleen at the time, as well as of the blood corpuscles seen under the microscope. With regard to the spleen, its tissue did not present the appearance of simple hypertrophy, noticed in former cases, but contained a number of irregular fibrinous deposits, apparently the result of previous hemorrhages into the parenchyma of the organ. The blood also was characterised by only a slight increase in the number of colourless corpuscles, but these were comparatively much larger in size, the smallest being, so far as I could judge, at least the 1-50th of a millimetre in diameter.

CASE XII.—*Leucocythemia detected during Life; Hypertrophy of the Spleen.*—(Communicated by Dr T. K. Chambers, of London.)

Jane Parsons, a soldier's wife, had usually enjoyed good health till 1845, when, while residing at Windsor, she had what seems from her description to have been mild quotidian ague. She was then 38 years of age. Shortly after this, her attention was attracted by a swelling below the waist on the left side, which, however, gave her no pain or any inconvenience, beyond a slight difficulty in drawing in the breath. About a year afterwards it began to increase rapidly, and in August 1846 she came under the care of Dr Nairne, in St George's Hospital, London. At that period "a large solid tumour occupied the fore part of the left hypochondrium, dipping down to the fore part of the left lumbar and umbilical regions, with no very definite limit towards the right hypochondrium, and not passing at all into the iliac region." It was very hard, and not painful when pressed. The general health was good; the skin clear and fresh coloured, without any of the tint either of jaundice or malignant disease. She left the hospital in a few days, furnished with a belt to support the tumour. She returned on the 13th of March in the following year, saying that she had experienced much comfort from the bandage, but that the tumour had begun again to get larger during the last month. She had also occasional vomiting and constipation, but still the aspect was healthy, and she stated that she was able to continue taking in a little washing from the officers in the regiment. A quinine mixture was ordered to be taken three times a-day, and she returned to the country, with orders to come again in a month. This, however, she did not do. On examining a drop of the blood, it was seen to contain an increased number of colourless cells, as in the last case described.

Remarks.—The parts worthy of observation in connection with this case are, that notwithstanding enlargement of the spleen, and the same slight condition of leucocythemia as was noticed in Case XI., her general appearance was healthy, and there was no epistaxis, diarrhœa, or other of the symptoms which we have previously seen to be the leading features of the disease. There was only slight difficulty on inspiration.

CASE XIII.—*Leucocythemia detected during Life; Hypertrophy of the Spleen and Liver.*—(Communicated by Dr R. Quain, of London.)

In the autumn of 1848, M. Boudler came under my notice as a patient at the St George's and St James' Dispensary. She complained of general debility, of swelling and general uneasiness in the abdomen. She said her age was 33, but she appeared older. She was born near Maidstone in Kent, and resided there till she was 22, when she came to London, and acted as a nurse maid and as a ladies' maid, until she was 26 years of age, when she married. She has never been very strong, though she had no disease of any consequence until she miscarried, about four months after marriage. She then lost much blood by flooding, and her husband soon after having lost his employment as a journeyman tailor, she has not been able since then to procure good food at all times, and she has had much anxiety. From that time she has never been quite well. She never had ague. In early life her menstrual functions were irregularly performed, and she was pale and delicate; but after coming to London she improved in these respects. She has two living children, one aged five or six years, the other nearly four. She has also had two further miscarriages since the birth of her last child. On all these occasions she has lost more or less blood. Her present illness she dates chiefly from the birth of the last child, which she tried to suckle, but which she had to wean at the end of five months, owing to her debility. She then applied for advice to a dispensary, and received some medicine, from which she derived

much benefit. She did not menstruate, and soon after observing that her abdomen was swollen, though not as on former occasions, she believed herself pregnant. This impression proved correct, for she shortly after miscarried. The size of the abdomen, however, did not diminish, and it has continued to increase more or less since then,—that is, for rather more than three years. She has during that time grown thin; she is now very emaciated, and her skin is of a sallow hue. She has suffered from dyspepsia, sickness, flatulence, and irregularity of the bowels; she had hæmorrhoids about five years ago, but not since; her appetite is bad; she sleeps badly; her breath is short, but she has at present no cough; she has never spat blood; she has palpitation of the heart on occasions of slight effort or of fright. The menstrual functions are very irregular, and she has leucorrhœa,—this she has always had more or less. Urine scanty, depositing lithates, and staining the bottle which contains it with a deep pink colour; no albumen. Auscultation and percussion failed to elicit any signs of disease in either the lungs or heart.

The abdomen when examined was about the size of a female's who had advanced to nearly the seventh month of pregnancy. The parietes were tense, and the umbilicus prominent. There was marked dulness at the left side, extending vertically from nearly the level of the fifth rib (on strong percussion) to the crest of the ilium; and horizontally, from nearly the median line in front to the spine behind. There was also dulness from the same height at the right side to three inches or more below the ribs. At the epigastrium the like dulness existed, but here it passed into a loud tympanitic resonance. The like resonance existed around and below the umbilicus. Owing to this distension of the abdomen with flatulence, it was difficult to define with the hands the edges of either the liver or supposed enlargement of the spleen. But when the patient was placed in the prone position, the existence and general form of these solid bodies were traced, corresponding to the dulness mentioned. The fluctuation of a thin layer of fluid was also observed in the abdomen. There was then no œdema, but she stated that her ankles were often swollen at night.

I prescribed, at the time of her first visit, some mild aperient; and took occasion to examine her blood microscopically at her next visit. I found the field of the microscope occupied by blood globules arranged as usual, the intervals between these being almost filled up with cells two or three times larger than the blood globules, and containing several granules. They conveyed the impression at first sight of being pus globules, but the contained granules were fewer and less regularly arranged. The patient's pulse on this occasion was marked as being 80, small and jerking. Her tongue pale, not coated nor swollen. I prescribed two grains of quinine, and one grain of sulphate of iron, ten drops of diluted sulphuric acid, and an ounce of mint water, three times daily, and three grains of blue pill, with some henbane, every fourth night, followed by a dose of Gregory's powder. The Samaritan fund of the Dispensary supplied her with some bread and milk and warm clothing. Under this treatment, slightly modified, she continued for nearly three months, and her general health was considerably improved. The size of the abdomen was but little altered. She then expressed an intention of going into the country, and I heard no more of her. I examined the blood on two other occasions, without seeing any material change in its appearance. The existence of nuclei in the large cells was rendered evident by the action of acetic acid when used once.

Case XIV.—*Leucocythemia detected during Life; Hypertrophy of the Liver, and probably of the Spleen.*—(Communicated by Dr R. Quain, of London.)

(Of this case I have no notes, having seen the patient only once.)

A commercial traveller, between 40 and 45 years of age, of rather stout conformation, called on me in January 1849. He said he was about to leave town the same evening, and he was anxious to get some advice before doing so. For more than six months he had felt less equal to his work than previously; he knew of no cause for this, as he generally enjoyed good health. The last four

months he felt this feeling increasing, and he complained more particularly of shortness of breathing, and being easily fatigued. He suffered much from indigestion and from hæmorrhoids. He said that he had became stouter round the waist. On examining the abdomen, I found considerable enlargement of the liver, and the dulness continuous with it extended from the epigastrium into the left hypochondrium. I recollect, however, that I had some doubt as to this being connected with enlarged spleen. The microscope happened to be on the table, and more as a matter of curiosity than anything else, I placed a drop of blood from his finger under it, and was somewhat surprised to find a very great number of the large granular cells mixed with the blood globules. They were, I think, fewer than in the other case. I prescribed some bitter tonics and mild aperients, with a little blue pill. I have not since seen this patient.

Remarks.—The hemorrhagic tendency was manifested in Case XIII., after every confinement, by flooding. Epistaxis, dyspnœa, and diarrhœa are not mentioned as having been present. In Case XIV. there was slight difficulty of breathing. In both there were hemorrhoids.

The two following cases, for which I am indebted to Professor Walshe, having been made the subject of clinical investigation with a class of students, were reported with extreme care, and are therefore of great value. From the copy sent to me, taken verbatim from the case-book, I have condensed what has appeared to me important in reference to the subject of leucocythemia.

CASE XV.—*Leucocythemia detected during Life and after Death; Hypertrophy of the Spleen and Liver.*—(Condensed from Dr Walshe's Case-book.)

Caroline Smith, æt. 28, admitted into the North London Hospital under the care of Dr Walshe, August 10, 1846. She had formerly been under the care of Dr Taylor, and was dismissed only seven weeks ago. Since then she has been labouring under cough, œdema of the inferior extremities, and diminished secretion of urine. She has also had bleeding from the gums to a considerable extent; and latterly there has been vomiting. Four months previous to her first admission she had intermittent fever, occurring daily; but this point was not sufficiently inquired into. On admission, the circumference of the abdomen at two inches above the umbilicus is thirty-six inches and three-quarters; its general surface is globular, even, and uniform, except one inch below and two inches and a half to the left of the umbilicus. This point corresponds with a hard and prominent part of the solid mass within the abdomen. The superficial veins are not distended. Over the surface of the abdomen (especially towards the hypogastrium) there are various irregular fissures, and slight lines of extravasation in the skin, from distrusion. The lateral left surface of the tumour, though in the main smooth and even, conveys the sensation of a rough, corrugated, finely splenic surface; no nodules or large tuberculation of surface; its lower border is four inches and a half above the pubes, and has evidently no connection with the pelvic bones; it passes up distinctly beneath the ribs. The upper margin of the dulness over it extends to a level with a line carried round from the point of the ensiform cartilage.

In front of the abdomen there is a distinct sense of peripheric fluctuation, which is lost when percussion is made over the tumour; no common fluctuation. There is fulness in the right hypochondrium; she feels easiest in the sitting posture, being thus less distressed by cough or dyspnœa. No great pain is felt anywhere; none in the tumour. The apex of the heart beats within three inches and a half of the clavicle and a quarter of an inch from the sternum. The mouth presents patches of apthous exudation. The three

following days there was considerable dyspnœa. No dulness on percussing the lungs posteriorly; but in the left supra-scapular fossa bronchophony was marked, inspiration harsh and exaggerated, with loud expiration. At the base, inspiration blowing, without corresponding expiration. On the right side the sounds are less marked. On the 14th the bowels were opened five times, and diarrhœa continued more or less severe until her death. On the 15th there was a return of the bleeding from the gums, which only continued a few hours. On the 22d the stools were streaked with blood. On the 24th œdema of leg and anasarca was observed to be extending. An erythematous blush on left leg. On the 26th expectoration thick and puriform. Pulse 128, feeble. Respiration under left clavicle bronchial. On the 28th respiration fifty-two in the minute, gasping; urine, eighteen ounces. 31st,—Emaciation much increased. Passes urine in bed. Slight delirium. Measurement of abdomen at umbilicus, forty-eight inches and a five-sixth. Died September 1.

Sectio Cadaveris, six hours after death.—The Blood in the veins is fluid, grumous, of a dirty brown colour. When the descending cava is cut, it flows out in large quantity, resembling in its appearance the grumous substance which issues from excessively softened spleens. Examined microscopically, it is seen to contain colourless corpuscles as numerous as the coloured ones. From the description given, they were evidently similar to those noticed in former cases. Although it is not stated in the report, these corpuscles were seen during the life of the patient, in blood drawn from the finger, and were frequently demonstrated to the class.

Chest.—The right cavities of the heart were distended with blood, presenting the characters above described. Pleuræ on right side universally adherent, but on the left side they are only adherent slightly at the posterior surface. Lower portion of left lung is not crepitant, of dirty grayish-brown colour, and sinks in water. Apex also somewhat condensed, and loaded with pigment. The right lung generally crepitant, with a cretaceous concretion, size of a pin's head, surrounded by a stellate puckering.

Abdomen.—The spleen entirely filled the left hypochondrium to the extent ascertained during life. On removal, it weighed three pounds and a half. It is eleven inches and three-quarters long, six inches broad, and two inches and three-quarters thick. Externally it presents the corrugated, wrinkled surface of a cirrhosed liver. On section, a large quantity of the blood formerly described exudes from it. It is generally dense and heavy, but more resistant in some places than in others. The cut surface is of a brick-red tint, slightly granular, minutely spotted with blood. The veins are dilated, but healthy in texture. The capsule is easily separable, and is thickened here and there by old exudation. The liver is much enlarged, its anterior border, by pressure, dividing the stomach into two portions, as is seen in cases of hour-glass contraction. When removed, it weighs thirteen pounds and three-quarters; measures, transversely from right to left, thirteen inches and a half; from above downwards, twelve inches and a half; and from before backwards, three inches and a half. Its surface is in general smooth and natural; free from adhesions. On section, the centres of the lobules are injected; but there is no cirrhosis. Its substance is very friable; and, from the large veins, great quantities of the grumous blood pours out on section. The kidneys of dark colour, congested. Other organs healthy.

Remarks.—In this valuable case there were present all the leading symptoms we have previously seen to exist in former well-marked instances of the disease: bleeding from the gums, dyspnœa, and continued diarrhœa. The enormous size of the liver, weighing thirteen pounds and three-quarters, is also worthy of observation. Nothing is said with regard to the lymphatic glands. The antece-

dent occurrence of intermittent fever is alluded to, although this was not positively ascertained.

CASE XVI.—*Leucocythemia detected during Life; Hypertrophy of the Spleen.*—(Condensed from Dr Walshe's Case-book.)

Pièrre Dujardin, æt. 38, admitted into the North London Hospital, June 24, 1850. Formerly a French artilleryman, but for the last three years a clerk. Has never had ague, or lived in an aguish district. Six weeks ago, when suffering from indigestion, he first perceived that the left side was hard, while the right side was soft. On admission, the abdomen is observed to be enlarged on the left side, as he lies on his back. On percussion, dulness commences on a level with the seventh intercostal space, and continues downwards laterally to the crest of the ilium. From thence the line of dulness turns round anteriorly, in the form of an arch. At its lowest point it is three fingers' breadth above the pubes, and ascends from thence in the middle line, joining the hepatic dulness at a right angle. Within these limits there is felt a solid substance, with a uniformly smooth surface. The right edge is rounded, and so well defined as to be almost capable of being seized by the hand. On placing the patient on his right side, the posterior edge of the tumour is found to extend back to within a hand's-breadth of the spinal column. The left half of the abdomen, two inches above the umbilicus measures seventeen inches; the right half, sixteen inches and three-eighths. Apex of the heart beats in the fourth intercostal space. Aortic impulse increased, but without abnormal murmur. Respiration of left lung impeded, and murmurs diminished, over inferior portion of chest on left side. Has no œdema of extremities. *Frictions with the Ung. Plumbi Iodidi, ʒj., over the tumour twice a-day.* ℞. *Potass. Iodidi, gr. iii.; Inf. Cascarill. ʒss.; Aquæ, ʒj. M. Ft. Haustus. To be taken twice daily.*

June 29.—The urine passed is ʒxvj., density 1026, containing a deposit of uric acid plates. Never suffered from urinary symptoms. The blood contains an increased number of colourless corpuscles, certainly five times as many as are seen in health. *July* 4.—The tumour is now two inches and a half to the right of the median line, and its lower edge two inches and a half below the umbilicus, having ascended since admission. *July* 20.—The lower border of tumour is drawn inwards. The abdominal swelling is increased laterally. Two inches above the umbilicus, the abdomen measures, on the left side, seventeen inches and a half; and on the right side, seventeen inches and a three-eighth. *August* 2.—Discharged, his general health being tolerably good.—*Since the 23d of July, the Potass. Iodid. has been discontinued; and he has taken three grains of Quinine three times a-day.*

Remarks.—This man's general health seems to have been tolerably good during his residence in the hospital, and none of the symptoms so prominent in the last case were present.

CASE XVII.—*Leucocythemia detected during Life; Hypertrophy of the Spleen.*—(Portion of a Letter received from Dr Hislop, of Birmingham, dated February 10, 1851.)

Several months ago, I examined microscopically the blood of an Irishman, who has a spleen which fills nearly the whole left half of the abdomen, extending midway between the umbilicus and pubes. I found in it a great number of the white corpuscles as figured by you, and described by Parkes in the "Medical Times," about four months ago. To-day I again saw this patient, and re-examined the blood. Though still existing in great numbers, I did not think them so numerous as at my last observation; but I was struck with the great size of the *nucleus* of some of these bodies, appearing, in fact,

nearly as large as the yellow corpuscles, and adherent, I thought, *uniformly* to the cell-wall; those corpuscles which contained minute ill-aggregated granules, for the most part contained no very obvious nucleus, while those which had a perfectly distinct adherent nucleus appeared to me absolutely free from other contents. Is the nucleus a subsequent stage of the granule condition in this case?—a stage nearer the yellow corpuscle? The cell-wall of the large body may possess the power of *aggregating* its contained granules into the nucleus-like corpuscle adherent to its wall; so that, speaking mechanically, all that was wanting was the *fracture* of that cell-wall for the manufacture of a yellow corpuscle. I wished to mention this case to you, because, thirteen years ago, he was attacked with *ague*, and had attacks of that disease for four or five years in succession. He has not himself noted the commencement of the tumour more than *three years*, but it is very probable that at its first origin it failed to attract his attention; so that Dr Fowler's idea, quoted in your paper, must be doubted, that *ague cases* do not show these bodies. He is pale; skin dry; and has the earthy-worn aspect of chronic ague. Lately his legs have become slightly œdematous. This may be owing to the fact of his urine having become albuminous, which was first noticed about four months ago. I did not examine it to-day. When he first came under my notice, twelve or fourteen months ago, the spleen was nearly of its present size.

Remarks.—This case is important, as pointing out that a state of leucocythemia may follow intermittent fever. This occurred also in Case VII. Whether the ague was in any way connected with the blood disease, however, may be questioned, as in one case it occurred thirteen, and in the other eighteen, years before the leucocythemia was detected. I hope to obtain a more particular account of this case hereafter, especially in reference to the other symptoms which may have been present.

Case XVIII.—*Leucocythemia detected after Death; Hypertrophy of Liver and Spleen.*—(Extracted from the Pathological Record of the Royal Infirmary, Edinburgh.)

Ann Stenhouse, æt. 37, admitted into the Royal Infirmary, under Dr Douglas, August 20, 1846. Six months ago she had a child; two days before delivery there was a severe flooding. She has suckled the child until a month ago; but has gradually become weaker. About three months ago, she first noticed a tumour in left side of the abdomen. It has caused no pain, but there has been great difficulty of respiration. During the last five days there have been acute pains in the left hip, and on the inside of both legs and thighs. On admission, she is greatly emaciated. The abdomen is relaxed; tender on pressure. In the left hypochondriac region a tumour can be felt, extending from the lower margin of the ribs to a level with the umbilicus; its anterior margin being four inches from the median line. There is no appetite; great thirst; bowels constipated; tongue red; pulse 108, small. The inferior extremities swollen and œdematous. On the 22d, she became comatose, and died at 6 P.M.

Sectio Cadaveris.—Chest.—Both pleuræ adherent throughout the whole of their extent by chronic lymph. Lungs somewhat congested, but otherwise healthy. Pericardium contained about one ounce of sanguinolent serum. Right cavities of the heart distended with grumous semi-coagulated blood. Valves healthy.

Abdomen.—Surfaces of liver and spleen extensively adherent by chronic lymph to the surrounding viscera. Liver much hypertrophied, weighing six pounds four ounces; its structure normal. Spleen also enlarged, weighing two pounds four ounces; its surface divided into fine lobules. On section, it was dense and resistant to the knife; but its structure was normal. The

solitary and aggregate glands of the intestine somewhat enlarged; the other viscera healthy.

The Blood of the mammary artery and veins was apparently mixed with pus. In the heart and venous system generally, its fluid portions were of a port-wine colour, and the grumous masses closely resembled the matter squeezed from a softened spleen.

Microscopic Examination.—The softened clots were everywhere composed of numerous molecules and granules, fragments of molecular fibres, colourless and coloured corpuscles. The yellow existed to the white corpuscle in the proportion of one to two; the latter were generally $\frac{1}{100}$th of a millimetre in diameter, although some were twice that size. In structure they were identical with those previously described.

Remarks.—This case was in the infirmary only two days, and, from the facts stated, it would appear that there had been flooding previous to the delivery of her last child, œdema of the inferior extremities, owing to the enlarged size of the spleen, and feverish symptoms before death. The examination was made by Dr Redfern, during my absence on the Continent in the autumn of 1846, and the appearances observed, together with the microscopic examination of the blood, render this a most decided example of the disease. The colourless were twice as numerous as the coloured corpuscles.

CASE XIX.—*Leucocythemia detected during Life; Hypertrophy of the Spleen and Liver.*

James Kerr, æt. 26, labourer, admitted into the Royal Infirmary, September 30, 1850, with jaundice and swelling of the abdomen. Has never had ague nor rheumatism. Three years ago had fever, and was ill for a month, but the character of the fever cannot be ascertained. After this illness he remained in good health till a year ago, when he states that he had occasional shiverings, and that he suffered from general pains and uneasiness, which, however, soon became localised in the abdomen. He had no vomiting then nor subsequently. About three months after the appearance of these symptoms, jaundice supervened, and has persisted more or less ever since. He has been in the house twice previously, and, on the last occasion, it is stated that the liver reached about five inches below the ribs, and that the spleen was also much enlarged. Since that time the abdomen has become larger.

On admission, there is no emaciation. Jaundice well-marked, the whole surface being of a yellowish-brown colour, and the conjunctiva pretty deeply tinged. His only complaints are of loss of strength, feeling of weakness in small of back, and sense of weight in abdomen. There is also slight pain in right shoulder, which he says dates from three weeks ago. He has no pain in the loins nor abdomen. Takes his food pretty well, but complains a good deal of thirst. Bowels rather inclined to be loose. Tongue of a florid colour, marked at the tip with several transverse cracks. Urine has been rather diminished in quantity for the last few days. There is evident enlargement of the abdomen; its greatest girth is thirty-six inches; a distinct bulging is observed in the epigastrium; both liver and spleen can be felt to be enlarged; a narrow space to the left of the mesial line, about as far across as the left nipple, can be felt between these organs. The hepatic dulness extends from the fifth rib down almost to the level of the umbilicus (about seven and a half inches), fills the whole epigastrium, and reaches to the left as far as the sulcus mentioned above. The splenic dulness reaches from the lower margin of the fifth rib to within an inch of the crest of the ilium. There is no fluid in the peritoneal cavity. The heart's action and sounds natural. An increased pulsation is distinctly visible at the root of the neck, and can be felt above the clavicles,

and under the top of the sternum. Here there is a somewhat rough murmur accompanying both sounds, but heard most distinctly with the first. He has slight cough, with scanty mucous expectoration, but auscultation and percussion of lungs are natural. Urine rather scanty, high coloured, sp. grav. 1026, contains some biliary matter, and deposits on cooling a pretty copious pinkish sediment of lithates.

Dr Robertson was good enough to analyse the blood of this case for me, with the following result :—

Specific gravity of the blood, ...	1049·5
„ „ of the serum, ...	1029·0
Composition of 1000 parts :—	
Fibrin, ...	5
Solids of serum, ...	95
Globules, ...	80
Total Solids, ...	180
Water, ...	820
	1000

This man is still in the infirmary (March 20th), having, since the report was taken, been labouring under repeated attacks of diarrhœa, of shorter or longer continuance, and of epistaxis. He is also occasionally troubled with pain in the right hypochondrium. His general health is much about the same as on admission. The treatment has consisted of the liq. Iodin. c., with remedies to check the diarrhœa, and occasional leeches and blisters to alleviate the hypochondriac pain.

Fig 1. Fig. 2.

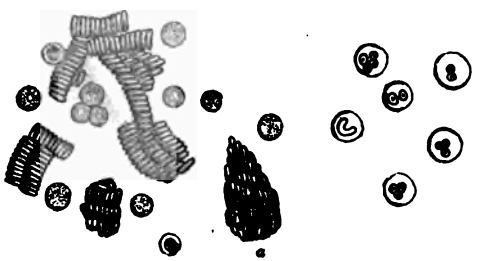


Fig. 1.—Appearance of blood in Case XIX., showing the coloured corpuscles in rolls, and the colourless ones somewhat increased in number. At *a*, the former were aggregated together in masses.

Fig. 2.—The same blood, after the addition of acetic acid.

Remarks.—This case appears to me interesting, as exhibiting the leading symptoms we have previously noticed as being so common in leucocythemia, conjoined with enlarged liver and spleen, while the blood presents an incipient stage of the disorder. The colourless corpuscles were decidedly increased in number, being perhaps two or three times as numerous as in a state of health, but nothing like so much so as in Cases I. and II. The blood, however, presented the same chemical characters as those observed in the other cases in which it was analysed by Dr Robertson.

II.—NOTICE OF CASES, IN WHICH THE EXISTENCE OF LEUCOCYTHEMIA IS PROBABLE, ALTHOUGH, FROM THE BLOOD NOT HAVING BEEN EXAMINED MICROSCOPICALLY, THIS CANNOT BE POSITIVELY DETERMINED.

There can be no doubt that cases of leucocythemia have often occurred, although the condition of the blood was not accurately determined. Were the records of medicine, indeed, carefully investigated, it is highly probable that many instances of the disease might be discovered, although as the information so obtained could lead to nothing positive, such investigation would scarcely repay the toil it would occasion. A short notice of the most important of such cases as are known to me, however, may here be given:—

CASE XX. BY BICHAT.[1]—At the examination of a body in the Hôtel Dieu, Bichat found, among the black abdominal blood, true purulent matter of a green colour, which so filled up all the branches of the splenic vein, the trunk of the vena portæ, and all its branches in the liver, that the pus they contained was sufficient to distinguish all the branches of the portal from those of the cava system, which contained normal blood. The body had, besides, a very extraordinary *embonpoint*. This was evidently not a merely post-mortem appearance, and there could be no doubt that the blood had circulated in a state, if not so much altered as it then appeared, yet very different from natural.

CASE XXI. BY VELPEAU.[2]—A lemonade-maker, æt. 63, who was addicted to drinking and loose habits, had had weakness in the back, when thirty-four years old, which hindered his stooping; but had become stronger. In his fifty-fifth year a very severe cough came on. Two years after he fell down stairs on his right side, which accident was followed by great pain and fever, of which he was relieved in six weeks by antiphlogistic treatment. For five years he scarcely suffered at all from cough; but at that time he had symptoms of stone, which were relieved by baths and diuretics. A year after there was swelling of the left side of the abdomen. A pretty firm painless tumour was felt, which increased without pain up to December 1825. From this period the patient's general health was deteriorated; he lost flesh, had slight wandering pains, and from time to time some little fever, but good appetite. On the 16th of February he had a sudden seizure. His countenance was livid; and an hour after, the left limbs were insensible, and the whole head painful. On the 18th he was admitted into the hospital. Countenance and whole skin were livid; the senses and respiration normal; the abdomen hardly at all painful, with two enormous tumours. Pulse small, irregular. Movements of the extremities not difficult, but somewhat restrained; and the head heavy. In the evening he became decidedly worse; in the night gave a sudden shriek; and died at three o'clock next morning.

Sectio Cadaveris.—The spleen occupied the whole left hypochondrium and side of the abdomen. It weighed ten pounds, and had a cartilaginous spot on its surface. The structure was as dense as that of the liver, without marked alteration. The liver was twice as large as usual; of its normal texture. In the heart, aorta, vena cava and its branches, and especially where the vessels were large enough for examination, thick blood was found, of the consistence of jelly, in which respect, and in its colour, it resembled the lees of red wine,

[1] Anat. Génér. Paris, 1801. I., 65. [2] Révue Médicale. 1829. II., 218.

and scarcely any of it was either fluid or coagulated. It became a question whether it were not rather laudable pus, mixed with blackish colouring matter, than blood. It resembled very much the reddish or black-brown pulp, into which one can break down many softened spleens. Chevreul kept some blood for examination, but it became decomposed too soon.

Case XXII. By Bessiere.[1]—A man, æt. 60, was afflicted with a very remarkable hypertrophy of the spleen (which weighed 6250 grammes), the liver and kidney being also to a less extent enlarged. He suffered from it no more than a mechanical inconvenience. He was treated with calomel, gr. iv., daily. Four days after he was affected with smart fever and dyspnœa, and died suddenly.

Sectio Cadaveris.—Excepting the above-mentioned hypertrophy, all the organs were healthy. The heart, however, was slightly hypertrophied in its ventricles. It contained a jelly-like clot, of a white colour. Similar clots were contained in the aorta, the pulmonary arteries, and in the whole arterial system. The vena cava contained smaller similar clots. These were submitted to chemical analysis. This gave, in 1000 parts,—

Water, ...	867·7
Albumen, ...	82·7
Fibrine, ...	22·1
Fat and extractive matter, ...	20·4
Salts, ...	2·1
Loss, ...	5·0
	1000·0

Case XXIII. By Rokitansky.[2]—In the body of a locksmith, æt. 33, were found, both in the smaller and larger blood vessels of the lungs, yellow-green tenacious blood-coagula, as well as greenish-yellow fibrin-coagula, in the heart-cavities and great vessels. The liver was much enlarged (six pfd. eight lth), pale, tallowy, poor in blood, and containing a muddy purulent fluid. The spleen was very heavy (five pfd. eighteen lth), dense, bacony, and infiltrated with brown red blood.

Case XXIV. By Oppolzer and Lehmann.[3]—The patient, a woman æt. 24, was delivered of her first child several years ago. The catamenia returned at the usual period, but she suffered under continuous uneasiness of the abdomen. Diarrhœa alternated with costiveness; the body increased in diameter. There was pain in the hypochondrium; and suppression of the menses which now took place produced congestive headach. Repeated irritations of the peritoneum were cured by local antiphlogistics. After this occurred repeated severe shiverings. This state continued with little intermission for nearly six months, when the patient had a gray wasted appearance, and became more and more feverish. Examination showed unusual enlargement of the liver and spleen. The body was distended, and there were occasionally severe asthmatic attacks, and remitting severe throbbing headach. After this profuse diarrhœa came on, attended with, on one occasion, bloody discharge. The abdomen was distended, patient exhausted, pulse weak and frequent. Fourteen days before death, a furuncle appeared in the neck, which destroyed the soft parts to the muscles. After this a general varioloid eruption appeared on the skin, resembling an exanthema which had become purulent. After this consciousness failed, and death supervened, preceded by delirium and coma.

---

[1] Canstatt's Jahresbericht. 1845. I. S. 26.
[2] Zeitschrift Wiener Aerste. 1845. II. S. 488.
[3] Schmidt—Kiwisch von Rotterau, die Krankh. d. Wochnerinnen. Prag., 1840. I. 109.

Sectio Cadaveris.—The liver was found so enlarged, that it extended to within a little of the pubes, and, completely covering the stomach, occupied the left hypochondrium, in which region it was connected with the upper extremity of the spleen by a pseudo-membrane. The spleen extended downwards to the iliacus muscle, being also connected to the wall of the abdomen by firm adhesions. Its substance was reddish-brown, much crushed, and presented in its interior several inflamed spots. On dividing the branches of the portal vein, coagula mixed with blood escaped, without appearance of inflammation of the coats of the vessels. The vena cava ascendens contained thin fluid blood, along with purulent coagula, as also the vena portæ and the splenic vein. The pulmonary arteries contained similar clots, and the right side of the heart was filled with fluid blood and sulphur-yellow coagula, which consisted of a purulent mass, surrounded by a gelatinous layer. The left side of the heart, aorta, and thoracic duct were normal. The membranes of the brain were congested, some of the veins of the pia mater containing purulent coagula. The axillary and inguinal glands were enlarged and infiltrated with pus.

Remarks.—This case, with regard to the symptoms which occurred during life, as well as the appearances observed after death, closely resembled the undoubted instances of leucocythemia recorded in section I. The four others had none of these symptoms, although there are facts in connection with the examination after death, especially in Case XXI., which are closely allied to the others recorded.

CASE XXV. BY LIVOIS.[1]—Jean Michel entered the hospital of La Charité, Paris, 15th of May 1837. In 1834, being employed in digging canals at Saint Brizon, caught an intermittent fever, which returned every three days. During the interval he continued his work, so that the disease continued thirty-three months, during which period he consumed an enormous quantity of sulphate of quinine. The fever diminished only on changing his place of work. From an early period of the disease he experienced a pain in the left side, and two or three months afterwards a tumour was perceptible in the painful part. In six months the abdomen became very voluminous, and, in addition to the tumour, fluctuation was distinguished, which gave rise to the idea that a cyst had formed. On the 26th he complained of violent colic; repeated vomiting of a greenish fluid came on, with copious diarrhœa. The dyspnœa which had previously existed was increased, and he died on the following morning.

Sectio Cadaveris six hours after death.—On opening the abdomen a large quantity of sanguinolent serum escaped. A cyst, the size of an ostrich egg, was attached to the spleen, containing a deep-red serum, mingled with a filamentous rose-coloured matter similar to fibrin. The spleen was enormously enlarged, filling the left hypochondrium, and displacing the diaphragm, stomach, and other viscera. Its weight, together with the cyst, was seven pounds. Its vertical length was thirteen, and its breadth transversely eight and a half inches. On section, it was seen to be only simply hypertrophied. The liver also simply hypertrophied, measuring transversely sixteen inches, from before backwards ten inches, and vertically four inches. The sinuses of the dura mater and cerebral veins were filled with black and liquid blood, and the cavities of the heart were filled with a similar fluid; but the right auricle contained a voluminous clot, the density and yellowish-white tint of which contrasted in a remarkable manner with the liquid condition and ash colour of the blood elsewhere. Some sanguinolent serum was found in the pleuræ. Other organs healthy.

---

[1] Bulletin de la Societé Anatomique, 3ième Année, p. 289.

Remarks.—Here enlargement of the spleen and liver was induced by intermittent fever. Dyspnœa and diarrhœa existed during life; and the appearance of the blood after death was peculiar, the density and yellowish-white tint of the coagula in the heart being exactly similar to what was observed in Case I.

Four other cases are recorded by Caventou,[1] Harless,[2] Andral,[3] and Bricheteau,[4] in all of which the blood after death appeared as if it were mingled with pus. Lastly, Dr Hodgkin[5] relates seven cases in which, with greater or less enlargement of the spleen, there was also hypertrophy of the absorbent glands. Although the condition of the blood in the blood vessels did not appear to have been noticed in these cases, a careful perusal of all the facts connected with them induces me to believe that they were closely allied to, if not identical with, those published under section I.

III.—CASES IN WHICH THE SPLEEN WAS HYPERTROPHIED, UNCONNECTED WITH LEUCOCYTHEMIA.

Numerous cases might be given to show that leucocythemia, although most commonly accompanied with enlarged spleen or liver, is not essentially connected with disease in those organs. The three following, in all of which the blood was carefully examined with this view, will perhaps be sufficient for the purpose:—

CASE XXVI.—*Hypertrophy of the Spleen; Phthisis Pulmonalis.*

Mr. C., æt. 25, clerk in a merchant's office at Glasgow, consulted me, in October 1849, for pulmonary symptoms, under which he laboured. I ascertained that phthisis pulmonalis existed in its third stage. He also directed my attention to a swelling in the left hypochondrium, which evidently depended on enlargement of the spleen, which could be felt in the abdomen extending as far forwards as the median line, and inferiorly to a level with the crest of the ilium. A few months before, he had returned from New York, where he had for some time been subject to intermittent fever. I carefully examined blood drawn from the finger of this gentleman, which presented its healthy character. He returned to Glasgow, and died of phthisis in January 1850.

CASE XXVII.—*Hypertrophy of Spleen; Cirrhosis of Liver; Ascites.*

Alexander Miller, æt. 32, a seaman, entered the clinical ward of the Royal Infirmary, November 5, 1849. He laboured under ascites, and œdema of the extremities, which he attributes to exposure to cold and wet, nine months ago, on the Mediterranean coast. The abdomen is greatly distended, measuring forty-five inches and a half in circumference, and the inferior extremities and scrotum are also much swollen from œdema. There was considerable dyspnœa, and the urine was diminished in quantity. When he lay on the right side, the spleen could be felt enlarged, but its exact limits could not be

---

[1] Révue Médicale, tom. iv., p. 567.
[2] Heidelb. Annalen, 1831, bd. vii., p. 26.
[3] Clinique Médicale, ed. 4, 1839, tom. i., p. 93. Obs. xvii.
[4] L'Experience, No. 364, 1844.
[5] London Medico-Chirurgical Transactions, vol. xvii.

determined, on account of the ascites. He died December 29. On examination, the spleen was enlarged from simple hypertrophy, and weighed three pounds eight ounces. On section, it generally presented its normal density and appearance; but in a few places there were spots of diffluent softening. The liver weighed four pounds, and was in the first stage of cirrhosis. Other organs healthy. The blood was examined both during life and after death, but presented nothing abnormal.

CASE XXVIII.—*Hypertrophy of the Spleen.*

Daniel Cameron, æt. 21, a showman at fairs, was admitted into the clinical ward of the Royal Infirmary, July 25, 1850. The throat was very extensively ulcerated, and the spleen greatly enlarged, the swelling being of four months' standing. He was dismissed on the 23d of September, the ulcers in the throat having healed. When re-admitted on the 25th of January 1851, he says that he has had frequent attacks of diarrhœa and epistaxis. He complains of severe pain in the tumour. The abdomen appeared much swollen, and measured at its widest part thirty-three inches and a-half during expiration. Fluctuation could be felt over the inferior portion of the abdomen where the tumour did not extend. The latter was very firm, and painful on pressure, and dull on percussion over its whole surface. It presented anteriorly a well-defined margin. Superiorly this margin was felt emerging under the false ribs, and passed directly downwards about three inches to the left of the umbilicus, as far as the level of the anterior superior spinous process of the ileum. It then curved backwards, passing close to Poupart's ligament, after which it became untraceable. The whole length of the tumour from above downwards was ten inches and a half, as ascertained by percussion. His appetite was impaired, and he suffers from considerable diarrhœa, his bowels being moved in general about six times in the twenty-four hours. Pulse 70, of moderate strength. On examining his chest, percussion was found to be universally normal. On auscultation, there was increased vocal resonance and prolonged expiration under the right clavicle. Since the period of his admission he has remained in the same condition, and has been treated with astringents for the diarrhœa, and with leeches and counter-irritation for the pain in the tumour. He has also had opiate night-draughts when required. He was dismissed on the 11th of April, the tumour retaining the same dimensions, and the other symptoms continuing unaltered.

The blood was frequently examined microscopically, but was not found to contain an excess of colourless corpuscles. The coloured corpuscles were not aggregated together in rolls, but accumulated in masses, as in Fig. 1, p. 321, *a*, from increased spissitude of the liquor sanguinis.

Remarks.—Perhaps this case is as important as any which has yet been recorded, for it indicates to us a large splenic tumour forming independent of intermittent fever, while the individual presented the cachectic aspect, the epistaxis, dyspnœa, diarrhœa, and occasional feverishness, as in the most marked cases of leucocythemia. And yet repeated examination of the blood exhibited no increase in the number of colourless corpuscles, although, from the manner in which the coloured particles arranged themselves, there was evidently an increased amount of fibrin in it. It is certain, therefore, that the symptoms we have noticed are not essentially dependent on the leucocythemia. We have seen them absent in many instances where the blood disease was well characterised, and present where it was slight, or, as in the case just given, altogether absent.

(*To be concluded in a future No.*)

ARTICLE III.—*Case of Obstruction of the Bowels, caused by Swallowing a Leaden Bullet.* By G. V. CUMMING, M.D., Garrison-Surgeon, Vizagapatam.

AMONG the families of the garrison an interesting case occurred. A boy, aged 11, named Dixon, son of a European pensioner, was admitted into hospital on the 15th of October 1850, complaining of pain in one spot of the abdomen—a little below and to the left of the umbilicus—and which, he said, was occasioned by his having inadvertently, two days before, swallowed a piece of lead with which he had been amusing himself. The portion must have been of considerable size, as, in describing it, he said it was nearly as large as a half rupee, but thicker, consisting of two leaden pencils which he had melted into what he called a flat bullet. On the lead slipping down the pharynx he was nearly choked, but the mother, who happened to be present at the moment, said, that by rubbing the throat and upper part of the neck with her hands she succeeded in forcing it downwards. Instant relief was now experienced; but next morning the pain in the abdomen, of which he complained on admission, commenced. In the hope of expelling or facilitating the descent of the bullet, a dose of castor oil, with a few drops of laudanum, was at first prescribed, and a few leeches were applied to the pained part as a precautionary measure. Next morning, though the bowels were several times relieved, the pain, still in the same spot, seemed rather worse, but it was scarcely increased on pressure. The oil was now repeated, and hot fomentations were ordered. Several dejections were again obtained, but otherwise no perceptible benefit accrued. In the space of two days, however, the pain shifted its seat to midway between the umbilicus and the os pubis, showing that the bullet had gone lower down; the pain was also more diffused, occupying a space about the size of a watch. Slight fever now existed. Obstinate constipation also soon occurred, which, for nearly three days, resisted the frequent administration of croton oil, pills of calomel, colocynth, and scammony, two doses of jalap, and the use of active purgative clysters. Prior to the action of these medicines on the bowels, excessive irritability of the stomach set in, almost every fluid taken being immediately rejected, and the pyrexia previously noticed still continuing. Two days afterwards, however, the febrile heat was succeeded by an opposite state of the system,—one of great depression. Cold clammy sweats existed, and he had also frequently-recurring, but not severe, convulsive fits, each being easily removed for the time by dashing a little cold water on the patient's face. Under these circumstances the remedies chiefly resorted to were artificial heat and diffusible stimuli to rouse the system, effervescing draughts, and one or two doses of creasote to allay vomiting, with anodynes at night, sinapisms, and subsequently a blister to the epigastrium. To the abdomen hot fomentations

were from the first sedulously applied. The pulse, previously thready, became latterly so weak, that any attempt to raise the head was apt to induce syncope. Cramps, too, of the hands and feet supervened, and constipation again existed. In short, the symptoms were such as to excite an apprehension that poisonous effects from the lead had at last manifested themselves. The sulphate of soda, therefore, was given in solution for two days in small frequently repeated doses, along with a few drops of the tincture of henbane, but seemingly without any improvement, although several scanty evacuations were procured. Twelve days had elapsed since the patient's admission, when the idea fortunately suggested itself to me, that in the event of the bullet being obstructed in its farther descent, by any accumulation of hardened fæces, which the previous purgatives might not have removed, the introduction into the rectum of O'Beirne's tube might have a happy effect. The fixed pain at this period was situated just above the pubis. In the absence of the regular instrument, the elastic tube of Read's stomach-pump was inserted, and, with some difficulty, pushed up twelve inches, so that it must have passed through the greater part of the sigmoid flexure of the colon. Warm water was now freely injected, and a remarkably copious stool, nearly filling a chamber utensil and presenting scybala, was the result; but the bullet could not be found in the dejection. In the course of the day he passed another stool, containing a lumbricus, but without the bullet. Next morning the fixed abdominal pain had entirely gone. A dose of turpentine was now exhibited, and he passed at stool another worm, but still no trace of the bullet. On this day he had only two of the convulsive fits, which were the last. Under the use of wine and light nutritious diet, with no other medicine than merely a vegetable tonic and one or two purgatives, he rapidly improved in appearance; and on the 6th of November, twenty-one days after his admission, he was discharged from hospital quite well, excepting that he was a little weak. In this case my impression is, that the bullet must have passed at stool, either in the large evacuation obtained directly from the use of the tube, or in the others which succeeded it; but that, from the examinations not having been made with sufficient care, it escaped detection. Had the dejections been merely thrown out, subsequent attempts might have discovered the bullet. It seems, however, that, agreeably to the usual practice, they were all cast into the backwater, situated directly in the rear of the hospital, and thus removed.

Since writing the above, the boy has presented me with a bullet, made by himself at my request, which, he says, in size and shape is very similar to the one which he had swallowed. Of this, the drawing given in the margin conveys an accurate idea. It is a quarter of an inch in thickness.

VIZAGAPATAM, December 14, 1850.

ARTICLE IV.—*On Micrometers applied to Microscopes.* By WILLIAM ROBERTSON, M.D., F.R.C.P., Ed.

THE ordinary stage-micrometer, as constructed by the best English and foreign opticians, cannot be directly applied to the measurement of very minute objects. Although it can be procured at a moderate price, and with divisions beautifully ruled on glass at intervals of $\frac{1}{100}$th of a millimetre apart, the scale is far too coarse for the use of the histologist; and it is usually quite impossible, in examining certain objects under high magnifying powers, to bring their edges into proper focus while the ruled lines of the scale continue tolerably defined.

The eyepiece-micrometer, consisting of a scale ruled on glass, and inserted in the stop or diaphragm of the ordinary negative eyepiece, is a very convenient instrument, enabling the observer, when using a magnifying power of 500 or 600 diameters, to estimate spaces of about $\frac{1}{250}$th or $\frac{1}{300}$th of a millimetre with tolerable precision, in favourable circumstances. But the breadth of the lines on the best ruled eyepiece-scale is so considerable, and the shadows caused by their channels so perplexing, even when the illumination is carefully managed, that, where extreme accuracy is required, other apparatus must be employed.

The cobweb screw-micrometer, when well constructed, is a far more perfect instrument; but, as Mr Quekett remarks,[1] "the measurements made by it are by no means so delicate as they appear to be." In taking a unit, from which to construct the scale, a stage micrometer must be employed, and on the accuracy with which this is graduated depends, of course, the exactness of the subdivisions effected by means of the screw. This objection applies equally to all eyepiece-micrometers; but the screw-instrument has the positive disadvantages of being constructed of parts very apt to become deranged, and capable of being replaced by none but a first-rate workman. The effects of friction cannot be wholly obviated; the screw is apt to wear, and to wear unequally; and the uniformity of all its parts,—even when it leaves the workman's hands,—may be reasonably suspected. The price is necessarily so high as to preclude its general employment by those engaged in microscopic observations.

Welcker's Micrometer.—In Henle and Pfeufer's "Zeitschrift für Rationelle Medicin" (band X. heft 1), Hermann Welcker, a medical student at Giessen, proposes a new kind of micrometer, capable of furnishing indications of extreme delicacy, and in elegance of principle and cheapness far surpassing the cobweb screw-micrometer.

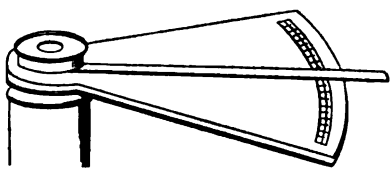
The following description will enable any one familiar with the

[1] Practical Treatise on the Use of the Microscope, p. 196.

elementary principles of trigonometry to comprehend the mode of constructing and using such an instrument:—

Construction.—Across the stop of an ordinary negative eyepiece, two very fine threads, from a small spider cocoon, are stretched at right angles to each other, and, by means of a little copal varnish, are fixed in such a position that the shorter intersects the longer thread, cutting off about one quarter of its length. The relative position of the threads is shown in Figs. II. and III., where they are indicated by the letters A B and C D. To the upper part of the tube of the microscope is fixed transversely a brass plate, along which plays a pointer, firmly attached to the eyepiece immediately beneath its milled rim. The appearance of this apparatus is shown on a reduced scale in Fig. I. Upon the edge of the brass plate is drawn

Fig. I.



an arc of a circle concentric with the eyepiece, and this arc is then subdivided into degrees, and any fractional parts which may be required.[1]

By experimenting with a stage micrometer, we next endeavour to ascertain how far the pointer must be moved, in order that the crossed thread shall traverse a space in the field corresponding to $\frac{1}{100}$th of a millimetre. By simply manipulating on the stage of the instrument, the stage micrometer can easily be put into the position shown in Fig. II., the long line A B accurately coinciding with a line of the micrometric scale. The eyepiece is then cautiously rotated, till the cross in the field, passing along the imaginary dotted circle in Fig. II., seems to touch the next line of the stage-scale, the long line now assuming the position *a b*. The arc traversed by the pointer during this rotation is then read off,—we shall suppose it an arc of 8^{0}. The *sine* of the corresponding arc of the dotted circle will, of course, indicate exactly $\frac{1}{100}$th of a millimetre; and from this simple foundation any measurement—*i. e.*, the length of the chord of any given arc in that circle—may be calculated; for the chord of any arc being equal to twice the sine of half that arc, the value of the chord of 8^{0} is found as follows:—

[1] This mode of division is preferable to a less exact method of constructing the scale, recommended by Welcker. The former involves, it is true, a little calculation in making measurements, but this disadvantage is more than compensated by the superior accuracy of the results obtained.

Proportion.

As sin. 8° : ·01 Millimetre : : 2 sin. 4° : chord 8°

Calculation.

Log. 2 sine 4°	=	9·1446145
Log. ·01 Millim.	=	$\bar{2}$·0000000
		7·1446145
Subtract Log. sine 8°	=	9·1435553
Chord 8° = Num. ·0100244 Millim.	=	$\bar{2}$·0010592

The same result may be obtained from the following proportion, both sine and chord being supposed drawn :—

Proportion.

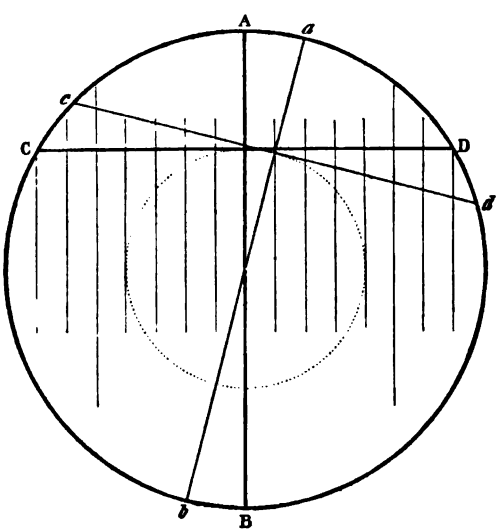
Rad. : ·01 Millim. : : secant 4° : chord required.

Calculation.

Log. secant 4°	=	10·0010592
Log. ·01 Millim.	=	$\bar{2}$·0000000
Subtract Log. Radius	=	10·0000000
Chord 8° = Num. ·0100244 Millim.	=	$\bar{2}$·0010592

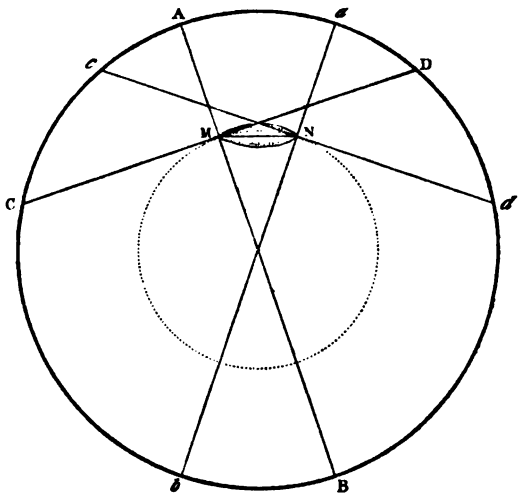
The equivalent number ·0100244 is the required value of the chord of 8° in fractional parts of a millimetre. This or some other chord having been carefully determined by the instrument-maker, or observer, it should be noted down, as essential for the exactitude of all measurements to be afterwards made. It will save trouble if its logarithm be also recorded.

Fig. II.



Mode of using the Instrument.—Suppose we wish to measure the long axis of an object, such as M N in Fig. III., we so arrange it on the stage that the cross in the field will, when the eyepiece is rotated, touch first the extremity M, and then the other extremity N. The arc A *a*, traversed by the pointer, is read off from

Fig. III.



the brass scale. The chord of the corresponding arc of the imaginary dotted circle is the measure of M N. Its length may be calculated with the greatest ease by the help of the ordinary logarithmic tables of sines. In comparing chords, we use the sines of half their including arcs, as in the example which is appended:—

EXAMPLE.—See Fig. III.

The arc A *a* is found = 38°
Required the length of M N.

Proportion.

Sine 4° : ·0100244 Millim. : : Sine 19° : M N.

Calculation.

Log. ·0100244 Millim.	=	$\bar{2}$·0010592
Log. sine 19°	=	9·5126419
		7·5137011
Subtract Log. sine 4°	=	8·8435845
M N. = Num. ·0467861 Millim.	=	$\bar{2}$·6701166

In like manner the length of any other chords of the dotted circle may be easily determined;[1] and a table—if required—drawn up,

[1] It is, however, unnecessary to carry these calculations beyond the fourth or fifth decimal place; for the unavoidable errors of observation render the apparent accuracy of the sixth and seventh places quite illusory.

from which the measure corresponding to each degree of the scale can, by mere inspection, be at once ascertained.

When the eyepiece rotates smoothly in the tube of the microscope, and a magnifying power of 500 or 600 diameters is used, measurements may be made with such an instrument with the utmost nicety. Welcker recommends that the top of the tube of the microscope should terminate in a hollow cone, into which is received a conical collar, supporting the pointer, and slipt on immediately beneath the milled rim of the eyepiece. The errors of manipulation should hardly exceed $\frac{1}{40,000}$th of an inch,—a degree of exactitude scarcely attainable by the cobweb screw-micrometer. An instrument constructed for me, by Mr James Bryson of Edinburgh, on the plan above described, has been tried against a finely-finished screw-micrometer, and found to perform with very great accuracy.

ARTICLE V.—*Case of Stricture of the Urethra treated by External Incision.* By JAMES SYME, Esq., Professor of Clinical Surgery in the University of Edinburgh.

(*Read before the Medico-Chirurgical Society of Edinburgh, 19th March* 1851.)

IN the billet of business for our last meeting, there was announced a case of "Perineal Section," by Mr Miller. Not being aware that he had ever performed the operation to which this foolish title has been applied, and naturally feeling desirous to know what part in the question at issue in regard to the treatment of stricture was to be taken by the Professor of Surgery, I regretted that circumstances prevented me from being present, especially when I afterwards learned that the case read was not Mr Miller's, but one of my own,—the whole responsibility of which rested upon myself. It appeared that the patient had written out a statement of his case, which was read to the Society without my sanction or knowledge; and having reason to believe, from what different members told me, that a very erroneous impression has thus been made, I applied to the secretary, who is usually supplied with the papers produced at meetings, for making out his abstract, which is recorded in the "minutes." Mr Miller not having given the case for this purpose, and being applied to on my own account, referred me to the patient, who, in reply to the request which I then transmitted, sent me the following letter:—

"March 10, 1851.

"DEAR SIR,—In compliance with your request, I readily transmit for your perusal the statement which I drew up of the circumstances attendant upon the operation which I underwent last year. Mr Miller could not, except with my sanction, have parted with the paper, it being a privileged communication; and being drawn up by an unprofessional person, and in untechnical language, prob-

ably might not bear strict criticism, although it faithfully represents my own ideas upon the subject.

"I beg you will understand that it is committed to you confidentially, and neither for publication nor public critique.—I am, &c.

"Professor Syme."

Considering the constitution and objects of the Medico-Chirurgical Society, I regard the precedent which would be established by allowing this procedure on the part of Mr Miller to pass unnoticed, as of the most dangerous character. Any man, not belonging to the medical profession, and not even acquainted with its language, might thus, without subjecting himself to criticism or censure, communicate to a body of practitioners, associated for their mutual improvement, his own fancies,—no matter how absurd, and calculated to produce the most erroneous impression in regard to modes of treatment, however beneficial they may have been to himself, or valuable for the relief of others. Nothing, in my opinion, could tend more to destroy the confidence existing between medical men and their patients, or impede the establishment of sound principles of practice, than the toleration of such a system. I, therefore, consider it right to express my entire disapprobation of this novelty in professional conduct, and will now read to the Society a statement of the case, for which they may hold *me* responsible.

In the end of January 1850, Mr Miller asked me to operate upon a gentleman, who had long suffered from stricture of the urethra, and seemed likely to perish from it, unless relieved. He was somewhat under forty years of age, during nearly twenty of which the disease had existed,—being at first attended with slight inconvenience, and having gradually become more troublesome, until at length it had affected his health and comfort in the most serious degree. In the first instance, he had been treated for several years by a surgeon in London, who employed wax bougies. He then came under the care of Mr Liston, who, during four years, vainly tried to dilate the contraction, which lay at the fore part of the bulb, by means of metallic instruments, never being able to get beyond No. 7, and instead of affording relief by their use, generally inducing an increase of irritation, with, on one occasion, a serious threatening of complete obstruction. After this, Mr Miller had tried to afford relief in the same way; but with no better success, and hence had been led to advise the operation I have recommended for such obstinate cases.

On the 31st of January, at four o'clock in the afternoon of a winter day, I first saw the patient, having been asked to examine him then under the influence of chloroform, and, if I deemed it expedient, thereafter to proceed with the operation. Having passed a small grooved director through the stricture, I divided it by external incision, and placed a catheter in the bladder. Everything went on most favourably for the next two days, and I then removed

the catheter. On the following day, I was called to see a case in Northumberland, and consequently did not revisit the patient until the fourth day after the operation, when I found him with a smiling countenance, clean tongue, soft quiet pulse, moist skin, and plentiful discharge of urine; but was entertained with a woful account of the terrible things that had happened since my last visit, the second day before. Suppression of urine, rigors, delirium, and collapse, were all most graphically described; but, when taken in connection with the patient's satisfactory aspect, so soon after their alleged occurrence, suggested to me no idea of danger either in prospect or retrospect. On the contrary, I concluded that everything was going on favourably, and that the patient, being an irritable, exacting, self-indulgent sort of person, had merely suffered rather more than usual from the nervous disturbance which is apt to attend the flow of urine, after the catheter has been removed, in such cases.

Accordingly all went on well, and, so far as is consistent with my own personal knowledge,[1] the only obstacle that intervened in the way of his recovery was a small abscess, which formed in the lower part of the scrotum, and was evacuated by incision eight days after the operation. I wished to pass a bougie occasionally; but was allowed to do so only once, when an instrument larger than the catheter employed was introduced without the slightest difficulty. The urine resumed its natural course almost entirely in the course of a month, and by the end of the following month the patient was dismissed from medical restraint. In July he afforded me an opportunity of passing Nos. 8 and 9, which were introduced with the utmost ease, and without any subsequent rigor. He was then, and still appears to be, in perfect health; and, I believe, considers himself completely relieved from a complaint which, but for the operation I performed, would have proved incurable. No. 12, I understand, can now be introduced without any difficulty; and, if the patient is not deeply grateful for the benefit he has received, he certainly ought to be so.

The points of this case which seem most deserving of attention are, in the first place, that a stricture of twenty years' duration, which had resisted prolonged treatment by different practitioners, and progressively increased in severity, with serious derangement of the general health, was completely remedied by external incision; and secondly, that twelve months having passed without any re-appearance of the peculiar characters denoting the irritable or contractile stricture, the relief thus obtained may be reasonably deemed perma-

[1] I have purposely abstained from taking any notice of an abscess, which was stated to have been discharged from the rectum, six weeks after the operation, since, whether "lumbar," according to one statement, or "intra-pelvien multilocular," according to another, it is in my opinion, when coupled with the patient's satisfactory recovery, no less inexplicable than another circumstance communicated to the Society,—viz., that the patient did not enjoy a wink of sleep for six weeks.

nent. The feelings and fancies of the patient during the period of recovery may have been intensely interesting to himself, and may have drawn largely on the kind nursing of his ordinary medical attendant, but are of no consequence whatever in regard to the general question, when compared with the effects usually experienced from the operation, now that the field of observation has become so extensive.

In conclusion, I beg to remark that the mode of treatment which I have proposed is intended for the relief, not of stricture in its ordinary form, which readily yields to dilatation, but of that which resists this and all other known means of remedy. In cases of this kind—than which there are few presented in the practice of surgery more distressing—any proposal for the means of escape, even though attended with some risk and uncertainty, might have been expected to meet with a favourable reception, credit being given for the good accomplished, and time afforded for the improvement of details requisite to promote its efficiency. The future history of the profession will determine how far the opposition to my proposal has been reasonable, honourable, or successful.

Unless a stricture resists dilatation, or speedily contracts after being dilated, I do not resort to division. Thus a few days ago I dismissed, with complete relief, through the use of bougies, a retired officer of the H.E.I.C.'s service, who came to Edinburgh, expecting that I would perform the operation. He had suffered severely for many years, and been under the care of different surgeons, one of whom, the late Mr Callaway of London, after inducing violent irritation and constitutional disturbance by ineffectual attempts to dilate the stricture, advised him never to allow an instrument to be introduced into his urethra, which, he said, was no less resisting than a "deal board." Yet, in this case, aggravated by duration and derangement of the health, from frequent attacks of ague, I met with no interruption in dilating the canal, and relieving the patient from every uncomfortable feeling.

In circumstances requiring the operation, I have maintained that it is always practicable, perfectly safe, and adequate to afford relief, not only complete, but permanent. As these positions have all been denied, I may say a few words in regard to each of them;—and first as to the permeability of strictures. The opponents of my proposal say that strictures are not unfrequently impermeable, so as to resist the entrance of a director for the knife. To this I reply, that there is no such difficulty that may not be overcome; and when told that this term implies arrogant over-confidence on my part, I ask to be shown an impermeable stricture. Mr Miller, while acting as a surgeon of the Royal Infirmary, I believe met with some such cases, and if, instead of resorting to extreme measures, he had tested my power to deal with them as permeable, and found the issue unsuccessful, he might then indeed have come to the Society with some show of evidence, against my fundamental principle. But as it is,

neither he nor any one, who does not so convict me of undue confidence, has any title to impute it in opposition to the large experience I am now able to produce.

As to the danger of the operation, I contend that if the incision be made in the mesial line of the perineum, there is no vessel of the slightest consequence that can be cut; and that, if the deep fascia of the perineum is not divided, which it never requires to be, there cannot take place extravasation of urine, unless perhaps forwards in a slight degree into the scrotum, where it may occasion a little suppuration, utterly unworthy of attention, when compared with the great object in view. I therefore maintain, that the operation is absolutely free from danger; and appeal to the fact, that I have performed it between forty and fifty times without a single fatal result.

With regard to the permanence of effect, I have ascertained beyond question, that even in the most severe and obstinate forms of the disease, complete and permanent relief may be afforded by the operation. But my opponents would seem as if they had understood me to say, that it *must* produce this effect, and have eagerly endeavoured to hunt out some case of relapse, as a complete extinguisher of the procedure. Now it is really difficult to imagine anything more unfair or illogical than this proceeding on their part, since, independently of all other circumstances calculated to influence the result, it is plain that any imperfection in the operation itself may prevent the result from being successful. A single instance of unequivocal success should therefore counterbalance almost any amount of partial or complete failure. But, in point of fact, the result of my practice has been entirely favourable, with hardly any exceptions, and these I doubt not might have been avoided with the assistance of more extended experience and knowledge of the circumstances adverse to complete attainment of the object in view. The precise extent to which the stricture requires to be divided, does not appear to be the same in all cases; and I have also generally remarked, that when the wound of the urethra heals by granulation, instead of the first intention, there is less risk of future trouble. In the "Lancet," there lately appeared a letter recommending attention to this circumstance, as the sure means of affording permanent relief, but without any mention of the fact, that the author, being one of the pupils of my class, had heard me express a suspicion to this effect in my lectures, and witnessed a case treated in accordance with it. Upon these, and various other points connected with the operation, I have been carefully making observations for several years past; and when they have been tested by time, so as to be matured from crude speculations into trustworthy principles, I shall not fail to bring them before the notice of the profession.

ARTICLE V.—*General Observations on the Roman Medicine-Stamps found in Great Britain.* By J. Y. SIMPSON, M.D., Professor of Midwifery in the University of Edinburgh. (*Concluded from p. 255.*)

IN terminating these hurried notices regarding the Roman medicine-stamps that have been discovered in the British islands, I shall offer in the present communication a few general observations on the subject; and these must necessarily be limited in a medical Journal such as the present.[1]

Before, however, entering on these general remarks, I may observe that, since the last communication on the same subject was published, I have, through the kindness of Mr Voelcker, of Cirencester, received an additional and undescribed specimen of a Roman medicine-stamp, found in Gloucestershire. The stamp is a small square stone, cut on three of its sides with legends, bearing the arrangement and punctuation which follow:—

1. JULICVNDI
   COLY.MELINU
2. JULICVNDI
   COLSTACTV
3. JULIVCVNDI
   COLLYR.PENC

The name of the practitioner is evidently JUL*ius* JUCUNDUS.

The name of each prescription is preceded by the term *collyrium;* an arrangement which we have not previously met with.

We have already had occasion to consider more than once the nature of the collyrium MELINU*m* which is inscribed on the first side.

The COL*lyrium* STACTU*m* of the second side has also formerly come under our consideration, in reference to stamp No. IV.

The third side of the stamp contains an inscription COLLYR*ium* PENE*cillum*, which is interesting and important in reference to the observations that I have previously offered in regard to the nature of

---

[1] In order to shorten and conclude this communication, I have omitted a necessarily long chapter showing that the stamps were, in some instances, used to impress the soft substance of collyria, as these collyria were becoming dried up into solid cakes by the addition of gum and starch, and subsequent exposure *sub umbra;* and that in other instances, they were employed to stamp the labels (pittacia) attached to the vessels containing those collyria which were permanently liquid. For the same reason, I have set aside other sections on the high price of some of the old collyria; on the status of the Roman oculists; on the probable date of the engraving on the stones, &c.

the inscription *Penicillum*, as found upon stamp No. VI., and upon other Roman medicine-seals.

When speaking previously of the meaning of this term *Penicillum* (see stamp No. VI.), I ventured to suggest that the word did not mean a brush, or pledget of lint, or sponge, as it had been variously held to do by Grivaud, Sichel, and Duchelais; but probably signified the eye-collyrium, lotion, or ointment, that was to be used with one or other of these bodies. In other words, I offered as a probable conjecture, that the *penicillum* was a term applied to a collyrium, lotion, or salve, in consequence of the special mode in which it was to be applied to the eye, in the same way as eye-drops passed under the special name of *stactum*, from the special mode in which they were applied to the organ. This new stamp shows the truth of the conjecture which I thus offered; for it proves that the name *Penicillum* was used upon the stamps as the name of a collyrium (*Collyrium Penicillum*). And further, we have one of the other sides of this stamp, bearing the legend *collyrium stactum* upon it, so far affording additional analogous evidence of the true signification of these two terms, *Penicillum* and *Stactum*, and showing us that they were both of them designations of special collyria, derived from the manner merely in which they were used and applied to the diseased eye.

MEDICINES MENTIONED IN THE STAMP INSCRIPTIONS.

In our list of British stamps, various drugs, both vegetable and mineral, are named as used by the Roman practitioners in eye-diseases; these drugs giving the specific names to the special collyria. On the stamps we have described, we have in this way mentioned as eye-medicines from the vegetable kingdom, saffron, myrrh, frankincense, opobalsam, and spikenard; and as ingredients from the mineral kingdom, scales of copper, cerussa, psoricum, and misy. Each of the collyria, it is almost unnecessary to add, contained many other ingredients.

Some medicines entered into the composition of the collyria, far more frequently than others. Opium, with various practitioners, seems to have been a favourite ingredient. In describing the collyrium *Diaglaucium*, Scribonius Largus states, that he increases its good effects, by mixing opium with it, and that he makes this addition to almost every collyrium and drug. "Hoc enim ego adjicio, et ita melius respondet, scilicet opium, et in hoc et in omni collyrio medicamentoque."[1]

Many of the medicines which were used by the older oculists are now properly neglected. But one or two of them seem not undeserving of notice. They employed, as we have seen, various metallic ingredients in their collyria. One of these, arsenic, is repeatedly

[1] Medicæ Artis Principes. Cap. iii. p. 197.

mentioned in their writings; but is scarcely, I believe, if at all, employed by modern oculists. Some of their prescriptions contained remedies, that were supposed to have particular therapeutic effects, which modern ophthalmic physicians scarcely venture to aim at. Galen, for example, gives the collyrium of the oculist Hero, as capable of producing sleep, in wakefulness arising from pains and fluxions:—"Heronis ophthalmici psitaces, ad dolores et fluxum largum, e vestigio somnum inducit."[1] This prescription is remarkable as containing mandragora,—a drug which has an undoubted local anodyne effect, if we may believe the testimony of many ancient authors,—and one, the inhalation of the fumes of which, was alleged to be capable of producing a general anæsthetic effect.[2] In treating of inunctions for the eye, Galen himself recommends mandragora, for the purpose of acting as an anodyne. "If," he says, "wakefulness is urgent, reduce equal parts of the root and juice of mandragora, and the polenta of the poppy, into a mass, and give this mass to be smelt [or inhaled], and the patient will sleep,"—("eamque odorandum exhibeto et obdormiscent.")[3]

Certainly some of the eye-collyria described by the ancient Greek and Roman authors, contain a great farrago of different articles; and Galen, Celsus, Marcellus, &c., recommend occasionally individual drugs and ingredients, which would scarcely be considered as orthodox by any modern oculist—such as, the blood of pigeons and other different animals—the urine, gall, and stercora, &c., of others. Thus, when describing the treatment of ecchymosis of the eye or eyelids from blows, Celsus observes, "There is nothing more suited for this case than anointing the part with the blood of a pigeon, or of a ringdove, or of a swallow. And this practice is not without reason; for when the sight of these birds is injured by any accident, it becomes in a short time restored, that of the swallow most speedily. Hence is the ground of the fable, that their parents effect the cure by an herb, though in reality the cure is spontaneous. The blood of these birds, therefore," adds Celsus, "is a very appropriate remedy for our eyes in external injuries; and in these different degrees of efficacy, the blood of the swallow is best; next in order that of the ringdove; and that of the pigeon is the least medicinal of the three, either to itself or to us."[4]

---

[1] Kuhn's Edition of Galen. Vol. xii. p. 745.

[2] On the alleged anæsthetic effects of the swallowing and inhalation of mandragora, when used before surgical operations, &c.—See *Pliny*, book xxv. chap. 13; *Dioscorides*, book iv. chap. 76. Theodoric and Parè mention its anæsthetic powers in more modern times.—(See *Monthly Journal* for December 1847, p. 451.) Mandragora (says Dr Quincy) "is of a strong narcotic quality, and is mentioned in some ancient writers as convenient to be given before an amputation, to deaden the sense of pain by stupifying the patient; but modern practice owns it not for any such use."—*English Dispensatory* (1718), p. 204.

[3] Kuhn's Edition of Galen. Vol. xii. p. 794.

[4] Milligan's Celsus. Book vi. cap. vi. p. 298.

In his work "De Remediis Parabilibus," Galen has given a variety of recipes for various diseases and ailments. Amongst others he gives a number of succinct prescriptions for different eye-diseases, and some of these are illustrative of our present object of remark. He recommends for instance the blood of the pigeon in incipient ophthalmia ;[1] and in cicatrices and albugines he gives as separate receipts for these lesions, the inunction of the diseased organ with the gall of the kid,[2] or with the gall of the lion mixed with barley,[3] or with the gall of the sea-dog or fowl,[4] or with a collyrium formed of the stercus or dung of the lizard, mixed with water ;[5] and if a young infant boy has albugo, "his mother (says he) may anoint his eye with the catamenial blood of any relation, or let her cut her little finger, and besmear the eye with the blood which flows from it."[6] In a different part of the same work, Galen notices, as a medicine proved by experience as very efficacious in the treatment for the same disease of the eye (ad albugines medicamentum comprobatum, quod multos juvit), a liniment made of the gall of the male partridge, mixed with honey (p. 497) ; and he gives as an approved medicine (medicamentum probatum) for the same, a collyrium made of laurel-berries and gum, dissolved, when required for use, in the urine of a young boy.[7] For cataracts, new and old (suffusiones novas veteresque), he offers in a succeeding page (p. 498), a collyrium composed of the

---

[1] Ad ophthalmiam incipientem. Succum menthæ infunde, resolvit enim ; sed melior est *sanguis columbi*.—Keuhn's Galen, vol xiv. p. 408.

[2] Felle hirci confrica oculum.—Ib. p. 411.

[3] Fel leonis cum melle indito.—Ib. p. 412.

[4] Felle canis marini unge, vel felle galli.—P. 412.

[5] Ex stercore lacertæ faciens collyrium cum aqua indito.—Ib. p. 412. In nyctalopia he elsewhere (p. 350), recommends the eye to be anointed with "expressum ex recenti stercore asinino succum."

[6] Si vero puer parvus habet albuginem, mater infantis ungat oculum menstruo alicujus de cognatione sua ; et similiter mater ejus incidat parvum digitum suum, et sanguine qui effluxerit perfundat oculum.—*Galen*, vol. xiv. p. 412. Nicolaeus Myrepsus gives the two following formulæ—"ad crassas albugines oculorum." "Aut sanguinem columbæ recentem, sub alio extractum, in oculum stillato. Aut pueri incorrupti sanguinem e digito in oculum stillato." —*De Collyriis*, p. 666. In his treatment of nyctalopia, Paulus Aegineta offers the following recipe :—"Having roasted the liver of a goat, collect the ichor during the roasting, and anoint, but give the liver itself to eat ; or boil, and direct the vapours to be received with open eyes (vol. i. p. 420). In cicatrices and leucomata, the same author recommends us, among other things, to "levigate the dung of the land crocodile with water, and anoint, having formed it into a collyrium."—*Dr Adams' Translation*, vol. i. p. 418.

[7] Recipe baccarum lauri mundatarum laevigatarumque gummi quod satis sit ; ex eis permixtis confice collyria ; at ubi eis uti volueris terito et ex urina pueri impubis illinito.—*Keuhn's Galen*, vol. xiv., p. 497. The urine of boys not come to puberty is (according to Dioscorides, Galen, Paulus Aegineta, &c.) more medicinal, because stronger, than that of men.—See *Dr Adams' Paulus Aegineta*, vol. iii. p. 289.

same ingredients, and dissolved in "urina pueri nondum experti venere."[1]

Some of my professional brethren, may, perhaps, be inclined to look with pity and contempt upon the practical knowledge of the ancient Greek and Roman physicians, in consequence of their employing such ingredients and prescriptions as I have just now mentioned. But before passing too severe a censure upon their credulity in these respects, we may well humble ourselves by recollecting, that it is not above a century or two ago, since many similar ingredients, and others equally or even more revolting and ridiculous, held a place in the Pharmacopœias of Great Britain. The first Pharmacopœia, for example, of the Edinburgh Royal College of Physicians, published in 1699 (one hundred and fifty-two years ago), contains among the list of medicines required to be kept by the druggists of those days, no less than nine varieties of stercus—the "urina pueri impuberis,"—the powder of the skull of a man who has died a violent death (cranium hominis violenta morte extincti) —the powder of the mummified Egyptians (Mumia)[2]—the human afterbirth—(Secundina Humana), &c., &c. And some of the prescriptions contained in this Pharmacopœia, consisted of as numerous and as absurd ingredients, as can be easily found in any of the old Greek and Roman authors.

Nor must the English reader suppose that such barbarous drugs and medicines were used in the 17th century only on the Scottish side of the Tweed. The third edition of the Pharmacopœia of the Royal College of Physicians of London was published in 1627, and the printer of it, John Marriott, assures us in a final note, that this edition is "freed from all errors by the great labor, paines, care, and industry of that Houourable Society, the authors of the Worke, and by them intending, only the publick good, diligently and truly corrected and amended, as also newly amplified, enlarged, and adorned with such additions as unto them seemed most needfull."

The edition of the London Pharmacopœia thus so proudly announced, and which was accompanied by "his Majestie's Royall Proclamation, commanding all Apothecaries of this Realme to follow it," contains, if possible, a still more curious and extravagant collection of extraordinary articles in the animal materia-medica,

---

[1] Perhaps some of my readers will remember the story told by Herodotus, of Phaeron being at last restored to sight by washing his eyes, as the oracle had ordered him, with the urine of a married woman who had never proved faithless to her husband;—the alleged extent of his investigations before he could find the material prescribed;—and his consequent immolation of his wife, and of the numerous other women whose urine did not produce the expected effect.

[2] Bellonius tells us that Francis I. never ventured to travel without doses of rhubarb and mummy (*Observat:* p. 118). Mummy was used against apoplexy, palsy, &c. But (argues Alstan, when treating of this article in the Pharmacopœia) "human flesh is not better than, if so good as, a hog's."—*Materia Medica*, vol. ii. p. 544.

than even those enumerated in the Edinburgh Pharmacopœia. In its catalogue of simples it includes, for example, as medicinal remedies, eleven varieties of excrements,[1] five varieties of urine,[2] fourteen kinds of blood,[3] seven kinds of gall,[4] saliva and sweat (saliva et sudor), the sperm of the frog and seal; mummified men (mumia sepulchrorum, pissaphaltum); the os triquetrum from the human skull; old castrated cats (catus senex emasculatus); ants (formicae); crickets (cicadæ); millipedes; (bombyces); &c. And its compound remedies and formulæ are frequently as singularly multifarious. A number of them have ten, twenty, and more articles entering into their composition. One of them, the "Antidoton Magnum Matthioli adversus Venena et Pestem" (p. 80, 81, and 82), contains alone upwards of one hundred separate ingredients.

When we reflect on such facts in the modern history of medicine, we shall perhaps feel inclined to judge more charitably of the apparent extravagance and oddity of the means occasionally used by the ancient physicians in the treatment of ophthalmic and other diseases. And perhaps, some century or two hence, our successors, when using, principally or solely, concentrated therapeutic ingredients, revealed to them by a higher and much more refined chemistry than ours,—will look back upon our present massive and clumsy doses of vegetable powders, bulky salts, nauseous decoctions, &c., with as much wonderment and surprise as we now look back upon the preceding therapeutic means of our ancestors.

Before leaving the subject of the present chapter, I would wish to make one more observation. The Romans brought along with them

---

[1] *Stercus* humanum, lupinum, murium, onillum, caprinum, columbinum, equinum, gallinaceum, hirundinum, vaccinum, caninum sive album græcum. Dr Quincy, in his "Pharmacopeia Officinalis" (London, 1718), describes the "Stercus canis officinalis, or dog's white dung," and the "Stercus equi non castrati, or stone horse-dung," among his class or section of Balsamicks. The latter, the stercus equi, "seems (he says) to owe its present credit in medicine to *modern* practice." He speaks of it as "certainly of great efficacy" in pleurisies and asthmas. "In all these intentions it is *now* very much prescribed." He recommends it to be given in decoction, and the medium may vary. "But (he adds) whatsoever it is infused in, I never observed it the less effectual for clarification, and that will bring the liquor to look fine, and be less nauseous, although it will not destroy its natural smell, but the patient may still discern it."—p. 149. A few pages further on (p. 160) he states that the Stercus Porcinum, which is "greatly detersive and diuretic," "may be managed into a form as cleanly and palateable as the horse-dung already spoken of, but hardly so as to be concealed from the patient." Most heartily for once do we agree with Culpepper, who, when speaking of the Stercora of the College Pharmacopœia, observes,—"But I will let them alone, for fear the more I stir them the more they stink."—*London Dispensatory*, p. 31.

[2] *Urina* hominis, apri, capri, canis, tauri.

[3] *Sanguis* hircinus, humanus, anserinus, caninus, caprinus, cervinus, columbinus, leporis, perdicis, vespertilionum, suis, taxi, tauri, testudinis.

[4] *Fel* taurinum, vaccinum, accipitris, milui, caprinum, suillum, leporinum.

in their conquests, and diffused among the nations they subdued, a knowledge of the various arts of civilised life, and, no doubt, among others, a knowledge of medicine. Some facts would seem to show that they introduced for growth into Great Britain for their own use, and for the use of those amidst whom they dwelt, various medicinal plants that were used in those times. Perhaps the scientific botanist may yet be able to show that various plants more common in the old sites of the Roman power in Britain, are not really indigenous, but introduced, species. But in reference to this subject I must content myself with quoting from the old work of Camden a passage, in which he informs us, that in former days the Scottish herbalists used to go a-simpling after Roman medical plants. In summing up his description of the Roman wall of Hadrian in Cumberland and Northumberland, he concludes by observing:—"I cannot conceal from the reader this circumstance, which I had from persons of credit: A fixed tradition remains in the neighbourhood, that the Roman garrisons on the borders, planted here, up and down, for their own use, many plants good for curing wounds. Hence some pretenders to Surgery in Scotland resort here every summer to collect plants, whose virtues they have learned by some practice, and extol them as of sovereign efficacy."[1]

CHARACTER OF THE EYE DISEASES MENTIONED IN THE STAMP LEGENDS.

It is not without interest to turn and consider for a moment the special diseases of the eyes which are mentioned and indicated on the Roman medicine-stamps. In the list of stamps which I have given, as found in the British islands, the following eye diseases are enumerated:—1. *Diathesis (oculorum)*; 2. *Omnis Dolor*; 3. *Caligo (ad Claritatem)*; 4. *Asperitudines*; 5. *Lippitudo*; 6. *Suppuratio*; 7. *Albugo*; 8. *Cicatrices*; 9. *Veteres Cicatrices*.

In reference to the preceding list, the following circumstances seem worthy of observation:—*First*. A number of general terms are used, such as *dolor*, *diathesis*, *caligines*, which may signify impairment of vision from any diseased state of the organ, whether inflammatory or not. But, *secondly*, with these exceptions, all the other diseases mentioned upon the oculist-stamps of Britain, and I think even still more markedly, all those mentioned upon the oculist-stamps found in Gaul and Germany, signify and imply acute or chronic inflammations of the eye or eyelids, or some of their results.

Indeed, the terms used upon the medicine-stamps seem to show that the Roman colonists and subjects in Western Europe suffered in those olden times from ophthalmia in its various forms and results in an extreme degree; and that the different varieties of these affections constituted probably one of the most common maladies to

[1] Camden's Britannia, vol. iii., p. 214.

which the Roman colonists and subjects of these days were liable. The frequency with which the terms *cicatrices*, *albugo*, &c., occur on the oculist-stamps, indicates further, that these diseases were often allowed to run on to their ultimate and more destructive stages before they were attempted to be arrested by medical art. Some facts would also seem to show, that the attacks of ophthalmia were sufficiently serious and destructive, even when watched and treated from the first. At all events, if we are entitled to judge of the severity of these ophthalmic diseases by the severity of the measures sometimes employed against them, we should feel ourselves forced to come to this conclusion. For, in treating the attacks of ophthalmia, the Greek and Roman practitioners sometimes used remedies, of a nature so very formidable and severe, as could alone be justified by the desperate character of the disease, and the unhappy results in which it otherwise usually terminated. Thus Paulus Aegineta, Celsus, and others recommend, in the treatment of chronic ophthalmias and discharges, incisions of the scalp, sometimes great in number, sometimes great in extent. Celsus talks of making nine incisions at once, and then converting them into issues. In some cases he thinks the bones of the cranium should be exposed and made to exfoliate. Paulus Aegineta speaks of stretching one deep incision over the head from ear to ear; or of making two or three long straight incisions on the forehead, &c., and pushing a spatula and knife subsequently beneath the scalp uniting them, in order to cut through all the vessels which descend from the head to the eyes, and thus sparing the external skin alone.[1]

SIGNIFICATION OF THE NAMES UPON THE MEDICINE-STAMPS.

Some have supposed that the names of the practitioners engraved upon these stones were not the names of those that used them, but the names of physicians or oculists who were celebrated at the time. M. Duchelais has endeavoured in particular to support this argument, but it seems to be entirely without anything like an adequate foundation. The name of the practitioner on the stamp appears to be always that of the individual who possessed the stamp; and if he happened to be stamping with it the collyria or drugs of any celebrated and known physician, he put in addition the name of that physician, as in the instance, amongst others, of the stamp No. II., where we have the *Crocodes Paccianum* (the crocodes of Paccius),[2] as

[1] See his account of Hypospathismus and Periscyphismus in his Surgery of the Eye, in *Dr Adams' Translation*, vol. ii., p. 256 and 258.—*Milligan's Celsus*, lib. vii., 14, p. 350.

[2] Paccius lived about the commencement of the Christian era, and is said to have amassed a fortune by the sale of a secret nostrum. He bequeathed the

prepared and sold by the proprietor of the stamp,—viz., *Sextus Julius Sedatus*. Further, that *Sedatus* was the proprietor of the stamp, is in this, as in every other instance, amply shown by the name recurring on every separate side of the seal, and in connection with every formula upon it.

Some practitioners no doubt possessed several stamps of this description, though commonly, amidst the lack of such remains, it has seldom happened that more than one has been discovered belonging to the same individual. Some exceptions, however, to this general remark are known. Thus, at the town of Nais in France (the ancient Nasium), no less than five or six medicine-stamps have been found bearing the name of *Q. Junius Taurus;* and with the separate sides of each filled with the appellation and uses of various and different collyria used and vended by *Taurus*, each side or legend bearing also the name of this oculist, as the proprietor of the stamps and the vendor of the different collyria inscribed upon them.

DOUBLE NAME ON SOME OF THE STAMPS.

Occasionally more than one proprietor's name occurs upon different sides of the same stamp. This fact we have already pointed out, as illustrated in the stamp described as No. VII. In this instance the figure of the stamp and engraving show, that originally it had appertained to L. JUL. IVENIS, and that afterwards it had come into the possession of, and had been used by, F. SECUNDUS. The engraving of the names of JUVENIS and SECUNDUS are so different in their characters, as to show very clearly that they have been executed by different hands,—that of SECUNDUS being done with unusual rudeness, and obviously by a hand not much accustomed to guide the burin. In this, and in similar instances, the stamp, after having been used by one practitioner, had evidently passed into the possession of another, and, it may be, of the successor to his practice. One amongst several stamps inscribed with the name of Q. JUNIUS TAURUS, and discovered at Nais, bears upon two of its sides a different name, viz., that of L. CL. MARTINUS, and inscribed in a manner calculated to show, that in all probability TAURUS had been his successor. The same stones were thus occasionally used by different practitioners, and one inscription obliterated and replaced by another, probably in consequence of the stone itself, of which the stamp was composed, not being readily accessible.

---

prescription at his death to the Emperor Tiberius, who placed a copy of it in the public libraries. (See *Scribonius Largus*, in Prin. Artis Medicæ, p. 209; Marcellus Empiricus, p. 324.)

LIST OF THE PRACTITIONERS INSCRIBED ON THE STAMPS.

The thirteen British stamps which I have described contain the names of twelve medical practitioners. This brief list is of course by no means adequate to serve as a Medical Directory of the physicians who practised in these islands during the Roman supremacy; but still the following list is not altogether without some interest:[1]—

T. VINDAC: ARIOVISTUS;—*Kenchester.*
J. B. CLEMENS;—*Wroxeter.*
T. JUNIANUS;—*Bath.*
L. JUL: IVENIS;—*London.*[2]
JUL: JUCUNDUS;—*Gloucestershire.*
MINERVALIS;—*Cirencester.*
Q. JUL: MURRANUS;—*Colchester.*
M. JUL: SATYRUS,
F. SECUNDUS,
S. J. SEDATUS,
} *London.*
M. JUVEN: TUTIANUS;—*Golden,* Ireland.
L. VALLATINUS;—*Tranent,* Scotland.

Two of these names occur among those names of the manufacturers of Roman pottery, that we find stamped upon the pottery ware dug up at the different Roman stations throughout England,—namely, the rare name IVENIS, or, as it probably ought to stand, JUENIS, and the common Roman cognomen of SECUNDUS. In Mr Wellbeloved's list of Roman potters, as furnished by vessels and fragments of Samian ware found at York, both of these names occur.[3]

---

[1] We have evidence regarding the name and functions of another Roman physician who practised in Great Britain; and that evidence is, in more than one respect, of an interesting kind. Some years ago there was dug up at Housesteads, in Northumberland, a monumental tablet, raised to the memory of a young Roman physician, ANICIUS INGENUUS, who had died at the early age of 25. The monument is remarkable and important, as affording evidence that the Roman armies of old were provided with military surgeons. For the inscription shows that this rediscovered monument to INGENUUS had been raised over his remains by the Tungrian Cohort (a part of the Roman troops that played a conspicuous part in Agricola's invasion of Scotland), and that this young physician was the "Medicus Ordinarius" to that cohort. This curious monumental tablet is deposited in the Antiquarian Museum of Newcastle. The inscription runs as follows:—"D. M. ANICIO INGENUO MEDICO ORD. COH. I. TUNGR. VIX AN XXV."

[2] I have entered four of the stamps as pertaining to London, not upon any evidence that these stamps were found there, but for the perhaps inadequate reason that the locality in which they were originally discovered is unknown, and that the *first* notice of them which we have is their existence in London collections. Three of them are in the British Museum.

[3] Journal of the British Archæological Association, vol. iii., p. 124.

This last name (*Secundus*) occurs also in the list of the Colchester potter's stamps,[1] and in Mr Roach Smith's catalogue of those found in London.[2]

Gruter's great work, "Inscriptiones Antiquæ Romanæ" (which is in itself a kind of directory for the old Roman world), shows us that most of the other names on the British stamps,—namely, *Clemens*, *Junianus*, *Jucundus*, *Minervalis*, *Murranus*, *Satyrus*, *Secundus*, *Sedatus*, and *Tutianus*, were all cognomens more or less common throughout Italy and the Roman colonies.

The name *Vallatinus*, which occurs on the Tranent stamp, is not to be found in Gruter, nor does it occur in any of the lists of potter's stamps with which I am acquainted; but its construction and form would seem to point it out as a cognomen sufficiently classical and Roman in its character.

I have already, in a preceding page, adverted to the name *Ariovistus*, as being identical with that of the great German leader, who so strenuously opposed the invasion of his country by Cæsar. This identity may perhaps entitle us to imagine that the Kenchester oculist was also of German origin. With this exception, however, the other names occurring in the British medicine-stamps afford us little or no clue to the probable nation or country to which the medical practitioners possessing them pertained. The names indeed, are in all other respects classical enough to allow us to suppose, that the oculists who dwelt and practised in this part of the world in these olden times, were of pure Roman or Italian birth; but we have ample reason also for knowing that this is not a sufficient test for determining such a question, inasmuch as it not unfrequently happened that the native subjects in the Roman colonies, assumed the gens names of their Roman conquerors. But leaving, in the meantime, this question out of view, as one not easily determined, let me remark that I can adduce one slight but most satisfactory piece of evidence, to prove that the fame of one British oculist at least was not unknown even in Rome itself, in the latter part of the second century of the Christian era.

In his numerous works, Galen speaks of many different physicians and many different oculists, but of almost none, either of his predecessors or of his contemporaries, does he speak in terms of commendation. Few circumstances strike the reader more, in looking over the works of this great writer, than the fact of the evident want of liberal and generous feeling with which he seems to have looked upon his professional brethren. Indeed, I do not remember any instance in which he speaks of a person being specially distinguished in any one special branch of medicine, except in the single instance to which I now wish to refer.

When giving formulæ for the prescriptions of various ocu-

[1] See Mr Acton's list in the Collectanea Antiqua, p. 40.
[2] See Akerman's Archæological Index, p. 89.

lists whom he names, Galen gives, amongst others, prescriptions for one or two collyria used by STOLUS; and this oculist he terms Ἄξιος (*Celebris.*) The ophthalmic physician to whom he pays this (on the part of Galen) very high and unique compliment, is to us interesting and remarkable, as being British, for he terms him

ΣΤΟΛΟΣ ΑΧΙΟΣ ΟΦΘΑΛΜΙΚΟΣ ΒΡΕΤΤΑΝΙΚΟΣ.

In these few words we have evidence left on record to show, that at the period in which Galen wrote,—that is, some seventeen hundred years ago, a British oculist occupied a position which made him confessedly celebrated in his time. The name of the practitioner ΣΤΟΛΟΣ appears sufficiently barbaric to indicate his British origin, though the context of Galen does not state whether STOLUS was celebrated as an oculist practising in Britain, or as a British native practising at Rome. In either case, however, it is not uninteresting as a point of antiquarian curiosity, and not unimportant in relation to ophthalmic practice in Great Britain, thus to find a British medical practitioner described as *celebris* some sixteen centuries ago. Besides, so far as I know, STOLUS is the first British medical practitioner mentioned in the pages of medical history; and even in this point of view, the unique compliment paid him by Galen may not be uninteresting to his many living successors.

Galen cites the name of Stolus in giving two formulæ for the *Collyrium Cinnabarium*, employed by this British oculist. I shall quote the whole passage, because it is brief, and affords us a sufficiently good instance of the kind of formulæ for eye-collyria, contained in the writings of the old Greek and Roman physicians,—an example of which in full I have not yet taken an opportunity of citing.

"Collyrium Cinnabarium celebris ophthalmici Stoli Brittannici, ad corrosos angulos, ophthalmias ad malum habitum perductas, intensos pruritus et inveteratos affectus."

"℞. Psorici drach. viginti-quatuor, cadmiæ drach. viginti-quatuor, croci drach. xvj., piperis albi drach. sedecim, cinnabaris drach. sedecim, opii drach. duodecim, gummi drach. viginti-quatuor, aquæ pluviæ quod satis est. *Aliud.* ℞. Psorici drach. viginti-quatuor, piperis drach. viginti-quatuor, cadmiæ, drach. xxiv.; croci ʒxxiv.; sandarachæ drach. octo, opii drach. octo, cinnabaris drach. octo, gummi drach. octo, aqua pluviali excipito."[1]

Medical antiquaries are not agreed as to what the substance termed *cinnabar* in the above and in other old prescriptions actually signifies.[2] Some believe that the so-called *cinnabar* of Greek and

[1] Keuhn's edition of Galen, vol. xii., p. 786.

[2] See, on the literature and difference of opinion relative to this subject, Dr Adams' learned Appendix of the Greek Names of Minerals, Plants, &c., in Dunbar's Greek Lexicon.

Roman authors was the dragon's blood, or *Sanguis Draconis*,—a resinous substance obtained from some of the palm tribe, and particularly from palms of the genus *Calamus*. Others hold that the term *Cinnabar* often signified in ancient time, as it signifies now, a mercurial preparation—the native sulphuret of mercury. We have already seen sufficient evidence, that the formulæ for eye-collyria in these olden times often contained various metallic preparations from copper, lead, &c.; and it seems in the highest degree probable that the cinnabar in this prescription of STOLUS was not the vegetable Sanguis Draconis, but was the metallic mercurial preparation which we still know under the same name; for Galen describes it in the same volume of his works, in his chapter "De *Metallicis* Medicamentis," vol. xii., p. 208.[1]

If this opinion be true, we have then the first British practitioner, whose name occurs in medical history, specially noticed and recorded by Galen as using a mineral medicine, which most of his professional descendants in later times acknowledge and employ as one of the most potent remedies to be found in the whole range of the materia medica, and one especially useful too, in some of the most destructive forms of ophthalmic inflammatory disease.

In relation to these Roman medicine-stamps, no discovery in Britain would be more curious than the detection of a seal used by STOLUS himself. And, though improbable, it is not impossible, that one may yet turn up, bearing both the name of this "celebrated British Oculist," and the name of his *Cinnabar* Collyrium.

MARKS AND WORDS ON THE FLAT SIDES OF THE SEALS; ORNAMENTS.

Occasionally on the flat and broad side of these seals, or in other words, on the sides not occupied with the inscriptions themselves, we find other letterings.

In one or two specimens these letterings have apparently consisted of marks, or conventional contractions, to enable the dispensing practitioner to use his seal more readily, and without reading the inscriptions themselves; as on the stamp found at Lyons, where two initial letters on the flat surface opposite each inscription denote the nature of that inscription, as AN, for *Anthemerum*, CH, for *Chelidonium*, and so on.

In one or two instances the name of the engraver appears on the side of the stamp. M. Sichel has described a stamp found at Paris with the letters RIPSIT GAI (*Scripsit Gaius*). In this, as in other specimens, these letters are not cut reversed like the letters of the

[1] His description is sufficiently concise:—"Cinnabari modice acrem facultatem obtinet. Habet vero et astrictionis quippiam." Keuhn's Galen, vol. xii., p. 221.

inscription, but straight, so as to read forwards, not backwards, thus proving that they were not intended to be used as a stamp.

In the Kenchester stone, see No. IX., there occurs on one side the word SENIOR, and on the other side the same word left unfinished, SENI. In this instance it is remarkable that the letters of the word are cut *retrograde*, as if with the view of being stamped, and by a rude and probably later hand than the legends on the sides. These letters occur on the stamp in which the practitioner is ARIOVISTUS. Was the term SENIOR intended to be added, when required, to any of the other impressions, in order to indicate a distinction between this ARIOVISTUS and some younger practitioner of the same name?

In addition to the inscriptions, occasionally we find engraved the figures of animals and plants, see Plate II., Nos. V. and IX., these being ornaments, appended apparently as the supposed figures of one or other of the sources, from which the principal ingredients or specific names of the collyria were derived. Rude figures of plants or trees seem the most common ornaments. On one side of the Bath stone are the figures of two birds, see Plate II. This occurs on the side bearing the name of the Collyrium *Crsyo-melinum*. But there is no apparent connection between the ornithological figures and the name or ingredients of the collyrium. M. Rever, in his "Appendix to the Antiquities of Lillebourne," gives the representation of a seal with the figure of a hippocampus upon it. Has this a relation to the collyrium *Thalasseros* inscribed on the seal? There is engraved on the same Lillebourne seal the outline of a double-handled vase, which Duchelais conceives may indicate the form of vessels in which the collyria were contained.

MINERALOGICAL NATURE OF THE STONE OF THE STAMPS.

The stone of which these medicine-stamps are made seems nearly the same, if not identically the same, in all the specimens that have hitherto been discovered and properly examined. It is of a greenish or greenish-yellow colour, easily cut by the burin, and yet so firm and compact, as to retain, with the requisite strength and sharpness, the impression of the letters. In appearance it is very analogous to the clay-slate schist used for forming whetstones, and found along with the primary and silurian slate, in different parts of Scotland, England, and the Continent. In describing its characters in some of the preceding notices, I have spoken of the stone as mineralogically a steatite, in consequence of the great mineralogist Haüy having pronounced some of the French specimens to belong to that family of minerals. If any speciality in the nature of the stone used could be pointed out, it might perhaps lead to some knowledge of the locality from which it was derived, and the probable place of manufacture of the stamps.

ARTISTIC AND LITERARY CHARACTER OF THE MEDICINE-STAMP INSCRIPTIONS.

As in most other ancient Roman inscriptions, the lettering upon the oculist-stamps would be considered at the present day very defective in various respects. In many of the seals there is a gross miscalculation in regard to the space or length which the whole legend would occupy, rendering it necessary (despite even the most awkward elisions and omissions) to give, in some instances, only a truncated part of the inscriptions; and in other instances, to gain suitable space for the words, some of the letters are engraved of a smaller size than the others, as in the two O's in the CROCODES of the Tranent stone, or the CHLORON of the Kenchester stamp. Often two or more letters are braced together in a monogrammic form, as in the *Lippitudo* of the Cirencester stamp. These defects, however, are defects not by any means peculiar to the medicine-stamp inscriptions, but they are to be found in most old Roman inscriptions, whether numismatic, votive, monumental, or of other kinds.

But the engravers seem to have been more deficient in their knowledge of the literature, than even in their knowledge of the mechanical part of their art. In many of the inscriptions there is no affectation of any attempt towards punctuation; and frequently there is no kind of separation even between the terminal and initial letters of the consecutive words. The whole words of the legend are thus often run, as it were, into one. The inscriptions abound, also, as we have seen, in errors of orthography. These errors exist, not only in the long professional terms which are sometimes used, but even in short words, such as the engraving of AT for AD. In all these respects, however, it is still, let me repeat, to be recollected, that the medicine-stamp inscriptions do not differ from the numerous other Roman inscriptions which time has spared for us.

The slightest study of these medicine-stamp inscriptions cannot, I think, but suggest one reflection. The inscriptions, with their numerous elisions and omissions, are sometimes read with difficulty by any scholars who endeavour to interpret them at the present day.[1] But the very fact of the common eye-medicines of the Roman colonial practitioners being inscribed with such inscriptions, tends to show us, that it was taken for granted, that the general body of the Roman colonists who did use these medicines in Britain, Gaul, and Germany, were capable of reading the inscriptions themselves, thus taking for granted, in matters of practice, an amount of educational knowledge among the Roman settlers and subjects in ancient times,

---

[1] One of the greatest Latin scholars and teachers of modern times endeavoured, some years ago, to decypher the inscriptions on the Tranent stone (see stamp, No. I.), but ultimately came to the conclusion, that they were merely strings of letters without any sense or meaning whatever.

for which historians have hitherto given them little credit. And yet some of the later Roman classics are not without significant allusions as to the extent to which education, even in some of its higher branches, was carried in Gaul and Britain during the times of their Roman colonisation. Suetonius tells us that the emperor Caligula instituted at Leyden, contests among competitors with regard to their proficiency in Greek and Roman literature.[1] Juvenal, dealing in figurative language, but in such figurative language as evidently had its foundation more or less in actual report and actual fact, speaks (Satire xv., v., iii.) of learned Gaul serving as a school of instruction to British lawyers; and even, he adds, Thule (Shetland?) talks of hiring a rhetorician—

> Gallia causidicos docuit facunda Brittanos:
> De conducendo loquitur jam Rhetore Thule.

Before passing from these engraved inscriptions themselves, let me make one more remark. The history of most important discoveries is marked by this fact, that these discoveries were often all but reached and accomplished long before they were actually and fully perfected. Here, with these oculist-stamps, we have the colonists of Rome printing in wax or other fictile matters, thirteen or fourteen centuries before the grand era of the discovery of type-printing. We cannot resist wondering now, that the whole art of printing should not have become divulged to them, seeing they did practise constantly, though partially, the art of printing with such stamps as we have described, and with their potter's stamps, signet-seals, &c. Some historians have stated that Guttenberg himself was led to the happy thought of printing with separate and moveable types, in consequence of looking at the inscriptions on the ring which he used to wear: —

> Annulus in digitis erat illi occasio prima
> Palladium ut coelo sollicitaret opus.[2]

Nothing is more likely than that the great thought of separate types and printing different words, sentences, pages, and books, by their aid, might have been suggested to Guttenberg's original and strong mind in the way here averred. But still there was a vast and mighty distinction between taking impressions from the fixed inscriptions of stamps and seals, whether cut in relief or in intaglio, and printing from types that were moveable, and capable of infinite re-

---

[1] The unsuccessful competitors were, according to Suetonius, sometimes severely and unhandsomely punished for those vanquished in a purely literary contest. For they were occasionally obliged, he informs us, not only to laud their conquerors, but to lick out their own unhappy compositions with their tongues, or be beat with a ferula, or ducked in the Rhine.—See *Suetonius's Life of Caligula*, chap. 20.

[2] See Wolf's Monumenta Typographica, p. 17.

arrangements and combinations. The magnitude of Guttenberg's discovery consisted in appreciating the great difference between these two methods of making permanent literary impressions, and in reducing his theory to actual practice. The power of taking or printing impressions from signets, stamps, &c., had been known long even before the time of the Romans, and had been turned to comparatively little account. The art had been used for thousands of years; but during that long period it had exercised little or no influence upon the progress of civilisation. The higher art of type printing has only, on the contrary, been known for four centuries; but, in the course of that comparatively brief period, it has already completely modified and changed both the intellectual and the general history of the human race.

"*Time conferreth a dignity upon the most trifling thing that resisteth his power.*" So, long ago remarked, one of the greatest of English physicians and English writers, the author of the "Religio Medici." The simple innate truth which the remark contains, is the main apology which I have to offer for protracting these Notices to such an unforeseen and unwarrantable length. In framing these Notices, the Tranent stone has served as a kind of historical fossil, around which, as a nucleus, I have attempted to reconstruct an imperfect amount of knowledge, relative to the state of one branch of medicine, as it was practised in Great Britain some fifteen or sixteen centuries ago. How very defective this attempt has been, none of my readers can be more sincerely aware of than I am. But still I do hope that some of them will agree with me in thinking, that it is not altogether uninteresting to recover, after a lapse of sixteen or seventeen hundred years, a glimpse (however slight that glimpse may be), of the state of medical practice in our own country in those distant times, when the Roman soldier and colonist ruled and dwelt among us, and when (as we know from their rediscovered altars), Apollo and Olympian Jove ("Jupiter Optimus Maximus") were worshipped on the banks of the Forth.

Part Second.

REVIEWS.

Medicines: their Uses and Mode of Administration. By J. Moore Neligan, M.D. Edin., &c. Third Edition. 8vo. Pp. 555. Dublin: 1851.

It is hardly necessary for us to express any general opinion as to the character of a work which has already received two commendatory notices in this Journal, and which possesses the unequivocal testimony in its favour of having reached a third edition. We shall, therefore, content ourselves by stating that in this third edition Dr Neligan continues to uphold the character of his book as a succinct work of reference useful both to the student and practitioner. The interests of the latter class of readers appear to have been more especially studied by the author in preparing this new edition, the increase in size of the volume being chiefly due to the introduction of more extended accounts of the therapeutical action of individual remedies.

In the less practical details, however, Dr Neligan has also brought his book up to the actual state of science, so that the pharmacien and student also will find that their interests have not been neglected. Of course, in this new edition, Dr Neligan has incorporated the changes rendered necessary by the appearance of a new edition of the Dublin Pharmacopœia, and most people on this side of the Irish Channel will, we think, be inclined to award praise to him for having done so, or rather, had he omitted it, would have thought him most unpatriotically forgetful of "justice to Ireland." It does not appear, however, that such ideas prevail in Dublin, in the very College of Physicians of which Dr Neligan is a licentiate. There, it would seem, it is held that the publishing a new edition of a book on Materia Medica, and bringing it up to the present state of the science in Ireland, is not only not commendable, but is illegal. We had heard some strange whisperings wafted across the Irish Channel, about injunctions and proceedings in the Rolls Court, but we had nearly forgot them till they were recalled to our recollection by the following editorial article in the "Pharmaceutical Journal," which is so curious an account of the Irish way of advancing a knowledge of the Materia Medica, that we are induced to copy it bodily:—

"THE COPYRIGHT OF A PHARMACOPŒIA.

"It is reported that 'King and Queen's College of Physicians of Ireland' has assigned its interest in the Dublin Pharmacopœia to Dr Apjohn, who has announced his intention of proceeding by injunction against all those who shall copy

the work. This threat is levelled against the authors of Dispensatories or other works on Materia Medica, containing the formulæ of the three colleges, placed in juxtaposition for the convenience of the medical practitioner, the dispenser, and the student. We have heard the names of three authors who are already singled out for attack. We understand that in the case of one of these gentlemen (Dr Neligan), Dr Apjohn served him with a notice a day or two before the appearance of the third edition of his '*Medicines, their Uses and Mode of Application*,' warning him not to commit a 'breach of the law,' in introducing the alterations or additions of the new Dublin Pharmacopœia.

"The question of copyright was formerly raised by the Royal College of Physicians of London, in reference to the London Pharmacopœia, the exclusive publication of which had been entrusted to Mr Richard Philips, in lieu of a direct pecuniary consideration for his services in the compilation of the work.

"Dr Collier, in the year 1836, resisted this exclusive right, and contended that the employment of a compiler or editor, not a physician, however qualified he might be as a chemist, was a violation of the spirit and letter of the charter of the College, and he publicly avowed his determination to defend himself against any injunction which might be brought against him. The College, however, did not deem it expedient to institute proceedings, and Mr Philips contented himself with the advantage which priority of publication and the official appointment had given him. We think he acted wisely in retaining the proceeds thus realised, which amounted to a considerable sum, instead of wasting in law that which he had earned by physic.

"From that time to the present, we have heard nothing of the copyright of the London Pharmacopœia. Several authors have re-published it with notes and commentaries, and it has been amalgamated in numerous works on Materia Medica and Pharmacy, with the Pharmacopœias of the other two colleges. The principle of free trade in Pharmacopœias has therefore been tacitly recognised by the London College of Physicians; and when we consider the class of works which would be affected by the restrictive policy—works of a comprehensive nature, and most important, if not necessary, to the student and the profession—it cannot be doubted that a return to monopoly would be a very unpopular measure; and that the Dublin College would not rise in the public estimation by making such an attempt.

"We also doubt the legality of the claim. An individual is protected against piracy by the law of copyright, and the justice of this provision is unquestionable. A society, or public body, issuing publications of a proprietary character, may also in fairness enjoy the same protection. But a Royal College of Physicians, publishing a Pharmacopœia, stands on different ground. The work is issued under the sanction of the Queen in Council for the benefit of the public. The compilation and publication of such a work is one of the duties comprised in the conditions expressed or understood, on which the Charter is held and the Royal sanction enjoyed; and we conceive that from the day that a Pharmacopœia sees the light, it is public property. Such we presume to have been the opinion of the eminent counsel, before whom, it is reported, Dr Apjohn recently laid a case; for the opinion received was so unfavourable that no further proceedings have been taken by Dr Apjohn."

Not being learned in the law, we do not presume to offer any opinion as to the legality or illegality of Dr Neligan publishing a correct, instead of an incorrect, edition of his book. We should think this question more fitted for a court of common sense than a court of common law. But looking at it professionally, we must say that we have rarely heard of a more extraordinary proceeding on the part of a scientific man or a scientific body. It just amounts to this, that the Dublin Pharmacopœia is to be held to be, by authorship and copyright, Dr Apjohn's, differing only from any

other individual author's book by its bearing the name of the Dublin College of Physicians on its title-page, and that, henceforth, every one else is to be prohibited, not merely from printing or publishing it, which we presume nobody has the least desire to do but from quoting it in the only way in which a pharmacopœia can be quoted. The effect would have been, had the Irish lawyers not been wiser than the Irish doctor, to have put a stop not only to Dr Neligan, but Dr Pereira, Dr Royle, Dr Christison, or any other author of a work on Materia Medica or a Dispensatory, publishing or re-editing their books, unless they would consent to quote as the present authorised work of pharmacy for Ireland, the defunct Irish Pharmacopœia of 1826.

But, *revenons à nos moutons;* we were reviewing Dr Neligan's book, and not the Dublin Pharmacopœia. It would lead too much into details were we to attempt any criticism on the individual subjects treated of by Dr Neligan. We select, however, two short extracts, each of them referring to an important agent.

The following is a succinct and practically good resumé of the points to be attended to in inducing anæsthesia by chloroform :—

"It is most effectually and safely administered in the manner first proposed by Professor Simpson, namely, by pouring the chloroform into the hollow of a handkerchief folded in the form of an inverted cone; at first f℥j. only should be used, and if the desired effect be not produced in about two minutes, the same quantity should be renewed. Various forms of *inhalers* have been proposed for the administration of the vapour of chloroform, but I must confess that I prefer the simple handkerchief. The chief points to be attended to are:—1st, That the patient should be lying on his back with the head slightly raised; 2nd, That he should be permitted at first to breathe atmospheric air mixed with the chloroform, which is effected by not bringing the handkerchief too close to the mouth and nose at once; 3d, That the vapour should be altogether withdrawn as soon as insensibility is produced, which is usually evidenced by the occurrence of slight stertorous breathing, for the condition can be kept up for any length of time that may be requisite, by the occasional re-application of fresh chloroform in the handkerchief; 4th, The patient's stomach should be empty when the inhalation is commenced, as otherwise vomiting is apt to be produced; and 5th, should fainting or other evidence of sinking occur, the best restorative is atmospheric air, ammonia also may be applied to the nostrils, the cold douche on the head used, or even artificial respiration may be had recourse to, *but no stimulants should be given by the mouth.* There is but one other remark which I have to make for the benefit of those inexperienced in the use of chloroform, namely, that during the process of inhalation, just before insensibility is produced, there is usually a struggle on the part of the patient; *this must be resisted, and the charged handkerchief kept just at that time closely applied to the mouth and nostrils.*"

We do not think that enough of prominence is given to the use of chloroform internally. It is spoken of, but rather coldly. Our own experience leads us to regard it as an anti-spasmodic and calmative of great value, but it is hardly worth while to give it in less quantity than 25 or 30 minims, which is about the maximum dose admitted by Dr Neligan. We have, in cases of asthma, given 50 drops, nearly equivalent to as many minims, with or without

morphia, and it has now superseded sulphuric ether completely in our favour. For a patient in the asthmatic paroxysm, chloroform has this advantage, that it is much more easily swallowed, and is not so liable to produce that intense choking, almost verging on suffocation, which we have seen to occur in an asthmatic patient when swallowing a draught containing ether. Dr Neligan says that on account of its volatility it should always be prescribed in draughts. We apprehend that the practice is right, but the reason wrong. It is rather on account of its great specific gravity, by reason of which if a mixture containing it is not accurately mixed by shaking, and the dose instantly poured out and swallowed, the most of the chloroform will be left at the bottom of the bottle. Mixture by means of mucilage, mentioned by Dr Neligan, is the best form, and we prefer ordering the chloroform separate, having it dropped into the requisite dose of mucilage, or whatever else is to be given with it, stirred with a spoon, and swallowed incontinently.

Under the preparations of aconite, we have the following remarks on the tincture now for the first time admitted into a pharmacopœia:—

"*Tinctura radicis Aconiti*, D. (Take of aconite root, dried, and cut small, ten ounces; rectified spirit, one pint: macerate for fourteen days, strain, express, and filter). Dose, min. viij. three times a day, increased by one minim daily until its effects are produced. A stronger tincture, and that which has been in general use until the publication of the last edition of the Dublin Pharmacopœia, is prepared by the following process of Dr Fleming:—(Take of the tubers of Aconitum napellus, carefully dried and finely powdered, ℥xvj. (Troy); rectified spirit, f℥xvj.; macerate for four days, then pack into a percolator; add rectified spirit until f℥xxiv. of tincture are obtained). This tincture is beautifully transparent, of the colour of sherry wine, and has a slightly bitter taste. Dose, min. v. three times a day. The strength of this preparation compared with that of the Dublin College is about 3 to 2. Either of them should be used with caution; the strong tincture is the best form for external use, when aconitina cannot be procured, and this is an important advantage which Dr Fleming's tincture possesses, namely, that it answers for both purposes, while that of the Dublin Pharmacopœia is too weak for external application, and there can be no more danger in giving internally a small dose of a strong tincture than a larger dose of a weak one; his tincture is also of a more uniform strength, inasmuch as the quantity of the root employed being in excess, the spirit is always fully saturated."

We are not inclined to assent to these remarks. Even if Fleming's tincture be extremely valuable as a local application, and we must say that as such it has disappointed us, and admitting that the Dublin tincture is rather too weak to act powerfully when applied to the skin, we doubt if these considerations on the one hand be not more than counterbalanced on the other by the potency of a tincture, the dose of which, internally, may vary from safe to positively dangerous by the difference of giving one or two minims more or less, and we must caution Dr Neligan's readers somewhat more decidedly than he does as to the dose. Five minims three times a day is assuredly unsafe. Some patients will undoubtedly bear this, or even larger doses, but many would be danger-

ously affected by such a quantity. We once produced very alarming symptoms, in fact we nearly poisoned a patient, by a single dose of 2½ minims. Smaller doses, one or two minims frequently repeated, for its sedative action is not very permanent, is a safer, and we submit equally effectual, mode of using it. There is one disadvantage of this strong tincture, which, though not a very important one, may be mentioned. Druggists complain that from the large proportion of vegetable matter to the spirit, there is much loss and great difficulty in procuring a uniform amount of product.

Pharmacopœia Nosocomii in Curam Morborum Cutaneorum Fundati A.D. MDCCCXLI. London 1850.

THIS is a pharmaceutical curiosity. It measures four inches in length, two and a half in breadth, three-tenths of an inch in thickness, weighs 427 grains, and as it has afforded us some little amusement, we think it entitled, both physically and metaphorically, to be called light reading. Its pervading characteristic is an amount of ingenuity in devising complex formulæ which set at open defiance all calculation as to their probable therapeutical effects or *modus operandi*. It would seem as if, for the cure of skin diseases in London, anything short of a compound of three ingredients were of little use, but that, provided there only be enough of ingredients, it matters little whether they have any legitimate business there or not. We take the first page that comes to us *ad aperturam*, and it being No. 15, we there find *Lotio opii cum alumine*, which consists of an ounce of laudanum, two pounds of alum, in a gallon of water, and as if this were not astringent enough, three ounces of tincture of galls are added, the effect of which, of course, must be to precipitate the alkaloids of the opium and render them comparatively inert. In this same page we find *Lotio Nigra*, which is made on the ordinary plan of decomposing calomel by an alkali, liquor potassæ being substituted for the lime-water more commonly in use; we turn over the page, and have, by way of contrast, *Lotio Rubra*, which contains bichloride of mercury, bisulphuret of mercury, and creasote. This last, we should think, was complex enough. But no; the next page reveals to us that there must be some cases for which even this is too simple; and we have, therefore, *Lotia Rubra Composita*, which consists of equal parts of the red wash, the black wash, and water mixed. We were struck, on looking at this last formula, with a hazy recollection of having somewhere seen this formula before, and after an effort of memory, we found it out. It was not in a book on skin diseases, nor in a pharmacopœia, nor, indeed, in a medical work at all, but in England's own immortal bard. Here is the original formula:—

"Black spirits and white,
Red spirits and gray,
Mingle, mingle, mingle,
You that mingle may."

Our readers will see from the above formula,—we mean the Pharmacopœia's, not the Weird Sisters',—that mercury plays no small part in the cure of skin diseases in London, for here are no less than three compounds of it in one formula, the so-called black oxide, the bichloride, and the red sulphuret. But they will be vastly mistaken who think that this is the only mercurial preparation. In the Hospital Pharmacopœia, there are 105 formulæ, and of these 40 contain more or less of mercury in one form or other.

In point of pharmaceutical accuracy and distinctness, this Pharmacopœia is so deficient that it can be of little use to any but those who framed it, and who know what its unexplained peculiarities indicate. The skin pharmacopœia professes at the outset to follow the London Pharmacopœia in whatever relates to weights, measures, and materia medica; but as it takes in some articles not recognised by the metropolitan college, it furnishes at the end a list of fifteen substances, which, it is stated, are not in the London Pharmacopœia, but of which not less than three actually are there, viz., *Barii Chloridum*, *Hydrargyri Ammonio-Chloridum*, both under these very names, and *Sal Marinus*, which surely might have been suspected to be identical with the London Pharmacopœia's *Sodii Chloridum*. But to keep the balance true, they omit to mention in their list at least three substances which are not in the London Pharmacopœia, viz., *Acidum Hydriodicum*, *Carminium*, and *Hydrargyri Sulphas*. In one formula we have two ounces of hydriodic acid employed. Now as no directions are given as to what strength of a solution of hydriodic acid is meant by this, we are left to suppose that it means either two ounces of hydriodic acid gas, or two ounces of a saturated solution of it. In either case, we wish those who have the preparing of it joy of their process. As the sole use of this hydriodic acid is to make a solution of biniodide of mercury, we would suggest, as a preferable plan, to add to a solution of corrosive sublimate iodide of potassium in excess; this would at one process make the biniodide of mercury and dissolve it, and the presence of a little chloride of potassium would not make the preparation either less useful or more useless than it is at present.

We should not care much for the pharmaceutical and chemical errors of this Pharmacopœia, though they be little to the credit of this department of British science; but in a therapeutical point of view, we must regret the appearance of such a work. Their absurd complexity renders it impossible for any rationale to be devised for the action of the majority of these formulæ; and whether cures occur under their use or not, the therapeutics of skin diseases will not be advanced by them one step, but will remain, so far as this hospital is concerned, hopelessly obscured by a dark empiricism.

The Anatomist's Vade-Mecum; A System of Human Anatomy. By ERASMUS WILSON, F. R. S. Fifth Edition. London. 8vo. Pp. 656. 1851.

The Surgeon's Vade-Mecum. By ROBERT DRUITT, F.R.C.S. Fifth Edition. London. 8vo. Pp. 660. 1851.

Dr Hooper's Physician's Vade-Mecum. By WILLIAM AUGUSTUS GUY, M.B., Cantab. New Edition. London. 8vo. Pp. 576. 1851.

WE group these three works together, as all belonging, in outward appearance at least, to the now very numerous class of "Manuals." In former reviews, we have expressed our dislike to these epitomes of medical science. We dislike them, because, being compilations from many quarters, they are of course destitute of the unity and vigour characteristic of original works; and because they are calculated to divert the attention of the student from the best sources of professional information. We object to them, moreover, because they prey and fatten upon more important and original publications, and thus tend directly to discourage literary labour. When a medical author, now-a-days, assumes the pen, to communicate to the profession the results of his experience and observation, well wots he what is in store for him. Literary medical tailors, dealers in second-hand clothing, are ready to pounce upon him, and adapt his work to their own purposes—to cabbage from it—to clip it with shears—to stitch the shreds to patches of other materials, and to hide the clumsy seams with the treacherous polish of "the goose." Such are the manipulations of the manual-makers.

We may add, in passing, that there are medical mummy-makers too—the Retrospects—Ranking to disembowel, and Braithwaite to prepare the skeleton; but, unlike their prototypes of Egypt, they obtrude their services and commence their office before the breath has left the body of their subjects.

To proceed to the works now before us. Though manuals, these are not all equally objectionable. Of the utility of such a compendium of anatomy as that of Mr Erasmus Wilson, there can, we apprehend, be no question. It can never supersede, while it may faithfully direct, that practice in the dissection of the human body upon which all useful anatomical knowledge must be based. Its explanations of many points in descriptive anatomy, perplexing to the student,—true *pontes asinorum*,—are generally distinct and clear, while the necessity for dry details is often obviated by the introduction of well-executed diagrams and wood-engravings. Mr Wilson is no plagiarist; he is well known to be practically conversant with the subject on which he writes; and the favour with which

four previous editions of his work have been received, is no mean testimony to its merits.

Mr Druitt's Vade-Mecum has also reached a fifth edition, but is precisely one of that class of books to which our strictures upon Manuals in general may be held to apply. It is due to the author to state, that the sources from which he has borrowed are duly acknowledged in almost every page, and that his selections—usually judicious—are often condensed into small bulk without sacrifice of clearness. We must, however, add, that the principles and practice of surgery are ill-used when gathered from all quarters, and compressed into such compass,—that the work wants originality,—and that its success is, in our opinion, more due to its attractive woodcuts and neat form of publication, than to any intrinsic excellence.

In Hooper's Vade-Mecum, we have an attempt to cram into the space of 660 small pages, all the most useful facts and opinions in the practice of medicine, together with physiology, pathology, general therapeutics, and a formulary of formidable dimensions. We need hardly say, that the attempt has proved but partially successful, despite the zeal and talents of the editor, Dr Guy, who is obviously capable of better things, and whose original contributions to the volume are in our eyes by far the most valuable portions of its contents. The student who, in prospect either of his examinations, or of the exigencies of practice, resorts *only* to this Manual, will find to his cost that a competent knowledge of the practice of physic cannot be acquired in two or three evenings' reading.

Part Third.

CLINICAL REPORTS, LECTURES, ETC.

CLINICAL MEDICINE.—PROFESSOR BENNETT.

REPORT OF CASES OF NERVOUS DISEASE IN THE CLINICAL WARDS OF THE ROYAL INFIRMARY, DURING THE WINTER SESSION 1850-51.—(*Concluded from p.* 287.)

Condensed from Lectures.

CASE VI.—*Paraplegia—Tubercular Caries of Vertebral Column—Phthisis.*

William Walker, æt. 42, mason, admitted October 17, 1850. States, that for upwards of twelve years he has suffered from occasional cough, usually dry, accompanied in lower and middle part of chest with pain, which extended back to the dorsal vertebræ. The pain and cough have, within the last three months, become more constant, and accompanied by nightly perspirations. About fourteen days before admission, he felt a prickling and coldness in the feet, and an unsteadiness in walking, especially in the left foot. Two days before admission, after a long walk, these symptoms were increased. Since then they have

gradually increased, so that now he has no power over his legs beyond bending the knee very slightly. Sensibility of the integuments not impaired.

On admission, looks emaciated, and anxious. Complains of no headach, or pain in spine, save between the shoulders. He cannot stand without support, and when he tries to walk he staggers, and would fall if not supported. He can move his legs in bed with tolerable freedom; they often feel cold. Superior extremities unaffected. There is occasional cough, with scanty expectoration. On percussion no dulness is perceptible, on either side, anteriorly. On auscultation, the respiratory murmurs are harsher and louder than natural at apex of both lungs. The *urine* not albuminous, but loaded with lithates, and sometimes voided with difficulty. Other functions healthy.

The paralysis in this man gradually increased. He could not stand, although, when in bed, he could slightly bend the knees and toes. Latterly, a swelling formed over the lower dorsal and upper lumbar vertebræ, five inches long and three broad. The urine became loaded with phosphates, and, with the fæces, was passed involuntarily. His general health also greatly diminished, painful twitchings occurred in the paralysed limbs, emaciation became extreme, the cough violent, the expectoration copious. Several sloughs formed over the left and right hips, notwithstanding he lay on a water bed, and every care was taken to prevent them. During the last week in February, and two first weeks in March, an abscess formed above the right clavicle, which opened spontaneously on the 15th of the last-mentioned month, and discharged about 8 oz. of matter. As the respiratory murmur was still heard at the apex of right lung, it was concluded that the abscess originated in the vertebral column. A considerable quantity of pus was subsequently discharged daily, being forced out at each inspiration. He is now (March 24th) greatly exhausted, pulse weak and irregular; low muttering delirium at night, with scarcely strength to expectorate.

Case VII.—*Paraplegia—Myelitis.*

Maximilian Saulsen, æt 35, perfumer, native of Warsaw, admitted January 9th. States that 2½ years ago, he first felt a pricking followed by numbness in the toes of his right foot, which gradually extended, ending in diminution of voluntary power over the parts. His left leg then became similarly affected. His general health was good. In 1849 he went to Germany, where he made use of the baths of Wiesbaden. Returning to England, he applied at one of the London dispensaries, and here he was cupped and galvanised without benefit. During last summer he went again to Germany. He says he could walk on board at this time; but when he left for England in October last, he was obliged to be lifted on board, his legs being useless, while his arms were unaffected. During the passage back from Hamburg, two months ago, his left hand felt benumbed, and he could with difficulty use the fingers, except the little finger, which he says was unaffected. The right hand remained natural, with the exception of the little finger which felt numb. Since then, the numbness in the feet and inability to move have increased very much.

On admission, his general appearance is healthy. He is unable to walk to any distance without the assistance of a stick. He is unable to direct the motions of his right leg without watching it. When he stands without support, great unsteadiness is observed; and when directed to close his eyes, he loses all control over his movements, and would fall to the ground if not prevented. He is unable to use the fingers of the left hand with any precision. Sensibility of the skin unimpaired. No headach; no tinnitus aurium; a little dizziness occasionally. Urine passed without difficulty; sometimes involuntarily during the night—1023 sp. gr. Complains of difficulty of defecation. He was ordered to be cupped and blistered. These remedies, in conjunction with repose in the infirmary, produced considerable amendment, and he was enabled to take long walks with the aid of a stick. On Friday the 10th, he passed a large lumbricus; and 40 grains of Pulv. Rad. Filicis Maris were er-

cuated. On February 23d, he was ordered one-twelfth of a grain of strychnine twice a-day, which dose was increased to one-sixth on the 28th. On the 1st and 2d of March he was awoke several times during the night by startings of the limbs; and he stated that their general strength was diminished. He evidently staggered more in walking. Galvanic currents were then ordered to be passed from the spine down both limbs, under which treatment he remains at present (March 24th.)

Commentary.—The two preceding cases of paraplegia are dependent on different pathological causes, and present consequently marked variations in the symptoms. In case VI., pressure was gradually made on the spinal cord from scrofulous caries, and deformity of the vertebral column. The anterior columns of the cord, under such circumstances, are those which are most injured, and, consequently, loss of motion is the leading symptom. In case VII., the first symptoms were pricking and numbness of the toes, followed by perversion or irregularity, rather than loss of motion, unattended with spasms. This condition is indicative of chronic inflammation of the cord or myelitis. Professor Romberg has pointed out a symptom in such persons, which he considers diagnostic of softening of the gray matter in the centre of the cord, as distinguished from lesion of the white matter. This consists of ascertaining that a man cannot stand steadily with his eyes shut. This symptom was well marked in Case VII., while everything indicated that it was not so much the conducting, as the tonic power, which was wanting. These considerations induced me to try the effects of strychnine, which, however, increased the irregular movements when walking, and caused loss rather than increase of power. In Case VI., the constitutional disease, the continued increase of spinal deformity, but more especially the pulmonary lesions, are rapidly hurrying forward a fatal termination.

CASE VIII.—*Paralysis of the abducens oculi and auditory nerves—Exophthalmia—Tumour at the base of the Cranium.*

John Wright, æt. 30, typefounder,—admitted, November 26th, 1850. States that four years ago he had a severe attack of rheumatism, soon after which he experienced considerable pain in the right side of the head. His right eyeball then became painful and began to protrude. Hearing also on the same side, was at first dull and then abolished. Ten months after the commencement of the headach, it abated on the right side, but became violent on the left, where it has continued ever since. He was treated with mercury and iodide of potassium. Two years since, he was attacked with spasms and grinding of the jaws, and on two occasions, the convulsions were pretty general and attended with loss of consciousness. His vision has been quite perfect, till about a fortnight ago, when he began to see double. He has continued to work until the 23d inst., when, owing to the imperfection of his sight, he was obliged to desist. On admission, complains of cephalalgia, most severe on the left side. There is complete deafness on the right side. The right eyeball is very prominent; can be turned inwards but not outwards. Vision is perfect in both eyes, but, from the axis of both not being alike, is double. He cannot lay hold of an object at once, and in attempting to grasp it, his hand is at first directed to one side. No other form of paralysis, and his other functions are healthy.

Since admission, this man has presented considerable alternations in his symptoms, the headach being sometimes more severe than at others; and on such occasions, there was considerable stupor, loss of memory, and confusion of ideas. His treatment consisted of the internal use of the iodide of potassium and purgatives, with counter-irritants externally. On the 9th of February, it was noticed that the right eyeball was less prominent. On the 25th it was ascertained, on careful examination, that he was not perfectly deaf on the right side, and that the right eyeball could be everted more than formerly. On the 1st of March, the prominence of the right eyeball was comparatively slight.

He can abduct it fully, and vision is now single. The pain in the head is unabated, but more erratic. By the 15th of March, the cephalalgia had greatly abated. There was a marked improvement in his general health. Movements of the right eyeball normal—deafness on the right side considerable. Blisters to the temples and neck, and a variety of remedies have been tried, to cause sleep, and diminish the pain; of which Cannabis Indica, M. xij. in the form of a draught, appeared to be the most beneficial.

Commentary.—In this case, the deep-rooted cephalalgia, the exophthalmia, the paralysis of the sixth and auditory nerves on the right side, clearly indicated the existence of a solid body, pushing out the eye, and pressing on the affected nerves. At one period, also, the motor branch of the fifth pair must have been irritated, as exhibited by spasms of the jaws, while other cerebral derangements occurred. It would seem, however, that the tumour must lately have diminished in size, for there has been gradual disappearance of the symptoms,—first, return of the eyeball within the orbit,—secondly, recovery of the functions of the right abducens oculi,—and lastly, improvement of hearing, with diminution of cephalalgia. The careful examination of this case, as it progresses, will be found highly instructive.

On the Pathology of Cerebral and Spinal Softenings, and on the necessity of employing the Microscope to ascertain their Nature.

The nature of cerebral and spinal softenings has been much disputed. Some attribute them entirely to chronic or acute inflammation; others, while they acknowledge that some softenings are undoubtedly thus produced, are also of opinion, that they may occasionally depend upon other causes. Thus softening has been considered a lesion *sui generis*, similar to what occurs in ataxic fever (Recamier), to *gangrena Senilis* (Rostan, Abercromby), to obliteration of the arteries (Bright, Carswell), or to a diminution of nutrition (Delaberge, Monneret). It has also been referred to post-mortem maceration (Carswell, Paterson of Leith), and is undoubtedly often produced by mechanical violence after death. The difficulty hitherto has been how to distinguish with precision one kind of softening from another.

From a careful analysis of 32 cases of softening of the nervous centres, which I published in 1842-3,[1] I think it may be concluded that two varieties of softening undoubtedly occur. In the one, the products of inflammatory exudation may be detected; in the other, these are not present. It is necessary to determine, with exactitude, the distinguishing characters of these two kinds of softening, which may be denominated inflammatory and non-inflammatory softenings.

Inflammatory softening always contains numerous granules and granular corpuscles, which are more numerous according to the degree of softening. The nervous tubes and normal structures are always, when the softening is great, more or less broken up. The nature of inflammatory softening of the brain, like all such lesions, depends upon the exudation of blood plasma, the development of granular corpuscles, and the subsequent breaking down of the latter.

If chronic, it may be considered a fatty degeneration of the brain. When recent the serum poured out also assists in producing the softening.

In non-inflammatory softening, on the other hand, we find the cylindrical and varicose tubes of the part are rendered more soft and easily separable from each other. They have more or less lost their natural firmness and consistence, are readily torn across, the varicosities are easily enlarged by pressure, and, when separated or broken off, assume a globular form. The tubes also are more or less broken into fragments, and no exudation granules, masses, or granular corpuscles are to be detected.

---

[1] Edinburgh Medical and Surgical Journal, Nos. 153, 155, and 157.

The nature of non-inflammatory softening varies according to circumstances. It would seem to arise from four causes:—1st. From mechanical violence in exposing the nervous centres. 2d. From a mechanical breaking up of the nervous tissue, by hemorrhagic extravasations, whether in mass or when infiltrated in small isolated points, constituting capillary apoplexy. 3d. From the mere imbibition of effused serum, which loosens the connection between the nervous tubes, and diminishes the consistence of the nervous tissue. 4th. From the process of putrefaction.

As regards the difference supposed by some to exist between the softening in adults and that in more aged persons, I fully agree with M. Durand-Fardel, when he says, "Cerebral ramollissement is the same malady in old persons as in other adults; that it presents no other differences than the modifications which a variation in age always produces, connected only with the form, and not with the nature, of the lesion."

As for the opinion, that softening of the brain is a lesion *sui generis*, due to diminution of nutrition, to gangrene, or obstruction of arteries, my observations have convinced me, that all such explanations are hypothetical in the highest degree, and have no real existence.

The cases which I have recorded also indicate that different symptoms were present in such cases as were inflammatory, and in such as were non-inflammatory. Thus, in 24 cases carefully examined and analysed, in which cerebral softening was observed, granular corpuscles were present in 18, whilst in 6 no traces of these bodies could be found. On analysing the symptoms of these cases, we shall find a marked difference between those accompanying the one lesion or the other. Thus in the cases where *only* inflammatory softening was present, well-marked symptoms invariably existed, such as loss of consciousness, preceded or followed by dulness of intellect, contraction and rigidity of the extremities, or paralysis. On the other hand, in the 6 cases of non-inflammatory softening, there was no paralysis or contraction, and no dulness or disturbance of the intellect. Again, in the 4 cases where both lesions were present, symptoms could always be observed in the sides opposite the inflammatory softening, while none existed opposite the non-inflammatory. An analysis of these 24 cases, therefore, leads to the conclusion, that the two kinds of softening we have endeavoured to establish are alike distinguishable, both by their intimate structure, and by the symptoms accompanying them during life.

Now all practical men agree in considering it a matter of extreme difficulty to reconcile, with any certainty, the morbid appearances found in the brain, with the symptoms previously observed. The future microscopic examination of the softening may serve to prevent much of the error that has hitherto been committed. For instance, softening of the fornix, septum lucidum, and central parts of the brain, may exist in two cases. To the naked eye they may be in every respect identical, and yet the microscope enables us to determine that the one contains granular corpuscles, whilst, in the other, not one of these bodies is to be found. It becomes evident, then, that previous to this distinction having been made, two distinct lesions were confounded together; and that a different train of symptoms should, under such circumstances, be occasioned, is only to be expected. Again it has frequently excited surprise, that, notwithstanding the existence of well-marked symptoms of softening, nothing was to be discovered after death. Now I have demonstrated in several instances that, although to the naked sight no morbid lesion was apparent, still portions of brain might contain the same granular corpuscles as are to be seen in more apparent lesions; and that by considering such parts diseased, all the symptoms might be explained according to the pathological laws I have previously explained. By excluding these sources of error, therefore, and by being enabled at once to distinguish the lesion dependent on inflammation from others which simulate it, we shall be enabled to obtain more exact data for future investigations. From the observations recorded, however, the two following propositions may, I think, be established.

1ST. THAT PATHOLOGISTS HAVE HITHERTO CONFOUNDED SOFTENING DEPENDENT ON INFLAMMATION, WITH SOFTENING OCCASIONED BY POST-MORTEM CHANGES OR MECHANICAL VIOLENCE. 2D. THAT NOTWITHSTANDING THE MOST ANXIOUS SEARCH, AND THE EXISTENCE DURING LIFE OF THE MOST DECIDED SYMPTOMS OF SOFTENING, INFLAMMATION, THOUGH REALLY PRESENT, HAS ESCAPED OBSERVATION.

With respect to the first proposition it may be observed, that in many cases where no symptoms were present during life, extensive softening of the brain has been found after death. This is a well known fact, and is one which tends in no small degree to throw confusion on the pathology of nervous diseases. Thus—In one case there was extensive softening of the central portion of the brain, corpora striata, and optic thalami, which, however, contained no granular corpuscles. The symptoms were sudden insensibility and convulsions, which evidently depended on a capillary apoplexy that was also present. No paralysis or contraction existed. Four other cases were recorded, with more or less softening of the brain, without head symptoms, and without granular corpuscles in the softened portions.

Now in all these five cases there was an extensive softening, which it was impossible for any one to distinguish positively, by unaided sight, from inflammatory softening. In neither did granular corpuscles exist, and in neither did those symptoms occur which are peculiar to similar softenings produced by inflammation.

In addition to these five cases there were four others, where, conjoined with an inflammatory softening producing particular symptoms, there was also non-inflammatory softening, occasioning no symptoms whatever. The circumstances attendant on these nine cases, therefore, must convince us that softenings produced mechanically, or by post-mortem changes, have frequently been mistaken for those dependent on inflammation, and *must necessarily be so, so long as unaided sight is made the sole means of forming a judgment with respect to their nature.*

A perusal of these cases must satisfy any one that pathologists have hitherto been confounding two distinct lesions, viz., a softening dependent on inflammation, and a softening dependent on mechanical or other causes.

The second proposition, that softening or inflammation, though present, has frequently escaped observation, is clearly established by the following facts:—

A man had paralysis, with complete resolution of the limbs on the right side, and intense rigidity of them on the left. Death occurred in six hours. On dissection, a large coagulum of blood was found effused into the left hemisphere, thus explaining the paralysis on the right side. In the right hemisphere an old apoplectic cyst was found, and a number of small cavities, described by Dr Sims as chronic softening undergoing a cure. Here, then, there was nothing acute—nothing to explain the intense rigidity. A microscopic demonstration demonstrated that their cavities contained numerous granular corpuscles and granules, thus proving the existence of inflammation on the right side of the brain, and explaining the rigidity on the left side of the body.

Another well-remarked case was that of a man who entered the Infirmary, under Dr Paterson, in 1842. All the symptoms of acute softening were present; paralysis of the left side, including rigidity and contraction of the left arm, dullness of intellect, and tonic spasms of the muscles of the mouth and neck. The right side was also affected in a slighter degree. As the case excited considerable interest, great care was taken in examining the brain. When the lateral ventricles were opened, it became a question whether the right corpus striatum was softened. Several persons applied their fingers, and endeavoured to ascertain the point. As the manual examination proceeded, the normal consistence of the part diminished, until at length it presented all the appearance of pultaceous softening. In this state it was shown to Dr Paterson, who naturally enough considered it to be the result of disease. I differed from him in opinion, first, because I had carefully observed the gradual increase of the softening in the manner alluded to; and secondly, because disease of the cor-

pus striatum, in one side of the brain, could not have explained the well-marked symptoms which existed on both sides of the body. When the pons varolii was bisected, Dr Peacock, who conducted the examination, conceived it to be softened; others who examined it could perceive no difference in the texture; its colour and consistence were unchanged. Reasoning from the symptoms, the lesion was very likely to exist. But how, it was argued, could a judgment be formed; we ought to reason from facts, not theories? Here, then, was an evident lesion of the corpus striatum, which explained nothing, and a problematical lesion of the pons varolii, which, however, did it exist, would satisfactorily account for the symptoms. In this state of uncertainty the microscope was sent for, and I demonstrated, and made evident to Drs Paterson, Peacock, and all the students present, that the corpus striatum contained no granular corpuscles, whilst in the pons varolii they were very abundant. I have endeavoured to describe what took place on this occasion, from which it must be evident that had not the microscope been appealed to, the right corpus striatum would have been pronounced softened, whilst the real lesion in the pons varolii might have escaped observation. Under such circumstances this case would have added another to the inexplicable observations with which the records of nervous diseases abound.

What renders these cases, and several others I could relate, so remarkable and satisfactory is, that they are not instances where the dissection was performed in a hurried manner, and by incompetent persons. On the contrary, from the particular symptoms connected with them during life, the post-mortem examination was in all conducted with extreme care. The physician who had charge of the case was present. The examinations were witnessed or conducted by myself, in the presence of clerks and numerous students, and I may say that we were all in doubt until the microscope cleared up the difficulty. *These cases, therefore, sufficiently demonstrate that the naked sight is positively unable to detect lesions, even although they are directly indicated by the symptoms, and carefully looked for by experienced morbid anatomists.*

If, then, the two propositions formerly stated have been satisfactorily proved, and it is agreed that pathologists have been confounding inflammatory with non-inflammatory softening, and overlooking the former, although undoubtedly present, it must be evident that many of the contradictions which have existed in connection with the pathology of nervous diseases, may be accounted for. It must also be clear that no confidence can be placed in the analysis of cases, however numerous, when the sources of error now indicated have not been carefully excluded.

CLINICAL SURGERY.—PROFESSOR SYME.

17th March 1851.

HARE-LIP.

THE subject of this case is a young female, thirteen years of age, from Berwick-on-Tweed. She presented an example of the imperfection in its most complicated form,—there being a double fissure, with an intermediate portion of lip shorter than the natural extent, and a corresponding irregularity in the osseous parts. In such cases, the part of the upper jaw which corresponds to the intermaxillary or incisive bones of the lower animals is separated by a fissure from the lateral portions, and usually produced into a projecting knob, which sometimes extends almost to the tip of the nose, and always has attached to its surface the piece of lip that ought to have entered into the formation of the mouth. When the osseous projection exceeds a certain degree, it must be removed, in order to let the edges of the lip meet together. But this should be done some time before the operation is completed, in order to avoid annoyance from the bleeding, which is apt to issue profusely from the vessels of the bone; and when this pre-

liminary procedure is undertaken, the corresponding portion of lip should always be left,—so that, if circumstances afterwards permit, it may be employed to lessen the gap between the edges of the fissure. This principle of practice has been lately claimed by the London medical journals for the Professor of Surgery in King's College, who ought to know that it has been long established and acted upon in Edinburgh. I have at present in Minto House a patient, who required the bone to be removed; but in this young woman the inequality of osseous surface was not so great as to render such a measure necessary, and I therefore proceeded at once to make the respective edges suitable for union.

In doing this, I availed myself of a little manipulation, which though not, so far as I know, previously described, has been found eminently useful during several years past. This is seizing the convex edge that requires removal with catch-forceps, so as to stretch the lip, and thus not only show clearly the proper extent for removal, but steady it under the knife. You saw how easily the operation was accomplished, the patient being completely under the influence of chloroform. Three needles were introduced at the points A B and C, and union has been accomplished without a single drop of matter, so as to form a lip in every respect perfect. This being the fourth day after the operation, the needles are now removed.

EXCISION OF THE ELBOW-JOINT.

I now place before you the portions of bone removed from the elbow of E—— C——, aged thirty-two, whose case was lately explained. They consist, you will remark, of the olecranon, the articulating surface of the humerus, the head of the radius, and the remaining part of the sigmoid cavity of the ulna. The first step of the operation you saw was to make a transverse incision fairly into the joint from the ulnar nerve close to the olecranon, right across to the articulation of the radius. A longitudinal incision at each extremity of this one, presented the form of a letter H, and the two square flaps which resulted from it being reflected in opposite directions, ample space was afforded for the subsequent steps, which consisted in removing the olecranon by cutting pliers, dividing the lateral ligaments of the joint, protruding and sawing off the extremity of the humerus on a line with its tubercles, decapitating the radius, and removing what remained of the sigmoid cavity by means of the saw. An arterial branch was then tied, the edges of the cut integuments stitched together, and a long bandage applied, in the figure of 8, to afford adequate support.

The point which I am most anxious to impress upon you in regard to this operation is the extent of bone that requires removal. It is the carious portion alone that constitutes an obstacle to recovery, and this is in general strictly limited to the surface of articulation. Beyond the comparatively narrow limit thus determined, the bone is thickened and roughened by the effusion of osseous substance in a nodulated or spicular form, as a consequence of the neighbouring irritation, and not from any morbid action different from that which effects union in the case of fracture, or provides a substitute for the sequestrum in necrosis. But from want of due discrimination, the sound bone thus simply roughened has been deemed a fit subject for removal, and as you may see from the representations of Movean, or the Irish Surgeon-General, in regard to his solitary case, three or four inches of the humerus, with a correspondingly unnecessary extent of the radius or ulna, has thus been taken away, with what delay to recovery and imperfection of the result may be easily imagined. As my experience of the operation now amounts to about eighty cases, I am able to speak with considerable confidence upon the subject.

ANEURISM OF THE RADIAL ARTERY.

A few weeks ago, as I was leaving the hospital, a man, apparently somewhat under thirty years of age, showed me a tumour, about the size of a flattened

gooseberry, at the root of his thumb, and said that ligature of the artery at the wrist had been advised for its remedy. The swelling looked so much more like a ganglion than an aneurism, that I supposed there must have been a mistake as to its nature; but, upon a more attentive examination, finding that there was a distinct expansive pulsation, I could not doubt that there was a sac communicating with the radial artery, pressure upon which instantly lessened the swelling, and deprived it of the pulsating character. I therefore had a little spring constructed, upon the principle of a rupture truss, so as to press upon the vessel at the wrist, and at the end of twenty-four hours after it had been applied could not detect any trace of pulsation.

It is seldom that we meet with aneurism of the smaller arteries, especially of the non-traumatic kind; but, by a curious coincidence, I am at present concerned in the treatment of another, along with Dr Moir of Musselburgh. The patient is a young gentleman, about seventeen years of age, who, in consequence of a blow on the side of the head, suffered from a pulsating tumour, about the size of a large olive, in the temporal region, with greatly increased action in all the branches of the artery from the ear upwards. Finding that pressure had a very uncertain control over the disease, I applied a ligature to the artery immediately above the articulation of the lower jaw, with the immediate effect of arresting all visible pulsation. During the week which has elapsed since this was done, the tumour and expanded vessels have shrunk greatly, and under the influence of a lead plate, which the patient is now wearing on the seat of the disease, I trust recovery will be completed.

I recollect a somewhat similar case, in which a young military officer, from falling on the ice while skating, received a severe blow on the temple, whence resulted a small pulsating tumour, the size of a field-bean. As the disease was so small in extent, and so distinctly limited, I cut through the sac, and tied both of the bleeding orifices. The patient suffered no further trouble either from the operation or the disease.

It may appear to you inconsistent in me to apply pressure at the wrist, instead of tying the radial artery, as I have strenuously contended against the substitution of pressure for ligature of the femoral artery. But the two cases are very different, since the latter-mentioned vessel has accompanying it the great venous trunk of the limb, which the utmost extent of human skill and care cannot prevent from being compressed along with the artery, and necessarily occasioning a degree of suffering to the patient which must, if at all prolonged, infinitely exceed the trivial disturbance which attends ligature of the artery,—while the radial artery lies directly under the skin, rests upon the bone, and has no associate disposed to resent the effect of compression. I may add, that it has always been an established principle with me, that the radial artery and its branches at and below the wrist are completely under the command of pressure.

I may take this opportunity of offering some practical rules, in regard to the suppression of arterial hemorrhage, which may prove useful in guiding you through difficulties which afford little opportunity for reflection or consultation. In the first place, you should hold it established, that it is always desirable, if possible, to arrest bleeding from arteries by means applied at the seat of injury. Secondly, you may be assured that bleeding at and below the wrist, and at and below the ankle, is always under the control of pressure, provided it be properly employed,—that is, not superficially, but from lint, or some other suitable substance being introduced into the wound, and made to press directly upon the orifice of the vessel. Thirdly, in wounds of all arteries accessible between the limits just mentioned and the heart, the vessel should be exposed at the seat of injury, and tied on both sides of the wound it has sustained. This principle has been so loudly maintained by Mr Guthrie, that I believe some people have given him the credit of its origin; but it has been long established as a sound principle of practice by surgeons of the highest eminence both at home and abroad, and more especially by Mr John Bell of Edinburgh, in whose "Principles of Sur-

gery" you will find many graphic and impressive lessons of the effects resulting from attention to it, and also from its disregard.

One evening I received a message from the Northern Railway, that there was a steamboat waiting at Granton to carry me across the Firth to Burntisland, where a special train would be ready to proceed onwards, but whither, or for what purpose, there was no information. Having travelled a considerable distance, I met several practitioners, of great experience and intelligence, who were suffering much anxiety in regard to a youth, in whose forearm an incision for an abscess had bled profusely. As it was quite away from the radial artery, the ulnar was concluded to be the source of hemorrhage, and had been sought for by dissection upwards towards the elbow, along the course of the muscles, between which it is wont to run, but without success; and, as the patient seemed little able to bear any further loss of blood, it was deemed desirable to have a consultation as to the most efficient measure of relief, even though it might involve ligature of the humeral artery, or removal of the limb. Acting upon the principle above mentioned, I scratched away the clot at the bleeding point, from which a copious stream instantly issued, but arresting this with my thumb, pressure being at the same time made upon the humeral, I dissected a little through the adjacent texture, and brought into view a large artery, under which a double ligature was passed, and tied on both sides of an aperture distinctly visible in its coats. In less time than I have taken to describe the process, the patient was thus transferred from a state of extreme danger to one of perfect safety. The artery was obviously the ulnar, which had come off higher than usual from the humeral, and pursued an irregular course externally to the fascia of the forearm, thus explaining how it had been wounded by the superficial incision, and how it had escaped the deep dissection.

The fourth rule I have to offer is, that when an aneurism forms after the wound of an artery, the same means should be employed as in the first instance, unless the vessel concerned be of a large size, and admits of having a ligature applied to it, without the intervention of any large branch between the seat of obstruction and the wound. The formerly not uncommon case of aneurism at the bend of the arm, as a consequence of the humeral artery being wounded in venesection, affords a good illustration of the advantage resulting from attention to this rule, since relief was thus afforded much more easily, safely, and securely, than by ligature of the humeral farther up the arm.

To illustrate the exception mentioned, I may relate the case of a young man, who, in one of the most remote of the Orkney Islands, accidentally thrust the blade of a knife into the middle of his thigh, so as to wound the femoral artery. The blood gushed forth with great violence, but was restrained by a compress, formed of eight half-crowns, wrapped in a piece of cloth. The wound healed, and an aneurism soon afterwards appearing, he was sent here to my care. Respect for the general principle, and suspicion, from the purring sound, that there was a communication between the artery and vein, suggested considerations which were opposed to ligature of the femoral, but I nevertheless preferred this operation, as the ligature could be applied without the intervention of any considerable branch; and I accordingly performed it, with the happiest result.

The following case will show the danger of not strictly limiting exceptions to the rule within the limits which have been mentioned. A middle aged woman, in a country town, while walking up a steep and slippery ascent, and carrying a knife, with which she had just killed a pig, fell, and thrust the sharp point of the blade completely through her leg, a little below the knee, entering between the tibia and fibula, and issuing at the lower part of the popliteal space. Blood gushed from both openings, but when she was laid in bed ceased, and did not return. At the end of a fortnight, the wounds having healed, she attempted to walk, and found that a swelling had taken place at the seat of injury, on account of which, by the advice of her medical attendant, she came here to be under my care. On examination, I found a large pulsating tumour in the fore part of the leg, immediately below the knee, and another of equal size in the popliteal cavity.

Feeling unable to determine whether the anterior or posterior tibial, or the popliteal artery itself, was the vessel wounded, and, on the whole, being inclined to think that the one last mentioned was most probably concerned, in which case ligature of the femoral would be the proper course, I adopted this measure. No bad consequence followed the operation, the tumours ceased to pulsate, and favourable expectations were entertained of the result for two or three weeks, when the anterior wound below the knee opened and bled profusely. I dilated it freely, evacuated the cavity of its fluid and coagulated contents, and applied firm pressure between the tibia and fibula, whence the blood was found to issue. Mortification followed, and I performed amputation, without saving the patient's life. There can be no doubt that, in this case, if the true state of matters could have been ascertained, and a ligature applied to the anterior tibial, which was divided just before it passed through the interosseous ligament, both the limb and life of the patient would have been preserved.

Part Fourth.

PERISCOPE

HISTOLOGY.

NEW MUSCULAR SYSTEM DISCOVERED IN THE MUCOUS MEMBRANE OF THE STOMACH AND INTESTINES. BY ERNST BRUECKE, PROFESSOR OF PHYSIOLOGY IN THE UNIVERSITY OF VIENNA.—(*Communicated by Dr Van der Byl.*)

At a meeting of the Imperial Academy of Sciences of Vienna, held on the 13th February, 1851, Professor Bruecke communicated his discovery of a muscular system in the mucous membrane of the stomach and intestines. In examining the intestinal villi of fowls, geese, dogs, and human subjects, with reference to the origin of the chyle vessels, he met with a system of longitudinal fibres, which in their morphological characters were found by him to correspond exactly with organic muscular fibres. He now remembered that already, in the years 1842 and 1843, Lacauchie, Gruby, and Delafond (from observations made by Lacauchie on recently killed animals, and by Gruby and Delafond on living ones), attributed a power of motion to the intestinal villi—and therefore sought to convince himself of the correctness of this statement.

After narcotizing a dog, according to the method proposed by Professor Ludwig of Zurich—by injecting a strong solution of the diluted alcoholic extract of opium into the jugular vein until the animal was entirely insensible, Professor Bruecke laid open the abdominal cavity by cutting through the linea alba—and then opened the small intestine in several places. On now drawing a thin blunt probe very lightly, but somewhat rapidly, across the mucous membrane, he distinctly saw the villi which had been touched shortening and thickening themselves. Professor Bruecke did not see them become long and slender on irritation, as Gruby and Delafond pretend also to have observed; and he doubts this change of shape as an active condition, since he was unable to find any circular fibres in the villi. Nor did the lateral motions take place, as described by Gruby and Delafond, but he does not deny their possibility, since they might be produced by the fibres on one side of a villus contracting, while those on the other side remain relaxed.

Professor Bruecke, now no longer doubting the nature of the fibres observed by him, sought to investigate their extent and attachment in the intestinal

canal of human subjects and dogs, and found that in the villi he had observed only the most remote tendrils (Ausläufer) of an expansive muscular system which extended through the mucous membrane of the stomach and intestines.

The principal layer of this muscular system is situated in the stomach below the pepsin glands,—in the small intestine below the Lieberkuhnian follicles,—in the great intestine below the glandulæ tubulatæ (simplices minores of Böhm), and consists of an external layer of longitudinal and an internal layer of circular fibres. Situated on the inner surface of the latter (or internal) layer, we have an irregular meshwork of these fibres, which surround the basis of the above-named glands, and pass between them to reach the very surface of the mucous membrane, where they are only covered by the epithelium, basement membrane, and capillary vessels. It is to this meshwork and *its tendrils* that the villi owe their contractile powers.

Note by Dr Van der Byl.—In sending this communication, I may add, that through the kindness of Professor Bruecke, I have had an opportunity of examining this new muscular system in the stomach and intestines, and convincing myself of the contractile power of the villi. The following remarks may perhaps aid those who are willing to repeat the observations described in this paper.

1. *To observe the Contractions of the Villi.*—The intestine being slit open, we must not rest disappointed if the contractions do not take place immediately. In the last dog experimented on by Professor Bruecke, the contractions of the villi became strongest about a quarter of an hour after the internal surface of the intestine had been exposed to the air; and, indeed, they may not set in until twenty minutes or half an hour after they are laid open. The contractions are not to be expected where the surface is granular (for here the villi are already contracted, as we may see from their thick round apices), on the contrary, the florid velvet-like surface, covered by a thin layer of transparent mucus, will exhibit the contractile power of the villi most satisfactorily.

2. *To Demonstrate the Muscular Fibres in the Fresh Villi.*—Remove a few villi from the intestinal surface, and spread them out on a glass (dissecting them a little, if necessary). On examination with the microscope, we usually find some broken through, and see the fibres hanging from the broken extremity. With a high power, we can then examine the fibres individually.

3. *To Demonstrate the Layers of this Muscular System.*—For this purpose it is necessary to boil the stomach and intestines in dilute acetic acid, or common vinegar, after the manner proposed by Purkinje, many years ago, for other purposes. When the organs are very thin, we wait until the acid boils, and then dip them in for a few minutes;—thick organs, however, are put in when the acid is placed on the fire, and removed as soon as it boils. After such boiling, the stomach and intestines are spread out, and allowed to dry moderately. When too dry, they become very brittle, and it is then extremely difficult to get good sections. Since the layers are comparatively very thin, for their better observation the sections should be made obliquely, not perpendicularly, to obtain a greater surface.

MEDICINE.

ON THE TREATMENT OF PRURITUS OF THE GENITAL, ANAL, AND AXILLARY REGIONS. BY DR TOURNIE.

What is generally designated by the term pruritus of the genital and axillary regions, is a cutaneous affection, under the form of prurigo, lichen, or eczema, of which the itching is nothing more than a symptom, and one of the principal phenomena, although it may sometimes exist without any apparent alteration of the skin, and without any other characters than that of the itch itself.

Without constituting a true disease, the itching is not the less a most inconvenient and disagreeable affection; moreover, it may sometimes determine, in nervous persons, consecutive symptoms of manifest importance. We know

that onanism and nymphomania often owe their origin to these violent itchings.

There are few practitioners who have not had occasion to observe some one of these troublesome affections; and who, perhaps, have more than once deplored the inability of the materia medica to cure this intractable malady. Among the remedies in common use are corrosive sublimate, borax, sulphur, camphor, iodine, &c. With the aid of these means cures have been obtained; but none, in Dr Tournié's experience, produced such constant and successful results as the following, which he employed in nine cases of pruritus of which he has reported the details:—1. An ointment of calomel, in the proportion of from 4 to 6 parts of calomel to 30 of axunge. 2. A powder, composed of four-fifths of starch and one-fifth of camphor, well pulverised and mixed. We may, however, in the ointment, increase the proportion of protochloride of mercury; and, in the powder, the dose of camphor, according to the obstinacy of the disease.

The mode of application is as follows:—If the diseased parts are covered with scales or dry crusts, as in eczema, then separation is promoted by cataplasms and emollient baths; having accomplished which, he applies, twice a-day, frictions with the foregoing ointment, and, after the frictions, sprinkles the parts with the mixture of starch and camphor.

The ointment alone is inefficacious; and the camphorated starch, without the assistance of the ointment, allays the itch, but does not effect a cure. Experience has proved this.

The following are the cases of which he has reported the details:—1. Prurigo of the labia majora and vulva—intertrigo. 2. Lichenoid affection of the labia majora—intertrigo. 3. Chronic eczema of the scrotum. 4. Lichen in axillary region. 5. Itching of anus. 6. Ditto, do. Besides these, he notices three other cases of perfect cure by this treatment, two of lichenoid affections of the anus in men, and one of prurigo of the labia majora in a woman. He does not give the details of these last three cases. In one only—viz. that of the fourth case—was it necessary to increase, in a notable manner, the doses of calomel and camphor; in all the other cases, the ordinary formula was sufficient to produce a cure.

When the itching of the anus is caused by the presence of ascarides, the calomel and camphor are less efficacious; but he has recourse in this case to mercurial ointment, and often verified its vermicide effects on this species of worm.

The state of pregnancy seems to neutralise the action of the therapeutic means, the success of which he has recorded above. Thus the itching of the genitals in two pregnant females proved intractable to every kind of treatment. In one only venesection seemed to allay the itchiness a little; but in the second it produced no effect;—in both, the itching disappeared immediately after delivery. In these two cases the itching had commenced during pregnancy; and it existed without giving the vulva any appearance of a lichenoid or pruriginous eruption.

Dr Maslieurat-Lagémard has published, in the 12th No. of the "Gazette Médicale," for 1848, page 204, a very curious and remarkable account of a general itching supervening, without eruption, in the course of eight pregnancies, in one of his patients. This did not commence till after the sixth month, and no treatment appeared to alleviate the violent itchings, which during the eight pregnancies resulted six times in premature deliveries.

In Dr T.'s hands the combination of the calomel ointment with the powder of camphorated starch, in the treatment of itching of the hæmorrhoidal region, has given results which no other treatment has ever obtained in so constant a manner.

Every practitioner who has been consulted for these affections has been obliged to direct himself to formulæ of great variety, before hitting on a successful expedient. In illustration of this, it is sufficient to relate the summary of five observations published by Dr Ruan, in "Hufeland's Journal."

Of these five cases, two were treated without any success by purgatives, diuretics, opium in large doses, Goulard's extract, and solution of borate

of soda; intractable to all these means, they were cured by the balsam of copaiva.

In a third case, the copaiva, which had succeeded in the two first, was inefficacious; whilst the borax, which had failed in the others, caused the itching to disappear in this case.

A fourth case resisted the divers means employed for the three first, and gave way to the administration of the subcarbonate of soda in small and repeated doses.

The fifth case resisted every treatment.—*From L'Union Médicale*, 16th January 1851.

[Dr Tournié takes no notice of hydrocyanic acid, which, in the proportion of a drachm to eight ounces of almond emulsion, we have found frequently to give great relief. In *prurigo ani* and *pudendi*, we have more frequently succeeded in effecting a cure with a weak solution of corrosive sublimate (one grain to eight ounces) than with any other application. The state of the digestive function and urinary secretion should always be attended to, whatever be the local treatment.]

OBSERVATIONS ON THE URINE IN PNEUMONIA. BY DR REDTENBACHER.

The author was engaged in the year 1846, in making a series of experiments on the urine of patients labouring under different acute and chronic diseases, with the view of determining,—firstly, the relation which subsists between the presence of inorganic salts, and the specific gravity of the urine,—and secondly, whether the increased or diminished proportion of these saline constituents is connected with the formation or retrocession of certain morbid processes. His observations were made in Skoda's wards in the Great General Hospital of Vienna. He found, that the amount of inorganic salts was not subject to much variation, and consequently affected the density of the urine in a very trifling degree; but that its density depends upon the inorganic compounds, and especially upon the urea and uric acid. In cases of pneumonia, he has satisfied himself that the amount of chlorides in the urine undergoes variations dependent upon the stage of the disease. Healthy urine, as is well known, deposits a copious precipitate on the addition of nitrate of silver. Redtenbacher asserts that in all the cases of pneumonia (amounting to 80) in which he has applied this test, the faintness or absence of the precipitation has indicated a diminution or absence of chlorides. The precipitate becomes less distinct, from day to day, till resolution commences, when the chlorides again gradually resume their normal proportion. Diminution of the chlorides likewise occurs in typhoid fever, in acute arthritis, and in capillary bronchitis; but is neither a constant phenomenon in these diseases, nor observed at any certain period of the morbid processes. In cases of tubercular infiltration chlorides are found in the urine in normal proportion, but suffer diminution for a short time during each fresh infiltration.

Redtenbacher anticipates the objection as to the value of these observations—that the amount of the chlorides of the urine depends upon the amount of culinary salt used with the food, and that the quantity of the former will be diminished with the supply of the latter,—firstly, by showing that the chlorides are not equally diminished in the course of other diseases, in which restricted diet is rigidly enforced,—and secondly, by mentioning that Skoda's cases of pneumonia, on which his observations were made, were, from beginning to end of the disease, allowed a drink acidulated with muriatic acid. He believes, consequently, that the uniform diminution of chlorides in the urine of pneumonia, is somehow essentially connected with the morbid process.—*Zeitschrift der k. k. Gesellschaft der Aerzte zu Wien, August* 1850. P. 373.

SURGERY.

REMOVAL OF INFERIOR MAXILLA. BY PROFESSOR MOTT, OF NEW YORK.

In speaking of the extirpation of the lower jaw, Professor Mott alludes to the different modes of operating, which have been adopted in these cases, and especially to the one suggested and practised by Dr J. R. Barton, of Philadelphia, of leaving a narrow rim or border of bone, at the base of the lower jaw, when this is practicable. This is a very great improvement, for which, he thought, Dr B. deserved the greatest credit. In removing one-half of the lower jaw or at the temporo-maxillary articulation, much hemorrhage is to be expected, from the division of the numerous branches of the temporal and internal maxillary arteries. Dr M., therefore, recommends that the common carotid be tied previous to removing the jaw at the articulation, as tending greatly to diminish the subsequent hemorrhage. He mentioned as a very curious and most important fact, that in four cases of extirpation of the lower jaw, where he had previously tied the carotid, and operated on the *same day* on which he had ligatured that vessel, the hemorrhage was notwithstanding this still very profuse. In five other cases of extirpation, where ligature of the carotid had been made *the day previous to extirpation*, the hemorrhage had been very little, so that it would appear that the best plan of diminishing the hemorrhage, is to extirpate the jaw *the day after* the carotid is tied.—*Professor Mott's Lectures, Reported in New York Register of Medicine and Pharmacy, Vol. I., No. XI., p.* 169.

[Unless in the case of epulotic growths, which involve merely the alveolar process, leaving a part of the base of the jaw would render relapse almost certain. The branches of the temporal are easily secured, and so is the internal maxillary, if cut, which it never need be. Ligature of the carotid, therefore, is wholly unnecessary.]

FALSE ANEURISM OF THE RADIAL ARTERY. BY M. VELPEAU.

A coach-guard, thirty years of age, on the 16th January, was stabbed in the lower and anterior part of the right forearm with a knife; and, when admitted into La Charité, at Paris, on the 30th, stated, that the direction of the wound was obliquely outwards and downwards; that the integuments united within three days under the use of graduated compresses, which were applied by a medical man immediately after the stab had been inflicted; but that a vast ecchymosis at the same time extended from the forearm to the axilla, while a large tumour made its appearance about the cicatrix. The compresses were re-applied till the ninth day after the stab. On their removal, the tumour was found to be more circumscribed, and to beat synchronously with the pulse. When examined in the hospital, there was seen, half-way between wrist and elbow, a little inside the normal course of the radial artery, and on a level with the point where the inner border of the supinator longus in muscular subjects begins to cover the vessel, a tumour of the size of a small hen's egg, and evidently aneurismal, as shown by all the usual signs. The patient was left without treatment till the 8th February, *that it might be seen what would become of the tumour*. On the 9th, M. Velpeau applied Petit's tourniquet, and compressed the humeral artery against the bone, on a level with the inferior attachment of the coraco-brachialis muscle. A refrigerant mixture, consisting of two parts of ice and one of gray salt, was applied to the tumour for seven or eight minutes. The tourniquet was ordered to be relaxed in the evening, if the swelling of the arm proved too great. In the evening, the swelling being excessive, the colour of the limb a dark violet, and a large phlyctena having formed over the whole part which had been in contact with the frigorific mixture, the tourniquet was slackened, and next morning removed altogether. From this time pulsation ceased to be felt in the tumour, which rapidly diminished in bulk; and, when the patient was last seen by the reporter, its size did not exceed that of a hazel-nut. The pulsations of the radial artery below the tumour were greatly enfeebled, soon after

the application of the compression;—when last examined, they were found to have nearly regained their usual force.—*Gazette des Hopitâux*, 13th March 1851.

[In our opinion it would have been safer and better practice to have opened the tumour, and tied the vessel on both sides of the wound in its coats.]

EXCISION OF THE SUPERIOR MAXILLARY BONE.

The "Dublin Medical Press" of 8th February transcribes, from the "British American Medical and Physical Journal," two cases of this operation, performed by Dr Macdonnell of Montreal, in which he used the single curved incision recommended by Mr Syme, to whom he gives the credit of suggesting it, instead of Professor Mott, who seemed to have no good ground for his claim. Upon this the Dublin editor thus comments:—

"In claiming the discovery of the above method, and dating its origin so far back as 1829, Mr Syme seems to forget that in the last edition of his 'System of Surgery,' published in February 1842, exactly one year prior to the appearance of his second paper in 'Cormack's Monthly Journal,' he himself recommended *exclusively* the two incisions. These are his directions for the first stage of the operation:—'In performing excision of the superior maxillary bone, *two incisions* should be made through the cheek, one extending from the inner angle of the eye directly downwards to the lip, the other beginning over the junction of the maxillary and malar bones, and terminating at the angle of the mouth.'—(p. 487). If Mr Syme really considered his plan so great an improvement, as he appears to do in the papers published twelve months after the above sentence was sent before the profession, he must have acquired his experience of its merits in the interval between February 1842 and February 1843."

[In justice to Mr Syme, we beg to remark, that the claim he established in 1829 was not to the form of incision in question, which did not occur to him till long afterwards, but to *the operation itself*, which had not before been performed by any surgeon of Great Britain or Ireland, and of which the case he published in July of that year is the first that stands on the records of surgery.]

MIDWIFERY AND DISEASES PECULIAR TO WOMEN.

CASES OF LABOUR, COMPLICATED WITH ULCERATING CANCER OF THE WOMB. BY DR OLDHAM.

Case I. Ulcerating carcinoma of cervix uteri during pregnancy—Breech presentation—Rupture of the uterus.—Patient, 33 years of age, and the mother of four children. Principal symptom—a frequent coloured discharge. At the seventh month, she was attacked with bleeding (vivid red), for two days, succeeded by a profuse watery discharge, accompanied with a burning pain in the lower part of abdomen and back, but less intense than is usual with the same disease in the unimpregnated state. The child moved freely.

There was malignant disease of the cervix, which had destroyed a large portion of both lips of the os uteri; the tissue of the cervix around the os was thick, hard, and uneven, and there was a hardened bar of solid matter on the anterior segment of the womb. At the middle of the ninth month, she suffered from aggravated false pains; the disease had extended over the whole interior of the canal of the cervix, which canal felt of stony hardness, and was broken up into a rough, deeply-rutted bleeding tissue. Labour came on, and the breech presented; the front wall of the cervix was indurated, thick, and unyielding, like a mass of hardened mortar. The bladder was distended. The dilatation of the os was not sufficient to allow of interference. Died on the second day. At the post-mortem examination, on opening the abdomen, the liquor amnii and blood ran out. The uterus was distended with gas. The fœtus projected through its back wall. The size of the

uterus, and the thickness of its walls, were natural. The inner surface of the cervix was thinned by disease, deeply furrowed, and hard. The interior of the body of the uterus was free from disease, and the vagina healthy.

Case II. Carcinoma of cervix uteri—Pregnancy—Delivered by Craniotomy.—Patient 40 years of age. Os uteri prevented from dilatation by a mass of ulcerating cancer in the cervix. There was a fœtid discharge which inflamed and excoriated the external organs. The head was felt through the disc of os uteri. Perforation of the head was resorted to, and it passed with little laceration of the cervix. Placenta strongly adherent—was peeled off with the hand Died about a month after. Post-mortem examination:—Malignant ulceration of cervix—on posterior walls of vagina ulcerated patches, which perforated them and communicated with the pelvic peritoneum. Peritonitis had supervened—purulent effusion in peritoneum—and a small cyst of the size of a chestnut in the right ovary.

Case III. Cancer of the uterus and vagina—Death of Fœtus in utero—Easy birth—Perforation of vagina.—Patient 42 years of age. Had born 12 children. After first month of pregnancy suffered from watery discharges—bleeding and severe pains in loins, hips and legs. Cancer of the womb. She was easily delivered of a dead male child at the full time. Both child and secundines were putrid. Discharges of blood mixed with fæculent matter followed. On examination, there was found to be an advanced stage of ulcerating carcinoma of the cervix uteri and posterior wall of the vagina, both of which were extensively destroyed. There was an opening into the rectum through which the fæces passed.—*London Journal of Medical Science, March* 1851.

CASE OF LACERATION OF THE UTERUS, AND INTERNAL HEMORRHAGE. BY MR ROBERTON.

Nov. 9, 1832, at 3 o'clock A.M., N. G., æt. 37, tenth child, was seen by Mr Clough, being reported by the midwife as in tedious labour. The funis was in the vagina, the *os uteri* dilated, and the head beginning to fill the cavity of the pelvis. At 9, *i. e.*, six hours after being seen by Mr Clough, the patient died suddenly and unaccountably.

Inspection.—The left side of the *uterus*, somewhat anteriorly, presented an immense ecchymosis, in one part of which there was an opening into the uterine cavity of about two inches, and through this the bag of membranes (and within the bag the foot of the fœtus) protruded. On enlarging the opening, it was discovered that the substance of the *uterus* had given way to a considerable extent, and that a part of the fœtus lay in the rent, covered merely by the membranes and the peritoneal coat of the *uterus*. The rupture had extended in part through the placenta; and several pounds of blood had escaped, and lay under the peritoneum, in the left *iliac fossa*. The fœtal head was rather firmly impacted in the cavity of the pelvis, the vertex just beginning to turn under the arch of the pubis. On withdrawing the head, the funis, which was very thick, was found prolapsed, the fold lying in the conjugate diameter of the brim, on the left side of the *symphysis pubis*, and, by pressure, had caused an indentation on the side of the fœtal head. Inside of the *symphysis pubis*, just at the junction, there was an exostosis, about the size of a large bean; the smallest diameter of which projected into the pelvis. The antero-posterior diameter of the brim was three inches and three-quarters. Transverse diameter, five inches and a half. The child, a well-formed male, weighed nine pounds and three-quarters, avoirdupois. The duration of the labour, previous to the rupture, was about *twelve hours.*—*Physiology and Diseases of Women, and Practical Midwifery.* 1851. P. 296.

MEANS TO PREVENT LACERATION OF THE PERINEUM DURING LABOUR. BY DR EICHELBERG.

To prevent the occurrence of *laceration* of the perineum during labour, Dr Eichelberg proposes that it should be *incised* to some extent during a pain! The incision of the perineum is not a new operation; but, certainly, its application by

Dr Eichelberg in ordinary cases of labour is equally novel and unjustifiable. Since 1835 he states that he has operated fourteen times with success. The incision is not made in the raphe, where it would have most effect in dilating the orifice of the vulva, but towards the side (*la vulve dans sa partie epaisse*).—*Rhein. Monatsschrift*, Juillet 1850, and *Revue Med.-Chir.*, 1851.

ACCOUNT OF A NOSENCEPHALIC[1] FŒTUS ADHERENT TO THE PLACENTA. BY MM. J. GUITARD AND JOLY.

This malformed fœtus was born living, at Toulouse, on the 26th July 1850. It lived forty-two hours, cried, and swallowed a little sugared water. The mother, a primipara, was 24 years of age. She stated that at the beginning of her pregnancy she had frequent quarrels with her sister, who on one occasion exclaimed, "I hope the child you bear will prove a monster." From this moment the mother was, she stated, continually possessed with this idea. She feared the realisation of her sister's maledictions, and this fear pursued her unceasingly up to the time of her delivery.

The placenta was adherent by membranous bands to the face, and to a nervous-vascular tumour, which represented the brain. The mouth was wide open, the nose scarcely perceptible, the eyes protruded from their orbits, a palatine fissure existed, the right superior maxilla was detached from an intermaxillary bone which was present. With the exception of considerable development of the abdominal region, and flexure inwards of the feet, the body externally presented nothing remarkable. Internally, the heart was placed on the right side of the chest, without transposition of the abdominal viscera. The external female genital organs were largely developed; but the internal sexual organs were wanting, the vagina terminating in a thin membrane.

The placenta was small, oval-shaped, and of the kind called *placentus en raquette*. The umbilical cord was about four inches in length, and was formed of a single vein and artery.

MM. Guitard and Joly attach considerable importance to the mental impression, the possibility of the influence of which in causing arrest of development, they consider to be placed beyond a doubt by the histories of other malformations. This fœtus is pointed to by the authors as proving the existence of the intermaxillary bone in the human species; also the independence in development of the urinary and genital organs—the latter being in this case wanting—while the former were perfectly formed.—*Comptes Rendus*, p. 396; *Medical Gazette*, Feby. 28, 1851.

MATERIA MEDICA.

ACTION OF METALLIC MERCURY. BY DR A. W. M. VAN HASSELT.

The author alludes to the opinions of Christison and others, that the effects of mercurial vapours, and of the mercurial preparations made by trituration, are due to the conversion of a certain proportion of the metal into oxide, the metal itself being supposed by these authors not to be active. He inclines to the opposite opinion, but does not advance anything new in refutation of the above doctrine. He mentions, among others, a statement of Oesterlen, that after the external, as well as internal, administration of mercurial ointment to animals, globules of metallic mercury are found in the blood, the bile, the urine, the spleen, liver, and kidneys, and especially the lungs. [This, however, in no way proves that the mercury may not have entered in the form of oxide, and been subsequently reduced in the body.]

With respect to fluid mercury itself, it is admitted that the heavy metal can

[1] "*Nosencephalic*—having the skull open only on the frontal and parietal regions, the posterior fontanelle being distinctly present.—*Dunglison's Medical Lexicon.*

pass rapidly, and without change, through the intestinal canal; and both in Germany and England it is held as undemonstrated that metallic mercury can cause death [Sobernheim and Simon, Taylor and Christison]. On the other hand, according to Orfila, metallic mercury can act as a poison if it remains some time in the intestines, so that it can be divided and absorbed. These different authorities found their respective opinions upon experiments and facts, which are to be found in the ordinary works on the subject, and which, therefore, we need not quote.

The author conceived that the contradictory results following the use of metallic mercury may depend upon the large doses, causing it to pass so rapidly through the bowels that it does not produce any effect. He, therefore, made the experiment of administering it in quantities of a few grains.

A healthy strong rabbit, weighing 1·54 lb. Flemish, got, between the 22d January and 3d March, thirty-five doses—ten grains each—of well purified bright metallic mercury, which was administered through an elastic catheter,—the animal remaining under observation during all this time. The movements, the appetite, and all the functions generally went on naturally, except that the gums were a little reddened; and there occurred, from time to time, a diarrhœa, during which the fæces, even from the third day, contained minute globules of mercury, scarcely discernible by the naked eye; also, towards the end of the experiment, there was slight loss of flesh.

From the accumulated fæces four drachms of quicksilver were obtained, so that about two drachms were wanting. No mercury could be detected in the urine. The rabbit was kept two months more under observation, and remained healthy. It then got one or two doses of half an ounce each, which quickly passed off by stool. During the last experiment it produced six young ones, which were either dead-born, or died immediately after birth. A month afterwards, on 14th June, the animal was killed and dissected.

In the head and abdomen nothing remarkable was observed. The posterior half of both lungs sank in spring water; they were more yellow than natural, and here and there exhibited inflammatory redness. The posterior lobes appeared externally to be tubercular; they felt hard in many parts, and showed small globular elevations, around which the interlobular cellular tissue appeared condensed or atrophied, as in cirrhosis of the lungs. When these were cut across, they discharged small glittering globules of metallic mercury; and the larger nodules exhibited distinct vomicæ, with thickened walls. Under the pleuræ, at the edges of the lungs, single scattered metallic globules were seen with a lens. There had thus taken place a mercurial infiltration into the lungs. That this had not taken place from direct injection of the mercury into the lungs by the catheter, was established by a comparative experiment, when the catheter was purposely introduced into the larynx, and also by another experiment, where mercury was injected into an opening made in the trachea.

This affection of the lungs had been observed by Cruveilhier, Gaspard, Moulin, and Viborg, when mercury was injected directly into the circulation; also by Oesterlen, in cats which had been rubbed with mercurial ointment. Analogous to it is the *pneumonia oleosa*, which Gluge and Thierness observed after direct injection, or long-continued internal use of the fixed oils; and the purulent affection of the lungs in pyæmia may also be regarded as analogous. The author draws the following conclusions regarding the subject:—

1. That not only finely-divided, but also fluid, mercury can pass from the intestines into the blood when it is given in small doses.

2d. That the quicksilver does not require to assume the state of oxide or chloride, but can be absorbed as metallic mercury.

3d. Its further poisonous action may manifest itself as a peculiar affection of the lungs, which, according to its duration, may certainly become dangerous to life.

4th. That metallic mercury is one of the substances which ought not to be sold except under prescription. That, in relation to toxicology, the question,

whether metallic mercury can act as a poison? must be answered in the affirmative.—*Niederland. Lanc.*, Aug. 1849, in *Schmidt's Jahrbücher*, No. 10, 1850.

TREATMENT OF PAINFUL AFFECTIONS OF THE SKIN. BY M. CAZENAVE.

M. Cazenave employs successfully, in the treatment of painful skin diseases, such as eczema, prurigo, &c., the following ointment, which rapidly alleviates suffering by causing the cessation of the violent itching:—Chloroform, 1 to 2 parts; azunge, 30 parts. Mix and make an ointment.—*Union Médicale*, 7th December 1850.

RAPID REMOVAL OF STAINS OF NITRATE OF SILVER.

A young officer had so dyed his downy beard with a pomatum containing nitrate of silver, that for fourteen days his face was hideously bespeckled. Brieger pencilled the stains over, first with tincture of iodine, then with a well-diluted solution of caustic potash, and subsequently washed with water. The stains under this treatment disappeared immediately—more rapidly than with the solutions of iodide or cyanide of potassium commonly used. The same method applies to stains of nitrate of silver on stuffs, &c.—*Journ. für Prakt. Pharm.*, February 1850, in *Schmidt's Jahrbücher*, No. 11.

[We did not succeed, on our own hands, in getting these instantaneous results; but we have often facilitated the removal of silver stains by aqua potassæ alone, which enables us readily to rub off the discoloured epidermis without hurting our hands; but we do not venture to commend this for the soft faces of young officers.]

MEDICAL JURISPRUDENCE.

SOLIDIFICATION OF FOOT-PRINTS ON THE SOFTEST SOILS, IN CRIMINAL INVESTIGATIONS. BY M. HUGOULIN.

The scientific witness ought never to neglect, when it is possible, to preserve articles submitted to his examination, in order that they may be produced on trial, and, if necessary, submitted to the inspection of a jury. The foot-prints of man or of animals, the marks of the wheels of carriages, of the butts of guns, sticks, &c., are in criminal investigations too frequently neglected on account of their fugacity, although they might often lead to important results.

If it were possible for the medico-legal chemist to solidify, without altering their form, marks even on the lightest soils, such as sand, the dust of roads, &c., the officers of justice would find a new line of evidence opened to them, which might be applied to a multitude of cases; for shoes, wheels of carriages, the weapons of the accused, &c., are easily got hold of, and there might thus be had during a trial objects of comparison which might, if required, be submitted to repeated inspections. The following is a proceeding of easy execution, which the author has had occasion to put in practice with M. Gras, chief pharmacien of the civil hospital, in a very important case (a triple murder and arson), where the results were not without influence in leading to the discovery of the true culprits. In this case, however, the investigation was readily made, the steps being imprinted on newly dug earth, easily impressible. The mode of procedure in this case could not be applied to such impression on dust or sand—that to be now described is applicable in every case.

When a crime has been committed, and the information leads to the belief that proof may be obtained from the footsteps, the first care of the officers of justice should be, to protect the best marked impressions with a box, a cask, or some similar utensil, to defend them from all alteration by wind, snow, or rain, and to commit the guarding of the spot to a trustworthy party, to give time for the "expert" to arrive at the spot.

The first care of the experimenter should be to erect a barrier on the side to-

wards the wind before raising the protection which has been placed over the foot-prints. Then follow the operations necessary to *solidify* the fugitive impressions.

He should have at his disposal a certain quantity of stearic acid (stearine candles), pulverised chemically—which may be done by dissolving the stearic acid in its own weight of hot rectified spirits,—pouring the acid so dissolved into a large quantity of cold water, which should be stirred with a spatula,—collecting upon a cloth, expressing, and placing it on sheets of paper to be dried by the air. We thus obtain the acid in the state of an impalpable powder.

The experimenter should procure a fragment of thin plate iron, a little larger in size than the impression it is wished to solidfy; he must then turn up the edges of it, and pierce them at different points with holes to facilitate the passage of air, he afterwards places the sheet of iron upon a wire grating in such a way that the bars of the grating shall be about the distance of half an inch from the impression; a few pieces of brick will serve to fix the grating at this distance. The sheet of iron must then be covered with burning charcoal; the iron becomes heated, afterwards becomes red, and by radiation heats the impression. Iron wires may be fixed at the corners of the sheet of iron, so as to raise the temporary furnace at pleasure, or replace it on the grating.

The impression thus heated to a temperature above 212°, about three ounces of the pulverised stearic acid are to be placed on a close-meshed hair-sieve and then sifted on the impression. The acid falls like a powdering of snow, so light in weight that it cannot alter the impression, however faint it may be; it scarcely has reached the ground when it melts and is absorbed by the earth; it may be thrown on until the soil becomes cold enough not to melt the stearic acid further. An excess of this may still be allowed to fall, which excess may be made to melt by bringing afresh the red hot plate of iron over the footprint.

The operation is then completed, and the ground is left to cool perfectly. The time of cooling varies; but it is better to err on the side of excess, that there may be no risk of destroying the impression. The operation will be accomplished more speedily in winter than in summer. If the ground is very light, consisting of sand, dust, or ashes, it will be sufficient to lift the cast with the hands, by the assistance of a spatula passed under the foot-print. It is then placed with care on a linen cloth folded several times, the edges of which are slightly elevated by means of little fragments of wood or stone, in order to form a sort of basin, of which the stearic mould of the foot-print is the bottom, and thin clean plaster of Paris is then poured into this basin to a uniform depth of about an inch. When the plaster becomes solid, the impression is hard enough to be packed in a box between two layers of cotton wadding.

If the ground is more firm, a small circular ditch is dug with care round the footprint, the sides being stiffened with a little plaster, and undermined from below, so as to lift it without injury; this being detached, its bulk is reduced by paring with a knife, and the operation is finished with a coating of plaster as before.

If the foot-print should be upon ground which is muddy or marshy, it would be necessary, before heating the foot-print, to form a circular ditch, and to fill it with dry plaster, which would absorb part of the moisture in becoming solid; it should then be scooped out from below, and the whole lifted, that it may be carried to dry in the shade; it will in this way harden without cracking. It is only after two or three days of drying that the operation with stearic acid can be attempted.

It is easy to see that this process can be applied, with some variation, to all cases of foot-prints upon the ground; and that, upon inspection, the procedure above described may be modified according to circumstances. A hot smoothing iron, a dish cover, or a portable oven, may be substituted for the plate of sheet iron.

These experiments were repeated at a public meeting in the amphitheatre of the Naval Medical School of Toulon, in presence of the president of the civil authorities, and professors; they succeeded so as completely to satisfy all present.

One only objection has been suggested, "How can foot-prints on snow be taken off?" The above process is not applicable to this case. After the next winter, the author will publish a process by which he hopes to be able to solve this difficulty.—*Journal de Pharmacie, January* 1851.

Part Fifth.

MEDICAL NEWS.

EDINBURGH MEDICO-CHIRURGICAL SOCIETY.

MEETING IV.—*February* 19, 1851.—Dr BEGBIE, President, in the Chair.

PUBLICATION OF SOCIETY'S PROCEEDINGS.

After the minutes were read, *Dr Richard Mackenzie* stated, that an erroneous account of the proceedings of the Society had been recently published, in which the facts of the case read by him before the Society, on the 19th January, had been misrepresented. He begged to ask the president, whether the Society could take any steps in this matter?

The *President* explained, that a subject of this nature must be considered by the Council before being formally brought under the notice of the Society.

The minutes were then approved of.

CASE OF PERINEAL SECTION. BY PROFESSOR MILLER.

Professor Miller narrated a case of perineal section, which he would have noticed at last meeting, had he not been absent. The operation had been performed according to the old method, without a director, the stricture being impermeable. The patient died, on the 19th day, of pyæmia.

Professor Miller begged permission of the Society to take this opportunity of enlarging somewhat on the subject of perineal section, on account of stricture; and sought to establish, among others, the following conclusions:—That all strictures are not permeable to the catheter or bougie. That the great majority of strictures which are permeable to these instruments are capable of being cured by them safely and satisfactorily, without the use of the knife. That these instruments are neither so insufficient nor so injurious as some authorities have represented. That certain permeable strictures exist, peculiar in resistance or irritability, in which perineal section is advisable; but that such cases are comparatively few. That in them the operation practised by Mr Syme, with a grooved director, is preferable to the old method without a guide through the strictured part. That certain strictures exist—still fewer than the preceding—which, being truly impermeable, are amenable only to the old method of operating. That such operation is not so difficult, uncertain, or dangerous, as has been sometimes alleged. That all forms of perineal section are not devoid of risk, and ought not to be performed, unless under the pressure of absolute necessity. That any such operation is not to be expected to be alone capable of permanently curing stricture; but that in almost all cases the subsequent use of the bougies will be required to secure a sufficient and permanent dilatation.

Dr Dunsmure agreed with Mr Miller in thinking, that the operation recommended by Mr Syme was a most useful one in those cases in which a tight car-

tilaginous stricture existed, and that the great use of the perineal section was to give the surgeon a start, as it were, in the treatment. He was of opinion, that cases did occur in which it was almost impossible, from the irritability of the urethra, to conduct the treatment by dilatation alone to a satisfactory termination. That although he advocated the perineal section in bad cases of stricture, yet he believed that the majority of the cases of urethral contraction could be easily and rapidly cured by dilatation, and that consequently the perineal section would be required comparatively seldom. Three cases had been published by Dr D., in the "Monthly Journal of Medical Science" for November 1850, in which the perineal section had been had recourse to. In the first of these, although the urethra had been opened into, yet the operation as recommended by Mr Syme could not be performed, owing to the stricture extending from fully an inch anterior to the scrotum to the middle of the membranous portion of the urethra, —so that the results of that case could prove nothing; but he thought it right that he should now mention what he knew of the third case. When the patient left the hospital, No. 13 went easily through the stricture; but at his last visit he was only able to pass at first No. 10,—11, 12, and 13 being, however, introduced immediately after; and he (Dr D.) had no doubt, from the tightness with which the bougie was grasped, that, if instruments were not passed, the stricture would ere long become as bad as it was before the operation. Judging, then, from the slight experience he had had from this case, he was inclined to think that instruments would be required to be inserted from time to time after division of stricture by perineal section; but that, notwithstanding, he believed Mr Syme had conferred a great boon upon the profession by showing them a means of treating very bad strictures, and especially those with an irritable state of the urethra, such as one of those alluded to in the course of Mr Miller's communication.

Dr Mackenzie remarked, that in estimating the danger attending the division of the stricture on a grooved director, the consequences of the old operation should be kept entirely out of view. The two operations were essentially different. In the operation proposed by Mr Syme, the limits of the part requiring division were distinctly defined, and the extent of the induration was felt by the finger, when the urethra was exposed by external incision. The point of the knife was made to enter the groove in the instrument immediately behind the induration, and pushed forwards, till the gristly texture constituting the stricture was fairly divided. The urethra was thus divided in the mesial line, and no more was cut than the contracted portion. In the old operation, on the contrary, the knife was plunged into the dilated part of the canal behind the stricture, and was carried forwards as nearly in the course of the urethra as the operator could guess, till it met the point of a catheter introduced down to the seat of stricture. In addition to the incision being thus a more extensive one, there was no certainty as to the parts which were divided, as to the urethra being opened in the mesial line, or even as to the constricted part of the canal being laid open at all. A passage was certainly established, by which the catheter could be introduced into the bladder, but a very different one from that formed by cutting on a grooved instrument passed through the stricture. Dr M. thought, that the term "impermeable stricture" could not properly be applied with so wide a signification as that given to it by Mr Miller; that cases in which part of the urethra had been obliterated, as the result of contusions or lacerations of the perineum, could not properly be included in the term "stricture of the urethra;" and that the term "impermeable stricture," if used at all, should only be applied to cases of narrowing of a part of the canal, in which the surgeon was foiled in his attempts to pass an instrument. In detailing the consequences of the different modes of treatment of stricture, Mr Miller had made no mention of the serious, and even fatal, consequences which occasionally ensued, from the introduction of bougies, in cases of old-standing and irritable stricture. The occurrence of severe constitutional irritation following the use of bougies, in such cases, appeared to Dr M. to be one of the strongest arguments in favour of the operation proposed by Mr Syme. Dr M. believed, that when the operation of division of the stricture

on a grooved director had been fairly tried, it would be generally acknowledged to be an important improvement in the treatment of the disease, although he thought that the class of cases requiring such a proceeding would be found to be more limited than at first contemplated by Mr Syme.

Professor Miller replied, that he had endeavoured to draw the distinction between the "old operation" and Mr Syme's as broadly as possible, both as to the mode of performance, and the cases to which they were applicable. He had carefully avoided confounding the two in any way. One of his objects had been, not to put the old operation on a par with the new, but to show, that some cases might be expected to occur to which the new operation was not applicable, and in which the old might and should be performed,—the more especially, as the dangers and difficulties of the latter seemed to him to be certainly over-stated by the out-and-out advocates of the former procedure. He had also, he thought, made out the fact incontestably, that "impermeable strictures" do occur. But, certainly, Dr Mackenzie had suggested one way of avoiding adhesion to that fact—viz., by change of nomenclature—calling changes in the urethra, when it remains permeable, *strictures*, and when there is no passage for the urine, *obliterations*. In most of the cases which Professor M. had adduced as samples of "impermeable stricture," there was no true obliteration of the urethra; and in one, at least, there was no complication with fistula or external injury. Professor M. had alluded, as fully as time would allow, to the use of the bougie; but mainly to defend it from the imputation of what seemed to him undeserved discredit. There was a risk attendant on its use, no doubt, in bad cases; but surely neither greater, nor yet so great, as the "ordinary risks" that attend on perineal section, even of the best kind. Dr Mackenzie had mentioned a case of his (Dr M'K.'s) own, in which the patient had died after the introduction of the bougie, and apparently in consequence of that operation. This proved nothing but an instance of extreme constitutional irritation,—unfortunately, not uncommon. And surely Dr Mackenzie would not say, that the same man, under the same circumstances, would have had a better chance of living, if, in addition to the bougie having been introduced, perineal section had also been performed. Professor Miller begged it to be expressly understood, that he did not appear as an opponent of Mr Syme's operation. On the contrary, he believed it to be an excellent operation, and preferable to all others in certain cases. But, at the same time, he was bound to believe that such cases were very few; and that, at present, there was a risk of this really good operation being put to disadvantage, by a natural tendency in its zealous advocates to impart to it a too wide and unqualified application.

MEDICAL REPORT OF THE EDINBURGH EYE INFIRMARY [INSTITUTED IN 1834] FOR THE YEAR 1850, WITH TABULAR VIEW OF THE CASES. BY ROBERT HAMILTON, M.D., F.R.S.E., F.R.C.S., AND BENJAMIN BELL, ESQ., F.R.C.S.

The experience of each passing day must convince every intelligent practitioner that the healing art, like most others, is a progressive one. This observation, clear beyond dispute, concerning the sciences upon which medicine and surgery are based, and especially in relation to the knowledge which the microscope is revealing, is applicable to almost every branch of professional pursuit, so that the medical man who would neglect this important truth, and disregard these rapidly accumulating discoveries, is speedily arrested, like a stranded bark, whilst others, pursuing a different course, soon leave him far behind.

No department of our science presents perhaps more striking and palpable demonstrations of this important fact than ophthalmic surgery; and the slight notice we now proceed to give of the contents of the accompanying table[1] will

[1] For which we are indebted to our excellent clerk, Mr Robertson.

afford many illustrations thereof. Much has been done within the last few years towards the elucidation of the delicate structures of the different parts of the eye, investigations which must be familiar to many of our auditors, and among which the recent researches of Professors Todd and Bowman are perhaps the most important.[1]

Diseases of Patients.			*Diseases of Patients.*	
Tumours of eyelids,		12	Corneitis, phlyctenular,	5
Abscess of do.		5	Injuries of iris and eyeball	5
Hordeolum,		2	Iritis, simple,	4
Ophthalmia tarsi,		58	Do. scrofulous,	2
Ectropium,		1	Do. rheumatic,	1
Entropium,		3	Do. syphilitic,	2
Trichiasis,		4	Ophthalmitis,	1
Ptosis from affection of third nerve,		3	Granular conjunctiva,	3
Epiphora,		5	Abscess of cornea,	1
Inflammation of lachrymal sac,		13	Opacities of do.	16
Fistula of sac,		3	Ulcers of do.	5
Shrunk papillæ and contracted puncta,		2	Prolapsus iridis,	1
Strabismus,		9	Staphyloma,	5
Oscillation of eyeball,		2	Synechiæ, opacities of capsule, &c.,	9
Injuries of conjunctiva,		14	Conical cornea,	1
Hypertrophy, &c., of do.		3	Cataract, congenital,	2
Inflammation of do., catarrhal,	5		Do. idiopathic,	12
Do. neonatorum,	5		Do. traumatic,	10
Do. gonorrhœal,	3		Myopia,	1
Do. puro-mucous,	9		Presbyopia,	2
Do. pustular,	15		Asthenopia,	8
Do. strumous,	23		Day blindness,	1
Do. acute, }	119		Night do.	2
Do. chronic, }			Amaurotic affections,	42
Do. rheumatic,	10		Cases unclassified,	20
Injuries of cornea,		19		
Corneitis,		16	Total,	525

The prefixed table brings under review more than fifty of the different diseases to which the eye is liable, in numbers varying from one or two only, to several scores; and were we to bestow upon each of the subjects two or three sentences only, our Report would become unreasonably long; hence we must make but a scanty selection, and must strictly confine ourselves within very moderate, not to say narrow, limits.

THE CORNEA.—The large proportional number of cases of blindness which arise from the diseases of the cornea, confer on the disorders of this membrane an importance which cannot readily be overrated. The researches of the gentlemen just named, and of others, seem to have established that *the horizontal layers*, of which the cornea is composed, and which are somewhat moveable upon each other, amount in number to about sixty. Of these two are distinguished by being especially *elastic* in their structure, the one on the *anterior* portion of the cornea proper, upon which again is immediately superimposed, and quite superficial, the conjunctival epithelium; and the other, *the posterior elastic lamina*, with which we have been long familiar as the membrane of Decemet, and which, like the anterior one, is also lined, of course posteriorly, with an epithelial pavement. These peculiar laminæ are sheets of homogenous membrane, very much resembling the capsule of the lens, being perfectly transparent and glossy, firm, resisting, uniform, and without appearance of internal structure.

[1] See Physiological Anatomy and Physiology of Man. By Dr Todd and William Bowman, F.R.S. Also, Lectures on the Parts Concerned in the Operations of the Eye, &c. By William Bowman, London. 1849.

AFFECTIONS OF THE CORNEA.—The number of the affections of the cornea enumerated in our table amounts to seventy, which constitutes nearly one-eighth part of all the cases. Regarding them, we shall make a few brief remarks upon *The Lodgement of Foreign Bodies in the Membrane*, on *the Hernia Corneæ*, on *Opacities*, *Staphyloma Corneæ*, and *Conical Cornea*.

Puncture of the Cornea.—A preliminary hint, however, suggests itself respecting *the surgical puncture of the cornea*. From the preceding remarks concerning the minute structure of the membrane, it will have been perceived that nothing was said of blood-vessels: and it is a fact that, in its normal condition, such vessels are entirely wanting. The membrane belongs purely to the capillary system. Hence it is that within certain, and not very confined, limits, this tissue is exceedingly tolerant of injury, as is known to every surgeon who is called frequently to operate upon it; and hence the perfect safety with which the membrane may be punctured for the purpose of evacuating the aqueous humour,—an expedient which will be found not only free from risk in itself, but which is attended with most marked benefit in not a few of the most painful and hazardous disorders of the organ; so proving a remedy which we believe is too much overlooked and too little practised. In illustration, we may mention, that within these few weeks, there was a patient in the Infirmary, a man past the prime of life, who had frequently suffered from severe rheumatic attacks, and who, after much recent exposure and fatigue, was affected with acute rheumatic ophthalmia of the right eye, with intense pain, and greatly impaired vision. Although the usual remedies were vigorously applied, yet the symptoms were persistent, and the danger to the eye great. Under these circumstances, we punctured the cornea, inducing marked relief of suffering and subsidence of the disease. The symptoms, however, after a few days returned; the prospect was again threatening, and again relief was obtained by means of the puncture. Thus this measure proved twice the turning point in this troublesome disorder; and the patient left the institution free alike from the complaint, and from the slightest sequelæ of the disorder.

Foreign Bodies imbedded in the Cornea.—It is interesting to perceive how a correct knowledge of the minute structure of the parts now under review throws light upon the most common perhaps of all the injuries of the eye, and which among our working population—blacksmiths, masons, engineers, &c. &c.—so frequently presents itself to notice.

It has often and long been observed as remarkable, not to say unaccountable, that injuries from these minute foreign bodies,—the *stahlfunken* of the Germans, many of them known as "*fires*" in this country,—are never met with in the sclerotica, but in the cornea only. The cause of this is now clear; inasmuch as the elastic tissues above alluded to, are peculiar to the cornea; and it is the anterior and stronger of these which forms the *nidus*,—the almost horny seat in which the foreign bodies get ensconced, where they cause so much irritation, and whence, by the powers of nature, they are with so much difficulty dislodged. Art, it may be said, has preceded science in the proper treatment of these cases. The touching and tampering with the epithelial covering ever proves exquisitely painful; but this membrane when extensively removed, is not less wonderfully than speedily restored. The best plan to pursue is to interfere as little as possible with the soft superficial membrane, to plunge the point of the needle at once beneath the impacted body, and so to effect its immediate excavation.

Hernia Corneæ, is an affection which finds an explanation at once most natural and satisfactory in the accurate knowledge of the minute anatomy. How common is ulceration of the cornea deep and penetrating? By and by, it appears complete, and the aqueous humour seems on the eve of being evacuated, but that the apparent breach of the cornea is plugged up by a transparent vesicle, which, sometimes for days, closes the opening, arrests the progress of the disease, and the threatened escape of the aqueous humour. This vesicle was wont to be regarded as the serous membrane, the delicate lining of the aqueous humour; whereas it is firm and resisting, and is in fact the posterior *elastic* lamella, which is not

easily excited to morbid action, and long withstands all tendency to inflammation and its hurtful consequences. It has sometimes been recommended to touch this plug with the lunar caustic, under the idea that inflammation might be thereby excited, and effusion of coagulable lymph produced; whereas the preservation of this membrane constitutes the safety of the aqueous humour and the iris, so that everything should be done to maintain its integrity. When at length, however, the elastic membrane fails, and *hernia iridis* and myocephalon occur, a different treatment must be resorted to, and the caustic may be applied.

Specks and Opacities of the Cornea.—A knowledge of the minute structure above adverted to, is most useful in guiding our prognosis, as well as sometimes regulating our treatment, in those numerous and vexatious cases so well known under the general appellation of opacities of the cornea. Regarding these cases, the experience of the past year has strongly impressed upon our notice the decided improvement which has often taken place upon specks on the cornea, when treated with gently stimulating applications, such as the solution of nitrate of silver. Under the continued use of this agent, and other gentle excitants, many of those depositions which, we feared, had seriously compromised distinct vision for life, have slowly, but entirely disappeared;—a result attributable, undoubtedly, mainly to nature's work, but probably somewhat assisted by the persevering use of the resources of art. The rationale of the process, and of the amount of its salutary agency, as well explained by Mr Bowman, seem to be, that the proper laminated tissue, above adverted to, is capable of enlarging its vascular resources for its support under disease, and for the subsequent removal of diseased products to such an extent, that, if it has itself escaped disorganisation, it is able, under favourable circumstances, completely to resume its transparency.

Opacities from Acetas Plumbi.—In this connection we may advert to an opacity, which perhaps, however, would be more accurately characterised as a deposit on the abraded cornea, produced by the ignorant and improper use of the *acetas plumbi*. We have witnessed more than one case of this sort within the year, and one of a highly aggravated character, where the deposit was so extensive and prominent, that it proved a severe source of irritation to the unfortunate sufferer, who, under its use, had become quite blind. Having become impatient, under a painful and somewhat protracted attack of ophthalmia, he had resorted to one of those quacks, who undertake to cure all sorts of disordered eyes, and who had supplied him with an infallible ointment, which he largely distributes. This the patient had used for a considerable time before he applied to us. Strongly suspecting the hurtful nature of this unguent, we desired the sufferer to bring the ointment which he had been using, and which we handed over to Dr Douglas Maclagan, who had the kindness to analyse it, and with the following extraordinary result:—"Mr. P.'s ointment," writes Dr Maclagan, "is made up of an immense quantity of acetate of lead, with a large proportion of red precipitate, the whole being coloured with *alkanet*, or some other vegetable matter. The sugar of lead is in crystalline particles, large enough to destroy any cornea by mechanical irritation, leaving the red oxide of mercury out of view." The white incrustation was unmistakeable; and we once and again succeeded in removing large scales from the surface, with marked relief. It is no wonder that, when such gross practice is pursued, with such injurious effects, a strong prejudice has arisen in the minds of many practitioners against the use of the salts of lead. But on this point we would observe, that it is the abuse and not the use which ought to be reprobated,—a remark the more necessary to make, since our frequent experience very strikingly vindicates the high encomium which was paid by the late Mr Tyrrell to the efficiency of the diacetate of this metal in removing, with other remedies, the troublesome granulations of the lining membrane of the palpebræ, which in so many instances affect the whole corneal conjunctivæ, greatly irritating the organ, and obscuring vision. The judicious use of this salt, as recommended by Mr Tyrrell, has often been most striking and satisfactory.

Staphyloma.—We are not aware that these microscopic discoveries have

thrown any additional light upon the subjects of staphyloma and conical cornea. For the former of these complaints we have had two operations: the one case being congenital, and spherical; the other conical, the result of previous inflammation. The former occurred in an infant, eighteen months old: it was put under chloroform, and lay under the painful operation more quiet than the shorn lamb.

Conical Cornea is not a very common disorder. In the Royal London Ophthalmic Institution, during the last ten years, it has not occurred in one case in a thousand, the numbers being 1 in 1088. As the case noted in our table was interesting from its complexity, and from the beneficial effects of treatment, we shall take the liberty of giving it in detail.

CASE.—The patient was a young female, eighteen years of age. Both eyes were affected, and presented the usual features of the disease, in a decided manner; the sparkling appearance, when viewed opposite the light, the conical crystal-like projection in profile. The complaint was farthest advanced in the left eye, and in the right, was combined with a completely-formed white cataract: in neither eye was there any nebulous opacity of the apex of the cornea. The vision of the right eye, where the cataract existed, had been extremely imperfect for about four years, and at the date of her application, barely enabled her to distinguish light from darkness. With her left eye she could discriminate objects held at the distance of two or three inches; but she was incapable of distinguishing one person from another, and needed to be led about, except in localities with which she was familiar. The light of a candle appeared to her to sparkle, and to be of various colours. In connection with this symptom, it may be remarked, that no inequalities were observable on the surface of the cornea.

Her general health seeming to admit of considerable improvement, it was deemed advisable to make this a primary object before attempting any surgical procedure for removing the cataract. She improved much under a course of alterative and tonic treatment, and then the operation of *keratonyxis* was performed, with Jacob's needle, in the usual way. The result has been so satisfactory that she can now read even small print without the use of a glass, and has perfect confidence in going any where unattended. Encouraged by this success, and remembering Sir W. Adam's practice in a similar case, we have since had recourse to the same measure in the other eye; and although, as was to be expected, a temporary cataract has been produced, absorption of the lens is now advancing rapidly, and there is reason to hope that eventually the result will be almost equally favourable. We say *almost*, because the conical projection of this cornea being more decided, the benefit following absorption of the lens will probably be less considerable.

THE OPHTHALMIÆ.—Upon the ophthalmia generally we must abstain from enlarging.

Gonorrhœal Ophthalmia.—Three cases of gonorrhœal ophthalmia have come under treatment, all of them marked with the urgent and violent character so striking in this disorder. In all of them one eye only was implicated. We attach very considerable importance, in this class of cases, to the early use of free radiating incisions of the sclerotic portion of the conjunctiva, which, it is alleged, when in a state of intense chemosis, strangulates the vessels going to the cornea, and superinduces thereby its sudden sloughing and mortification. Be that as it may, this practice was had recourse to in our cases. In one, the cornea got off scathless; in another, it quite recovered, after considerable ulceration of its superior circumference; in the third, occurring in a young man in the height of vital power, but who did not come into the infirmary for several days after his attack, when sloughing had already commenced, the cornea was seriously compromised, and the vision nearly lost.

Aquo-capsulitis.—The microscopic researches above referred to, appear to throw an amount of light upon the disease hitherto known as aquo-capsulitis, which probably will require a modification of our views respecting the pathology of the disorder. It is the doctrine of some of our British oculists, that the

aqueous humour is contained in a proper serous membrane, by which it is secreted and absorbed. The microscope, however, it appears, ignores this doctrine; and confines what has hitherto been considered a serous membrane to the posterior part of the cornea alone. These researches in this matter corroborate the views which have been long held on this point by some eminent German authorities. The disease known under the name is an interesting and important one. An insidious and chronic inflammation appears to be set up at all events upon the posterior membrane of the cornea, composed of the epithelial cells. This occurs usually in individuals in a weak state of constitution, and gradually induces obscurity of vision, with slight uneasiness, rather than pain. Upon first looking at the eye, something like an obscure opacity of the cornea is perceived, which, on minute examination, appears to have its seat in the deeper layers. It is like a general haze or cloudiness, which, however, when examined with a lens, resolves itself into what appears to be the deposition of numerous minute points, as if some darkish-coloured powder had been dusted on the inner surface of the membrane. Many are the cases we witness in which what is here described is too clear to be mistaken. That the disease, however, never proceeds further, and affects the posterior chamber, we are not prepared to aver. On the contrary, we are still inclined to the belief that there is, at all events, a closely allied disease, which we have often witnessed, and which occasionally implicates in a serious manner both the iris and crystalline capsule. In these cases, the iris becomes changed in colour and fringed with lymph, and sometimes adherent at its margin, while in the middle it bulges forward, as if the posterior chamber was morbidly distended with fluid. An account of this disease was given, many years ago, to this Society, by our late able colleague Dr Watson Wemyss, under the title of Chronic Inflammation of the Iris and Membrane of the Aqueous Humour. Is it, then, that we must recognise two diseases affecting these parts rather than one? There can be no question that the anatomical doctrine must greatly affect our opinions regarding the sympathies and diseases of the parts, and hence the importance of the inquiry in a practical point of view. Meanwhile we remark, that the efficacy of improving the general tone of health, and of the gentle operation of mercury, is frequently, in the former class of cases here alluded to, not less striking than in syphilitic iritis.

ARTIFICIAL PUPIL.—We have only had one case of operation for artificial pupil during the year; the success has been gratifying; and we supply a slight sketch of it.

W. N., aged 18, had violent inflammation in both eyes, about ten years ago, which deprived him of all useful vision. In the right eye there is complete *synechia anterior*, with a large central opacity of the cornea; in the left, *synechia posterior*, and an opaque capsule, from deposition of lymph, but the cornea is transparent. Both eyes are of the natural shape and firmness. With the right eye he can barely distinguish light from darkness; with the left the sensibility to light is greater, but no objects are discernible. The left eye seemed to be the more promising for operative interference; and accordingly, on two occasions, the procedure, to which Mr Tyrrell gave the name of "Drilling," was had recourse to, with the view of partially detaching the iris from the opaque capsule, and also of promoting solution of the lens. Small advantage followed these attempts, and as it appeared probable, from the size of the anterior chamber, that little of the lens remained, we determined on performing what is commonly called Cheselden's operation for artificial pupil. The iris accordingly was divided in the usual way, and a large triangular pupil was the result. The patient now enjoys a considerable amount of vision, being able to discern persons on the street, and the lamp-posts; also the dogs running about, and the larger signs above the shop-doors, &c. Had the nervous apparatus not been out of use for so long a period, the recovery, which, however, is still progressing, might have been more striking and complete.

CATARACT.—The number of the cases of cataract which have presented themselves during the year has been twenty-four; the precise number enumerated in the last Report of the Glasgow Eye Infirmary we have seen, dated

in 1850. Of our number, two were congenital, ten traumatic, and twelve idiopathic; and it appears that only eight or ten of these have been the subjects of operation. With respect to these operations generally, we have to remark, that whilst some were performed in the Infirmary, others were undertaken in the patients' dwellings, so that our statistics regarding them are not so minute and accurate as we could have wished.

The following are the only remarks which our limited space permits:—Concerning the operation of *Extraction*, continued experience only confirms the sentiment, which is perhaps more felt than acted upon,—the dogma, namely, which the German school has been so forward and urgent in advocating, That perfect integrity of the organ itself, and of the whole system, are essential pre-requisites and elements to the operation of extraction. So delicate is the organ, and so numerous the risks, that unless the above conditions exist, this method of relief should not be attempted. All this, *ex cathedra*, is very well; but complications and qualifying circumstances present themselves, which sometimes induce, if they do not sanction, an infraction of the rule. Thus the following case occurred in an individual aged 63, in good general health. Forty years previously, when in the army, he had suffered severely in both eyes from Egyptian ophthalmia, on account of which he was discharged. The right eye had been rendered useless from opacity of the cornea, and general disorganization. The left relatively escaped; but both, subsequently, were liable to troublesome inflammatory attacks. Though the vision was far from perfect in the left eye, yet it was long sufficient to enable the patient to walk about with considerable facility. About four years however, previous to the date of his application, the dimness of this eye began to increase from the growth of cataract, and gradually the vision, for all practical purposes, became useless. He now found himself in that cheerless state of dependence and distress, so common under the circumstances, and urgently besought that any operation which held out even the slightest prospect of relief might be undertaken.

What, under the circumstances, was the office of good surgery, and of humanity? *Couching* of the cataract might readily have been effected, but with a high degree of probability, amounting almost to certainty, that the displaced and foreign body would act as a severe and endless source of annoyance, greatly hazarding the recovery, and irritating the parts in all time coming. Remarks of similar bearing apply to the operation by *Solution*, especially in such a case, where the cataract appeared to be hard, and the eye was far from perfect. The objection applied nearly as strongly to the operation of *Extraction;* so that the question occurred,—Was all interference to be declined? This, perhaps, would have been the wisest, as the safest, decision. The patient, however, was urgent; his existing vision was useless; the matured cataract undoubtedly acted as a screen, which shut out the light of day, which, if removed, *might* restore vision; the general health withal was good, so that the temptation was great to endeavour to remove the obstructing cause at once, and for ever.

The operation proved safe to the organ, though ineffectual as a cure. True, as was anticipated, the incision was scarcely completed ere the lens appeared, and with it a portion of the somewhat disorganized vitreous humour. This, however, was to no prejudicial amount; and the cornea healed somewhat slowly, but satisfactorily. Now, the patient flattered himself and friends that his sight was restored; and, again, he complained of blindness still. Upon the whole, this case, though satisfactory as to the eye, was ineffectual—from the state of the retina we believe—as to the restoration of vision.

Couching.—Our estimate of couching apparently concurs with that of the officers of the principal institutions throughout the country. During the last ten years, for example, cases of depression, in the Royal London Ophthalmic Hospital, to the other two operations, have been in the proportion of 26 to 451 extractions, and to 396 divisions,—in other words, of one couching in every 32 operations; and in Glasgow, in the year above specified, it was in the ratio of one couching in 18 operations.

minister of the place, with whom we have conversed upon the case, and who, from first to last, has taken a very lively and intelligent interest in it. Feeling solicitous that no doubt or uncertainty should exist regarding it, we thought it advisable, in drawing up our report, afresh to apply to Mr Renton, who has sent us the following specific details:—

"W. J. could see no object *by day*, but from the very first of his illness, could see by *night*. He found no deficiency of sight by night, which was the same in all respects as before his illness. For example, he could not have walked on the road *by day*, without stumbling, and he required a guide. *At night*, he could do so quite well. On the night of his return from Edinburgh last March, it being quite dark (about nine P.M.), he found his way home to his own house, from the coach-office, ALONE, which during the day he could not have done.

"When standing at the door of his cottage at night, he could see distinctly the opposite wall (there are no houses directly opposite his), which divides the road from the fields; he could distinguish the top of it, and if asked, could have given a good idea of its height. All this he could not have done by day.

"He could see by night, when looking westwards, a large house at about 200 yards distance, while during the day, he could not see half the distance of the length of himself.

"At night, looking at the sky, he could quite well discern the stars and the moon, and the clouds passing over the moon. During the day he could see nothing; a cloudy and a clear sky being alike invisible.

"In a moon-light night he could distinguish animals passing his door,—for example, a cow from a horse. By day, he could not possibly have done so.

"In conclusion, his power of vision by night continued during all his illness, and was not at all affected by the recovery of his sight by day."

Amaurosis.—The following amaurotic case, which apparently belongs to that variety, whose cause is referable to congestion of the optic nervous apparatus, we select chiefly because it happens to be confined within narrow limits:—

J. M., æt. 49, a coach-hirer, of dark complexion and spare habit, and by his own confession rather intemperate, had been troubled for a month with an increasing dimness and imperfection of sight in both his eyes. He attributed his disease chiefly to sleeping for several weeks in a damp room. He was unable to distinguish a man from a woman across the street, to read the signboards of the shops, or the large capitals on our admission cards. There was no vascularity of either eye; but the pupils were sluggish, and their expression vacant. His pulse was deficient in power, and his tongue furred. The case seemed decidedly unpromising. The treatment was at first directed mainly to his constitutional health. The bowels were cleared out with moderate doses of croton oil, and a few ounces of blood were taken from the temple by cupping.[1] He was then directed to take cod-liver oil, and a blue pill at bed-time, until the gums should be slightly affected. He commenced this course on the 7th of September, and on the 19th, vision was so much improved, that he could read the large print upon our cards without difficulty. The mercury was steadily continued, and on the 21st of November, two months afterwards, it is noted,—"vision still improving; he can read the smallest print easily." He was never confined during the treatment; and when last seen, in the beginning of January, all amaurotic symptoms had disappeared.

We here take occasion to remark, that in employing the atropa belladonna as a mean of exploring the deeper-seated parts of the eye, the solution of the atropine will be found a much more agreeable as well as powerful agent than the extract of the juice of the belladonna itself. So energetic are its powers, that in some instances circumspection is required, as it not only induces great mydriasis, but sometimes, and especially in amaurotic cases, so long maintains the increased

[1] We feel much obliged to Mr Cafe for his daily and assiduous services to the Institution.

dimness of vision, that the patient is both alarmed and discouraged, whilst the medicine and the practice are apt to be stigmatised and condemned. Such panics, however groundless, it is well to avoid.

AFFECTIONS OF THE APPENDAGES OF THE EYE.—We have little room left for remarks upon cases or diseases affecting the appendages of the eye. they must be confined to a few sentences.

Affections of the Third Pair of Nerves.—These cases have been enumerated in the table under the name of Ptosis, this perhaps being the most prominent symptom of the complaint. As during the past year, three cases have presented themselves, the affection cannot be regarded as a very rare one; and a fourth case has since appeared, evidently occasioned by the *Bacchanalia* of the new-year's-day festivities. In one of our cases, the disease was double, by which we mean, that it occurred in both eyes, though more severely in the one than in the other. In this instance, though the vision was very imperfect, and the iris was not strikingly dilated, the power was decidedly improved when exercised through a small hole in a card. Whenever the palsied eyelid was raised, giddiness to a distressing extent, as usually happens, was superinduced. Dr Marshall Hall, we should think, is quite correct in stating that this affection is an instance of cerebral paralysis; nor is this view at all contradicted by what is stated by Mr Wharton Jones, that the disease is sometimes produced by exposure to cold and damp, like that *blight* which so frequently occurs in the *portio dura* of the seventh pair, inducing the paralysis of the numerous muscles it supplies. We have not happened to see any instance of the ophthalmic disorder proceeding from this cause. Our cases have been all obstinate, and success in the treatment, when attainable, will be the result of searching and enlightened views of the cause of the disorder,—at once general, as it respects the condition of the whole frame, and accurate, as it regards its true and peculiar exciting cause.

Strabismus.—The number of our cases of squinting, amounted to nine; and it was but a small proportion of these that required, or admitted, of operation. One case presented itself of the *Strabismus sursumvergens*, not a very common disorder. It occurred in a youth aged sixteen years, who had been annoyed with it from infancy. In the morning, after being refreshed with sleep, he was comparatively but little incommoded; but as the day advanced, and the exhaustion of fatigue supervened, it became exceedingly vexatious. We offered to attempt its removal by operation, to which the patient most willingly consented. His parents, however, refused their concurrence, and prohibited him from submitting. In the cases in which we have operated, the results have been such as to vindicate the high character and complete success of the operation.

CONCLUSION.—But we must conclude. Whilst we had planned at the commencement of our report, to illustrate the importance of every practitioner keeping abreast of the rapid advances making in anatomy, physiology, and the other correlative sciences, we had moreover, intended to include what we consider a not less important topic, namely, the regarding the local diseases of the eye, not as isolated, but also, and chiefly, as in connection with the whole frame, whenever and howsoever morbidly affected. This we would venture to affirm, in many surgical and medical cases occurring in ophthalmological disorders, is the true secret of successful practice. The principle is nothing more than what Mr Abernethy so ingeniously demonstrated nearly half a century ago; and what we believe particularly distinguishes, at the present time, many members of the profession who are successfully introducing improvements, and establishing for themselves an honourable and exalted fame. We believe that ophthalmic practice might afford a most striking demonstration of this important truth; and we regret that we may not now enlarge upon the theme. There are few of the disorders which have been above adverted to,—the most important and the most insignificant,—whether asthenopy or amaurosis on the one hand, or the slighter, as ophthalmia tarsi, or mucocele of the sac on the other, in which it is not most important for the practitioner to direct attention not only as much, but even more, to the constitutional disorder than to the local disease. The truth of this in some com-

plaints is so obvious, that it cannot escape the observation of the most careless attendant. Thus is it in *arthritic ophthalmia*, perhaps the most troublesome of all its forms; and in the rheumatic, scarcely less vexatious. We were lately called to a patient labouring under rheumatic ophthalmia in both eyes, complicated with constitutional venereal disorder, whose ocular pain was so severe, that for three nights, as he expressed it, he had not had a wink of sleep. He could scarcely discern a single object, his pulse beat 120 in the minute, and at the time he had no rheumatism in any other part of the body. The specific appearances, however, of the ophthalmia were most marked, and the treatment indicated, was precisely that required for articular rheumatism or rheumatic fever. This was vigorously adopted, and with speedy and most marked relief. The principle we are now contending for has long been familiar to every one, as it regards strumous ophthalmia; in many varieties of which it would scarcely be too much to affirm, that nine-tenths of the treatments must consist of constitutional remedies—correcting the diathesis—and the remaining tenth of local: And the gist of our thesis is this, that a principle somewhat similar, a similar enlargement of view, with corresponding practice, at once varied and specific, is required in a vast number, we would say the majority, of ophthalmic disorders. In a word, all judicious and efficient practice should be based alike on an intimate acquaintance with physiology, pathology, and therapeutics; and unceasing attention should be directed to the digestive organs which supply the nourishment to the frame,—to the respiratory, which renovate the vital fluids,—to the circulatory system, which distributes these,—and last, though not least, to the excretory organs which should maintain the purity and salubrity of the whole.

PUBLICATIONS RECEIVED.

Ninth Report of the Birmingham and Midland Counties Pathological Society. Birmingham, 1851.

General Index to the First Thirty-Three Volumes of the Medico-Chirurgical Transactions, published by the Royal Medical and Chirurgical Society of London. London: Longman. 1851.

Religion and Science: their Independence of each other, and their Mutual Relations. By a Physician. Lond.: Churchill. 1851.

Dr Hooper's Physician's Vade-Mecum. New Edition. By Wm. Augustus Guy, M.B., Cantab. London: Renshaw. 1851.

Medicines: their Uses and Mode of Administration. By J. Moore Neligan, M.D., &c. Third Edition. Dublin: Fannin & Co. 1851.

The Transactions of the American Medical Association. Vol. iii. Philadelphia, 1850.

Surgical Anatomy. By Joseph Maclise, F.R.C.S. London: Churchill. 1851. Fasciculus VIII.

On the Causes, Symptoms, and Treatment of Spermatorrhœa. By M. Lallemand, of Montpellier. Translated and Edited by Henry J. M'Dougall, Surgeon, &c. Second Edition. London: Churchill. 1851.

Essays and Notes on the Physiology and Diseases of Women, and on Practical Midwifery. By John Roberton, formerly Senior Surgeon to the Manchester and Salford Lying-in-Hospital, &c. London: Churchill. 1851.

Journal of the Statistical Society of London. Vol. xiv. Part I. March, 1851.

Cases in Midwifery. By the late John Green Crosse, M.D., F.R.C.S., with Introduction and Remarks by Edward Copeman, M.D., F.R.C.S., &c. London and Norwich, 1851.

A Letter to Lord Campbell on the Chloroform Clause in the Proposed Prevention of Offences Bill. By John Snow, M.D., &c. London, 1851.

COMMUNICATIONS RECEIVED.

We have received communications from Dr Van der Byl; Dr Wilson, Kelso; Mr Fraser, Aberdeen; and Dr Potts, Cranbourne, Dorset.

A letter, addressed to the Editor, by Professor Miller, we must decline to insert in this Number of the Journal.

PERINEAL SECTION.

LETTER TO THE EDITOR, IN REFERENCE TO MR SYME'S COMMUNICATION, BY PROFESSOR MILLER.

[Since the last sheet went to press, the following communication has been received from Mr Miller:—]

51, Queen Street, March 24, 1851.

MY DEAR SIR,—This evening, I wrote to you to the following effect:—

"I think it right to address you in your official capacity, as Editor of the 'Edinburgh Monthly Journal,' as well as Secretary to the Edinburgh Medico-Chirurgical Society; taking the liberty to remind you that simple justice and ordinary fair dealing require that no statement of the questions between Mr Syme and myself, as exhibited at the last meeting of the Medico-Chirurgical Society, shall appear in the 'Monthly Journal,' without a sufficient opportunity having been afforded of laying *both* sides of the question fairly before the professional public."

At a quarter to eleven o'clock P.M., I have received your answer, with the proof of an article by Mr Syme, containing groundless personal charges against me, which, when originally made at the recent meeting of the Medico-Chirurgical Society, I had met and *disproved*. These charges, as they now stand, seem to be somewhat modified from their original form; but still remain sufficiently offensive to require indignant contradiction and refutal.

This is the first notice you have given me of your intention to publish Mr Syme's paper in this number of your Journal, without any report of the discussion to which it led; an instance of haste and hurry all but unprecedented, at least since the Journal came under its present management.

You ask me to furnish you with my "reply," "by to-morrow afternoon at four P.M." It is now midnight. To-morrow, as usual, unavoidable professional engagements will occupy my every hour, from early morning till past the time you have specified. It is, therefore, manifestly impossible for me to furnish you with the documents on my part, which ought to have accompanied Mr Syme's paper. Herein, I consider myself aggrieved.

If Mr Syme's paper *is* published in your April Number, you have put it out of my power to furnish any other *present* reply than the letter I now write. And, while I protest against this, I shall expect as an act of simple justice, that you afford me space, in the May Number, for a short and dispassionate statement, embodying the oral refutation which Mr Syme's charges against myself have already received. Meanwhile, I beg to append a note from Dr Matthews Duncan in reference to the case to which Mr Syme alludes; and, taking that as a sample of the evidence which I may have it in my power to produce, I have little doubt the profession will spontaneously withhold their judgment until they have heard both sides of the question.

"6, Wemyss Place, March 26, Vespere.

MY DEAR SIR,—I have read, in the 'Lancet of last Saturday, an account of your case of aggravated urethral stricture, in which Mr Syme operated by his own new method.

"I was present at, and assisted in, the operation.

"In the course of the third night afterwards, I was called from my bed by the patient's attendants to see him.

"I found him in the most serious condition of collapse (coldness of skin—feebleness, almost absence, of pulse at the wrist—vomiting, rigors, &c.)

"From this time till the period of his leaving Edinburgh, I saw the patient daily.

"I can confidently state, that your account in the 'Lancet' of the sequence of phenomena, and their interpretation, in this case, during that period, is to my certain knowledge faithful, unbiassed, and correct in every particular.

"I am, my dear Sir, yours very truly,

"To Professor Miller." "J. M. DUNCAN.

Dr J. M. Duncan having alluded to the narrative of the case in the "Lancet," it is right to insert it here:—

"In illustration of such risks, I may here briefly narrate the leading particulars of a case in which Mr Syme and I have a common interest; and as we have also a common object in view—namely, the truth—it seems immaterial from which party the narrative shall proceed, the more especially as I extract the particulars from an account written by the patient himself—a gentleman of middle age and great intelligence. He had long laboured under tight and very irritable stricture. He had been under the care of Mr Liston and myself, at various times; but the remarkable irritability of the urethra prevented continuous treatment, and his life was embittered by oft-returning attacks of cystitis. This seemed a case in all respects suitable for perineal section. It was just possible, that by the aid of chloroform the bougie alone might have ultimately removed irritation, and accomplished a cure—as has happened to me in other examples (Principles of Surgery, 2d edition, p. 792). But, on the whole, Mr Syme's operation seemed preferable. And, accordingly, this having been agreed to in consultation, it was performed by Mr Syme on the last day of January 1850. After the operation, suppression of urine took place, during twenty-four hours, along with unpleasant symptoms of *shock*. Fever set in, accompanied by pervigilium, and great general uneasiness. After forty-eight hours, the catheter was removed. On Feb. 3, the constitutional disturbance became extreme; as indicated by violent sickness and vomiting, rigors, loss of voice, cold blue surface, feeble pulse, and recurrence of suppression of urine. After about twelve hours' continuance, these symptoms yielded to stimulants.—Feb. 5: There was great uneasiness about the scrotum and perineum; and on the 7th, abscess had formed in front of the wound. This was opened, and through the aperture urine as well as pus was discharged; the former continuing to pass through this wound as well as through the original one for many days. The greater part of the urine, however, came per urethram, in a flat yet free stream; and without that peculiar distress to which the patient had previously been so long accustomed. When the wounds had nearly closed, a bougie was passed; but the effect was to re-open the wound, with increase of pain in the urethra. And, in consequence, at the urgent solicitation of the patient, the bougie was refrained from till a more advanced period of the case.—Feb. 22: The urine had become very loaded and fœtid, and continued to be of a depraved character for nearly a month. Up to this date, "no appreciable sleep had been enjoyed; the patient never knew that he had slumbered, even in broken rest, for one moment; he was reduced to skin and bone; and now began to feel great discomfort in lying on his right side. At the same time, intense pain occurred in the rectum after stools." These symptoms increased; and on the 15th of March, I detected a large abscess pointing in the rectum, about two inches from the anus, and mainly occupying the right side of the pelvis. This I immediately evacuated, with instant relief; and "the patient, with the aid of morphia suppositories, was afterwards blest with the first genuine night's rest since the 30th of January." The abscess continued to discharge for about three weeks; and at the end of that time the presence of matter could no longer be detected in the stools. On March 31, the wounds had been closed for fourteen consecutive days, and accordingly it was deemed safe to pass the bougie. Nos. 8 and 9 were insinuated with great gentleness; but next day the perineum was again inflamed; abscess formed; and once more the urine was discharged in front by the opening in the scrotum. From this date, however, the patient gradually recovered. He left Edinburgh for the south on the 18th of May, with the perineum quite closed, and passing his urine in a very satisfactory way. In July he returned, to have a bougie passed; and No. 9 entered without difficulty or evil result. "The contraction of the urethra, however, was by no means permanently cured;" and accordingly, on the 9th of November, the regular use of bougies was commenced, with the view of securing final and full dilatation. At first, I passed No. 6 with difficulty, but No. 12 now enters without obstruction; and the patient himself having acquired the power

of occasional introduction, cure may be considered complete. He thus concludes his narrative:—"The patient feels satisfied that No. 12 would never have passed without the operation, and that the effect of the latter has been to render the urethra less sensative than formerly, and hence has enabled a cure to be perfected, which otherwise had been impracticable. At the same time, it remains to be seen whether periodical recurrence to mechanical treatment may not be necessary; and it may be doubted whether, at the risk evidently incurred, the operation should be hazarded, except in cases incapable of being otherwise treated."

I cannot conclude even this hurried communication, without expressing my deep regret that the discussion of this question of practical surgery has been embittered and deformed by acrimonious and personal attack.—I am, my dear Sir, very truly yours,

JAS. MILLER.

Dr Wm. Robertson.

The Editor of the "Monthly Journal" begs to assure Mr Miller, that the article by Mr Syme, which appears in this Number of the Journal, is substantially *identical* with that which was read at the March meeting of the Medico-Chirurgical Society; a short foot-note only being interpolated by the author at page 335; and at page 333, line 9 from foot, the words "not having given" being substituted for "having declined to give."

There is nothing unusual in printing communications which have been made to medical societies, apart from the discussions to which they lead, or before these have been made public. Of this practice, even had it not been frequently followed in this Journal, Mr Miller's own contribution to the "Lancet" of the 22d March, would be a sufficient illustration.

Further, as Mr Miller's own paper was not sent to Mr Syme—whom it greatly concerned, and who had not the advantage of hearing it read—before its publication in a journal in which, for good reasons, Mr Syme cannot reply, it seems unreasonable that Mr Miller should now complain that Mr Syme's case (which Mr Miller had heard read), was not sent to Mr M. till on the eve of publication. It is not surprising that some difference of opinion should exist among the Conductors of this Journal, as to the justice of inserting Mr M.'s reclamation in our present Number.

Mr Syme, to whom the Editor has thought it right to show Mr Miller's letter of the 24th, thinks it unnecessary to make any rejoinder at present. He holds himself responsible for the accuracy of the statements which he has already put forth, and which it is his intention still further to substantiate in our May Number.

March 26, 1851,—11 P.M.

Part First.

ORIGINAL COMMUNICATIONS.

ARTICLE I.—*On the Treatment of Stricture by External Incision.* By JAMES SYME, Esq., Professor of Clinical Surgery in the University of Edinburgh.

HAVING so recently had occasion to express my sentiments in regard to the treatment of strictures by external incision, I did not expect that it would soon be necessary for me to write again upon the subject, but now feel that this operation requires to be defended from the representations respecting it, which have been published in the "Lancet" by Mr Miller. Before proceeding to do this, however, I may shortly remind the reader of what has already passed between that gentleman and myself, relative to the matter in question. The billet of business for the meeting of the Medico-Chirurgical Society on the 19th of February last, announced a "Case of Perineal Section" by Mr Miller. Now, whether rightly or wrongly, this title has certainly, in all the late discussions, been understood to denote the operation which I proposed. It is also certain that, although there may have been allusion to other cases, the one by far most fully then detailed to the Society was one in which this operation had been performed; also, that the operation, in this case, was performed, not by Mr Miller, but by myself; and also, that the account of it—read without my sanction or knowledge—was written neither by him nor me, but by the patient, who did not belong to the medical profession. Having reason to believe, from what was told me by various members of the Society, that a very erroneous impression had been made by the reading of this communication, at which circumstances prevented me from being present, I applied to Mr Miller for access to the paper, and by him was referred to the patient, who allowed me to see it on condition that I understood it was committed to me "*confidentially, and neither for publication nor public critique.*" Being thus precluded from noticing the statement which had been made to the Society, I saw no way of counteracting it, except by giving one that might convey my own knowledge and belief, for which I should be held responsible, and did so in the paper which was published last month through the medium of this journal. Mr Miller says it con-

tains "groundless personal charges" against him, and expresses his regret, that the discussion of this question of practical surgery has "been embittered and deformed by acrimonious and personal attack." The reader may, and I trust will, judge for himself how far Mr Miller has been warranted in making such an accusation. He says, indeed, that my charges "are modified" from the original form in which they were produced at the Society, but with what truth will appear from the fact, that my paper, after being read, was put directly into the hands of the secretary, and by him committed to the printer without any alteration. The proof then came to the proprietors of the journal, and they can testify that no change took place, except in the addition of a foot-note, with the substitution of "not having given," instead of "having declined to give." To this extent, and no farther, was the statement modified.

In the "Lancet" of 23d March, Mr Miller contends—first, that a large proportion of strictures admit of remedy by simple dilatation; secondly, that all strictures are not permeable; and, thirdly, that the operation which I have proposed, though warranted under circumstances of extreme urgency, is not free from the risk of producing the most dangerous effects.

The unavoidable inference from the first of these positions, maintained not by any ill-informed writer at a distance, but by a surgeon of the same hospital in which I practise, and a colleague of the same school in which I teach, must be, that I undervalue the process of dilatation, and resort to the operation in cases that would yield to the bougie. Now, every member of the profession in Edinburgh must know, that I have always been a strenuous and consistent advocate of simple dilatation, and that from first to last up to the present time, I have constantly employed this as the proper means of relief, unless it appeared from experience that some special peculiarity of the case rendered division of the stricture necessary. In originally bringing the operation before the notice of the profession (1844), I thus expressed myself—"In conclusion, I beg it may be understood, that nothing can be further from my intention than to propose division of strictures by a cutting instrument as in general preferable to the treatment by bougies. It is strictly to those cases which are found to resist a careful trial of the latter method, that the operation should be limited."[1] It thus appears, that in so far as the principles taught by me are concerned, there was no occasion for Mr Miller's vindication of the bougie. But it may be supposed that there was, perhaps, some laxity in my practice which required restraint; and I will, therefore, afford the reader grounds to form a judgment as to this, by placing before him all the cases which I have subjected to operation in the Royal Infirmary during the present session. They amount to seven in number, and may be thus shortly sketched:—

[1] Edinburgh Monthly Journal, 1844. P. 823.

1. George Thom, æt. 45, from Upper Canada, came home on purpose to obtain relief from a stricture deemed impermeable, with great thickening and induration of perineum. Stricture dilated by bougies, but speedily contracted—with aggravation of all the symptoms.

2. James M'Millan, æt. 27, farmer, from Ireland, sent by one of the surgeons of the Belfast Hospital, as labouring under stricture irremediable under the ordinary means of treatment, all of which, and especially dilatation in every form, had been carefully tried.

3. Thomas Stevenson, æt. 46, mason from Thornhill, extremely tight stricture. Bougies of very small size tried in vain for two months.

4. John Dixon, æt. 35, labourer, from Lincoln. Sent as suffering from stricture incurable under the ordinary means of treatment, which had been assiduously employed in the county hospital at Lincoln.

5. Robert Lyell, æt. 45, railway porter, from Berwick-on-Tweed, sent as labouring under impermeable stricture,—phymosis,—fistula in perineo,—and scrotal tumour.

6. Daniel Macdonald, æt. 55, bootmaker, from Glasgow. Stricture repeatedly dilated by bougies, at different times, without relief. Division performed at his own desire, from the good effects witnessed by him in the hospital.

7. Stewart Niven, æt. 47, seaman, from Campbelton. Sent as labouring under incurable stricture—for which he had been under a great variety of treatment, and six times in the Glasgow Infirmary.

In my private practice, the cases subjected to operation have been, if possible, still more hopeless under any other sort of treatment, and on the whole, therefore, I feel entitled to say, that if Mr Miller's statements have led any one to suppose that I do not employ dilatation by bougies as the ordinary rule of practice in treating strictures, they have conveyed an impression equally groundless and unjustifiable.

The second position which Mr Miller has endeavoured to establish, is, that all strictures are not permeable, and here he has resorted to an argument which I approach with feelings of no very pleasant kind. He says (the italics are Mr Miller's) :—"But, without seeking for distant authorities and manifold examples, I would content myself with the following, derived from a source which, under the circumstances, must be held as quite unexceptionable—namely, from a paper on the Treatment of Stricture, communicated by Mr Syme to the *Monthly Journal of Medical Science*, in 1844. In Case 3 (p. 822), a tight stricture followed the kick of a horse on the perineum; and it is stated, 'I found a stricture of the urethra opposite the scrotum, so tight that it would not admit the smallest probe, and surrounded by a mass of thickening and indurated texture, &c. * * After trying various instruments, without success, I passed an exceedingly slender catgut bougie,' &c. This is *almost* an impermeable stricture; the next is *completely* so. In Case 2, stricture fol-

lowed gonorrhœa; and the urethra having been examined (p. 821), was 'found to be contracted a little more than two inches from the orifice, so tightly, that the smallest probe could not be passed through the stricture, which was distinctly recognised through the integuments by the induration surrounding it, and felt like a broad, hard ring.' Mr Syme, baffled with the bougie, 'resolved to divide the stricture by incision;' and '*as the guidance of a director was not available for this purpose*, I proceeded in the following manner,'—performing the old operation, in so far as there was no director to guide the knife, but deviating from it in using the knife subcutaneously, instead of by direct incision—a procedure, as already stated, inseparably connected with much risk from infiltration of urine. Here, then, is indisputable evidence of impermeable stricture having occurred in the practice of Mr Syme himself."

Now, I beg the reader to remark that Mr Miller has *transposed* the two cases here quoted from my paper, so as entirely to conceal the fact, that it was from failure in the first of them, by cutting without a director, that I was led to establish the principle of always employing one, and for this purpose treating every stricture as permeable, with whatever amount of care and attention might be requisite for guiding instruments through it. Previously to adopting this view, I had shared in the general persuasion that strictures were occasionally impermeable, and not only taught the propriety of employing the old operation in such circumstances, but actually performed it. The procedure, to which Mr Miller alludes as having been practised in one of the cases just mentioned, was not, as he says, the "old operation," but one altogether new and unprecedented in the records of surgery, which proved the step that led me to adopt my present system of treatment, and which I certainly did not expect would ever be so strangely represented as an argument against the very principle established upon it.

Ever since becoming satisfied that all strictures were permeable (1844), I have never met with one that was not so. I have refused no case presented to me, and have in vain requested the advocates of impermeability to test the soundness of my principle, by affording me an opportunity of treating strictures, deemed by them examples of this condition. Mr Miller suggests—"This may be one reason, among many others, why the bold challenge of Mr Syme to English surgeons has not been accepted. A stricture, impermeable in London, may, to the same, or even inferior hands, spontaneously become permeable elsewhere."

This explanation of travelling and change of air, rendering strictures more amenable to treatment in Edinburgh than London, suggests the question, whether or no Mr Miller thinks that a journey in the opposite direction, from Edinburgh to London, would have the same effect. Mr Miller says,—"Besides, a stricture which is at one time impermeable, may, by spontaneous change, irrespective of all treatment, become permeable. An example of this is afforded by

Mr Syme, when he says that a case of stricture which had been under the care of Mr Liston, in the North London Hospital, and had obstinately refused to yield to that surgeon's efforts to pass an instrument, afterwards came under his own care in the same charity; and that he succeeded in passing an instrument into the bladder. Putting the manual dexterity, tact, experience, and general accomplishment of Mr Liston and Mr Syme on the same level,—an arrangement to which I am sure the latter gentleman will not object,—it follows that the stricture, at one time impermeable, at another time had ceased to be so, independently of curative treatment."

Now, passing from any question as to the good taste and propriety of drawing such a parallel, I beg to remark, that there is one material point of difference between Mr Liston and myself, in so far as he believed that strictures might be impermeable, while I am fully persuaded that they never should be so. I claim no greater dexterity than that which belongs to my neighbours; but I contend, that upon the belief as to permeability and impermeability will very much depend whether the one or the other of these conditions is found upon examination. It certainly seems surprising that a principle so important in practice, if it be correct, and at all events so void of offence, should have been resented in so many quarters as a personal attack.

As to the case here mentioned by Mr Miller, in illustration of his argument, I have merely to say that it never occurred, has not been related, and consequently exists nowhere but in his own imagination.

In regard to the danger which Mr Miller anticipates from the principle of permeability being committed to "the younger members of the profession," I must express my surprise at such an objection. The extraction of a cataract, cutting for the stone, and placing ligatures upon arteries, are proceedings not free from danger when executed by the "younger members of the profession;" but on this account, are surely not to be banished from the practice of surgery. If force, pain, and bleeding, even in the slightest degree, are considered entirely unwarrantable in exploring the urethra, the smallest instrument employed for this purpose may be deemed perfectly safe in experienced hands. More than this I have never alleged.

In supporting the third position, which respects the danger of the operation, having had no experience of it himself, and having, indeed, never performed it even in a single instance, Mr Miller relates a case in which, at his request, I divided the stricture, since, to use the language of his paper, it had resisted the "subjugation" of "instrumentation," and afforded no prospect of relief in any other way. He says,—"But, on the whole, Mr Syme's operation seemed preferable. And, accordingly, this having been agreed to in consultation, it was performed by Mr Syme on the last day of January 1850. After the operation, suppression of urine took place, during twenty-four hours, along with unpleasant symptoms of *shock*. Fever set in, accompanied by pervigilium, and great general uneasiness. After forty-eight hours, the catheter was removed."

For my account of this case, I refer generally to the last Number of this Journal. But, with reference to the particular statement just quoted, I distinctly declare, and positively affirm, that up to four o'clock of the afternoon of the third day, when I removed the catheter, there was no lack of urine nor the slightest symptom of local or constitutional disturbance; that, having occasion to go to the country on the fourth day, I did not think it worth while to visit the patient either in the morning before my departure, or in the evening after my return; and that, when I saw him on the fifth day, he was in a condition that seemed to me in every way satisfactory. After this, I saw him with Mr Miller twice or thrice at considerable intervals, and again in the course of the summer, when he returned to let me ascertain, by passing a full-sized bougie, that all was right. In regard to Mr Miller's narrative, I may merely remark in the first place, that, as the patient resided at the distance of a few hundred yards, and as Mr Miller's duty brought him to the hospital daily at the time of my visit, it does seem strange that he should not have acquainted me with the alarming occurrences which he has described, when they were in progress, especially as the responsibility of the result still rested entirely upon myself; and secondly, that in all my experience of operations for stricture,—now amounting to upwards of forty,—I have never met with such consequences as Mr Miller has related, so that, granting his account to be correct, it would only constitute an exception from the general rule, and could not be of any value as a guide in practice.

In conclusion, I beg to direct the reader's attention to the following extract from Mr Miller's paper:—"He thus concludes his narrative: 'The patient feels satisfied that No. 12 would never have passed without the operation, and that the effect of the latter has been to render the urethra less sensitive than formerly, and hence has enabled a cure to be perfected, which otherwise had been impracticable. At the same time, it remains to be seen whether periodical recurrence to mechanical treatment may not be necessary; and it may be doubted whether, at the risk evidently incurred, the operation should be hazarded, except in cases incapable of being otherwise treated.'"

Although still maintaining that the operation is absolutely free from all ordinary sources of danger, I do not object to the practical opinion here expressed, since it is precisely what I have always maintained and inculcated. But I do object to a patient, and especially one not belonging to the medical profession, being permitted to express any opinion as to a principle of practice founded upon his own personal feelings. Were such evidence admitted into serious discussion, the science of medicine and surgery would speedily degenerate into idle gossip, and the careful observations of anxious experience, extended over a series of years, might come to be weighed against the hysterical ramblings of a disordered imagination.

ARTICLE II.—*Contributions towards the Study of Certain Phenomena, which have been recently denominated Experiments in Electro-Biology.* By ALEXANDER WOOD, M.D., Fellow and Member of Council of the Royal College of Physicians, &c.

(*Read before the Edinburgh Medico-Chirurgical Society, on 2d April* 1851.)

[NOTE.—Those who believe in the real existence of certain mesmeric phenomena, are divided into two classes. The first attempt to account for them, by supposing the existence of some imponderable or other mysterious agent by whose operation they are caused. The second content themselves by referring the matter to certain mental peculiarities,—and there they leave the matter, much as the child who, wearied with conjecturing how the motions of some ingenious toy are produced, throw it aside with the exclamation, "Oh, it is all done by machinery!" The author of the following paper, while professing to belong to the second class, and therefore disclaiming all novelty in his views as regards the conclusions to which he has come, has attempted a more minute analysis of the connection between the particular phenomena observed, and the particular mental states by which he believes them to be produced, than, as far as he knows, has elsewhere been tried.

The title of the paper sufficiently indicates its nature. It prefers no claim to be a complete disquisition on the whole subject, far less to account for every one of the very singular phenomena which, as has now been demonstrated, can be produced at will by one human being in another; it will have sufficiently fulfilled the end intended, if it is found to accomplish the humbler task of acting as a guide to farther inquiry, and, by attempting to show how many of the experiments can be accounted for, to lead to the discovery, in other hands, of some general laws which may eventually be found adequate to the solution of the whole.]

It would be a most interesting investigation to trace the progress of medical science, comparing it at each stage of its development with the peculiar characteristics which the corresponding period in the history of other sciences exhibited. Unfortunately, the materials for such a task are neither very numerous nor very accessible, but they are nevertheless sufficient to show that the ages of "dogmatism," "empiricism," &c., as they are styled in medicine, were due less to causes of an intrinsic character, than to peculiarities of popular belief affecting it in common with every other science.

The present age may surely be denominated one of strangely-blended scepticism and credulity. Strangely-blended, however, they appear only to those who have never observed that these two apparently extreme states of mind often co-exist; and that a sceptical spirit in regard to truths which rest on evidence or authority, is very commonly associated with a peculiar credulity in regard to supposed

facts or theories which all evidence and all authority would concur in disproving. While, then, there is no science which has not been more or less affected by this pernicious tendency, there are some which it evinces a peculiar disposition to haunt; and nowhere has it ever lingered with more deleterious effect than over that debatable land which separates the world of matter from the world of spirit. Call this "the night side of nature" if you please, and let each fantastic dreamer reckon it his or her peculiar privilege to people it with the airy visions of a disordered imagination, even here must stern philosophy claim obedience, and compel submission to her rigorous laws.

Ignorant as we are of the precise nature of that connection by which the spiritual and corporeal entities of man are linked together, it is not wonderful if the mysterious influences which they mutually exercise on one another, should offer many tempting subjects for theories, often of the most crude and baseless kind. It has been so in the ages that are past; it is so in the age in which we live; and will continue to be so till the end of time, unless before that the nature of the connection ceases to be a mystery at all.

Of these various theories, the one which has perhaps attracted the largest share of attention, is that which seems to have been first propounded by Anthony Mesmer, about the middle of the last century, and which, under various names, viz., Animal Magnetism, Mesmerism, and more lately, Electro-Biology, and in many diversified forms, has largely engaged public attention.

Many theories of mesmerism have been propounded, and some of these have been supported by much ingenious, as well as absurd reasoning; they all rest on, and derive strength from, certain phenomena which are exhibited to wondering and credulous spectators, by itinerant performers who feed on popular credulity.

But it will not do in the present age for physicians to stand with folded arms, and regard such proceedings with the sneer of scornful contempt. Some of the phenomena elicited are of an interesting and instructive kind, and while we refuse to admit their production by the agency to which their exhibitors ascribe them, it is a duty incumbent on us to endeavour to ascertain the laws by which their development is regulated. The thorough supporters of animal magnetism as a distinct science, demand our belief in two classes of phenomena, differing widely in the amount of credulity which each may claim.

First, Those which may be denominated analogous, and which have their analogues in many diseased states.

Second, Those which may be termed heterologous, such as the alleged manifestations of clairvoyance, the phenomena presented by which not only have no analogues in the body, healthy or diseased, but seem totally opposed to all the recognised laws by which the physical or psychical phenomena of the living body are regulated.

With regard to the second class, it is sufficient simply to assert, that, in the present state of the evidence regarding them, they are utterly unworthy of the consideration of a society such as this. Why

should we occupy our time in discussing the rationale of supposed facts of a marvellous and extraordinary character, until we have satisfied ourselves that the evidence for the existence of these supposed facts, is sufficient to outweigh their very great and serious improbability? It may be unhesitatingly affirmed, that every case of alleged clairvoyance, when examined, is found open to so many sources of fallacy, that it is more easy to suppose that some of these had escaped detection, than that the marvellous phenomena recorded actually did occur in the manner which their annalists have supposed. Indeed, many of the most surprising of these cases which for a time baffled the minute and careful watching of observers, have been eventually found deceptive.

In a little work published by Dr Forbes, entitled, "Illustrations of Modern Mesmerism from Personal Observation," we have the examination by that exact and honourable physician of four or five cases of clairvoyance, and a perusal of these will serve to show how little any, even the most apparently well-substantiated narration, can stand the test of real scientific investigation. An examination of the evidence adduced in it will be sufficient to secure a general acquiescence in the following passage extracted from the preface.

"No one conversant with these proceedings, as hitherto conducted, can deny that few, if any, of the greatest marvels recorded by the mesmerists, and promulgated as unquestionable facts, repose on more sound foundations than before trial seemed to support those which the investigations detailed in the following pages proved to be utterly baseless and false. As *all*, then, *may* be untrue, are we not authorised to demand a new course of inquiry, or a new series of evidences, before we are called upon to admit the truth of clairvoyance, and the other transcendental phenomena of mesmerism? Are we not justified, for the future, in refusing to receive from the mesmerists marvellous statements as truths and facts, unless it is, at the same time, proved to be *impossible* to explain or account for them on other ordinary, or what may be called natural, principles?"

Restricting this inquiry, for these reasons, to the first class exclusively, it is proposed to consider the phenomena which it includes under the following divisions:—

1st. Certain physiological and psychical laws will be laid down which seem to offer a probable explanation of the phenomena exhibited by patients in the mesmeric trance.

2d. An attempt will be made to refer the various mesmeric phenomena to these laws respectively.

3d. It will be shown that these views are in strict accordance with the recently received doctrines of the anatomy and physiology of the nervous system.

4th. It will be considered to what extent the power of producing at will these phenomena, and the manner of their production, can be explained on any known principles.

I.—WHAT ARE THE ADMITTED PHYSIOLOGICAL AND PSYCHICAL LAWS TO WHICH THE PHENOMENA EXHIBITED BY PATIENTS IN THE MESMERIC TRANCE CAN BE MOST SATISFACTORILY REFERRED?

A. Volition—Its power and its offices in the body.

The greater proportion of the outward actions of an intelligent being in its ordinary state, result from what we call volition—that spontaneous power which the will can exercise at pleasure. From the manner in which the various powers of the mind are ordained to act on one another, the will is usually regulated by the reason, and thus man is constituted a reasonable being. Thus Bacon says (*De Aug. Scien.* V. i,)—"The philosophy that regards the *intellect*, and that which regards the *will* of man, are, as it were, twins in their origin. For light in the understanding, and freedom in the will, arise and fall together."

Volition must be regarded as the product or expression of a purely mental change. Now we find in the human brain, a part apparently destined to be the bodily organ of such mental states.

The central or hemispheric ganglia are the bodily organs by which ideas are formed, by which reasoning processes are carried on, and by which the will exerts its power. It is of great importance to remark, that supreme as is the control of the will over the voluntary muscles in most cases, the power which it exercises is not direct. It determines that a certain action shall be performed, and the necessary muscles begin to act, but it does not command the particular muscle, but leaves their selection and combination to be effected by that apparatus under the control of which muscular power is directly placed. This function is evidently seated in the spinal cord, and just as impulses on the peripheral extremities of the nerves of sensation excite in it *automatically* motorial power, so do the impulses of the will, conveyed down to it by the fibrous strands connecting the hemispheres with the rest of the brain, produce *automatically* also, in one sense, an exertion of power.[1]

Dr Todd (*Cyc. of Anat. and Phys.*, art. *Phys. of Nervous System*) thus explains it:—

"The impulse of volition excited primarily in the brain acts at the same time upon the grey matter of the cord (its anterior horn), and through it on the anterior roots of the nerves implanted in it. This grey matter, in virtue of its association with the brain, becomes part and parcel of the organ of the will, and, therefore, as distinctly amenable to acts of the mind as that portion which is contained within the cranium. If we destroy the commissural connection with the brain through the pyramidal fibres, the spinal cord ceases to take part in mental nervous actions; or if that connection be only par-

[1] Dr Cairns, in his work on moral freedom, seems to have had a clearer perception of the bodily relations of volition than is usual with pure metaphysicians, and divides volition into simple and complex.

tially destroyed, that portion of the cord, which the injured fibres had associated with the brain, is no longer influenced by the mind. Again, if the seat of volition in the brain be diseased, the cord, or part of it, participates in the effects of the disease as far as regards voluntary actions." Thus it is seen,—

1*st*, That the operations of the will are usually regulated by the reason.

2*d*, That the will, though a purely mental affection, acts through bodily organs, and that the cerebral hemispheres are its corporeal seat.

3*d*, That the will can exercise no direct power over the muscles, but affects them secondarily through the spinal marrow, the ordinary prompter of muscular action, and thus in a certain sense automatically.

4*th*, That by destroying wholly or partially the commissural connection, subsisting between the hemispheric ganglia and the automatic apparatus, the power of the will over the muscles can to a corresponding extent be impaired; or if the seat of volition in the brain be affected, the same result will follow.

B. Volition—Its power and its offices in the mind.

The power of the will, however, is not limited to the direction of the motor nerves; it also influences the mental operations.

In the human mind, there is a law implanted by which the order and succession of our thoughts is regulated, one idea naturally suggesting another.

> "For when the different images of things,
> By chance combined, have struck the attentive soul
> With deeper impulse, or, connected long,
> Have drawn her frequent eye, howe'er distinct
> The external scenes, yet oft the ideas gain
> From that conjunction an eternal tie,
> And sympathy unbroken. Let the mind
> Recall one partner of the various league,—
> Immediate, lo! the firm confederates rise,—
> And each his former station straight resumes:
> One movement governs the consenting throng,
> And all at once with rosy pleasure shine,
> Or all are saddened with the glooms of care.
>
> (*Pleasures of Imagination*, Book iii.)

But, though the train of our thoughts depends immediately on the laws of association, it is much under the control of volition. We can stop the current at will by singling out one thought, and making it a special object of attention; we can also divert that current into a new channel. Farther, by study and by practice, we acquire a command over certain classes of our ideas, so that we can at pleasure call them before us.

The objects of our knowledge are derived from two sources, consciousness and perception; but ideas once received are not lost, but may arise spontaneously, or be summoned up by the powers of

memory, conception, and imagination. As the contrast which exists between perception and conception is of importance, in accounting for the mesmeric phenomena, it will be necessary briefly to advert to some points in their philosophy which more directly bear upon the subject.

By perception, we obtain ideas of external objects through the medium of the external senses. By conception, we form ideas of past objects of perception, or of sensations formerly felt, and which cannot then be made the subjects of perception. This power is well styled by Shakespeare (*Hamlet*, Act i., Sc. 4) the "mind's eye." By imagination we combine the parts of different conceptions together so as to form new wholes of our own creation (*Stewart's Elements*, ch. iii.).

Of course, the ideas received through the external senses, or the objects of our perception, are believed to exist, and justly, unless there exists some lesion of the organ of sense, depriving its informations of their wonted correctness. The ideas, which the objects of conception (or imagination) suggest to the mind, are also believed, but their impression is so momentary, and, if wrong, is so immediately corrected by the surrounding objects of perception, that in the ordinary state of our minds, they never produce that permanent conviction which influences our conduct (*Stewart, op. cit.*).

It is very different, however, when conceptions become the sole tenants of the mind. Then the belief which the illusions of imagination produce, is as strong as that ordinarily arising from the informations of the senses, so that we ascribe to them as separate and independent an existence as we usually do to the objects of perception.

The relation of this to volition is very simple. The power which it gives us over the train of our thoughts, enables us at will to recall or dismiss the image before us. We cannot do this in regard to the perception of an external object—hence we learn by experience to acknowledge its independent and permanent existence; we can do this in regard to the creations of fancy—hence we learn to consider them as illusory and vain. But let the influence of the will be suspended, and this test can no longer be supplied, and we come to attach the same notion of reality to the products of conception that we have been accustomed to do to the objects of perception.

But another test, by which we are in the habit of trying the ideas brought before us by this mental power, is that of comparing them with surrounding objects of perception; should the external senses, however, be closed, this other means of correction is withdrawn, and the mind has no choice, but to receive as real existences any strange or fantastic images which the fancy may present.

Hence the clear and defined impressions of our senses are adverse to the development of the imagination; hence the highest activity of that power is manifested, not in the broad light of day, but as the shades of evening creep on; at such times it is, that our fancy, uninstructed, or at least uncorrected, by the sober test of clear per-

ception, invests with its thousand forms of beauteous or terrific aspect, the surrounding objects whose shadowy outline has failed to produce a distinct impression on the mirror of the mind.

> "A thousand fantasies
> Begin to throng into the memory,
> Of calling shapes, and beckoning shadows dire;
> And airy tongues that syllable men's names
> On sands and shores, and desert wildernesses;
> These thoughts may startle well, but not astound."—*Milton.*

Under this head, then, it may be concluded,—

1*st*, That the will in the natural state has a power of regulating and controlling the current of our thoughts; and that these, when not so governed, fall under the regulation of the ordinary laws of mental association.

2*d*, That, independently of consciousness, the materials on which the mind is exercised are supplied by the faculties of perception and conception.

3*d*, That we learn by experience that the objects of perception have a real and independent existence.

4*th*, That we learn the unsubstantial and illusory nature of the conceptions: *first*, by finding that we can recall and dismiss them at will; *second*, by comparing and contrasting them with our perceptions of external objects.

5*th*, That when the power of volition over the mind is suspended, and the external senses, wholly or partially closed, we cannot apply either of these tests; and hence we come to regard the creations of imagination as equally real with the objects of perception.

C. Sources of Motion, other than a simple exercise of Volition.

I. *Emotion.*—The various motions are usually arranged in three classes—voluntary, emotional, and automatic. Of the first, mention has already been made; and it is of importance to observe, that in it the spinal cord seems to be the direct originator of the motion to which the volition acts as an excitor, and the efferent nerves as the channels of motive power.

The emotional actions are often exerted independently of the will, and sometimes in direct opposition to it. Two circumstances prevent us from allowing to them their full share of importance in the production of muscular motion. *First*, That, unless when very strongly developed, they are more usually displayed in one class of muscles only—those, namely, receiving their supply of nerves from the medulla oblongata, mesocephale, or crura cerebri. Thus the connection of the fifth nerve with the lachrymal gland seems to point to it as the conductor of that power which causes the tears to flow at the moment of grief. Sobbing, crying, and laughing, again, are all performed by the same muscles, and all these muscles are supplied by the portio dura, the glosso-pharyngeal, the fifth, the phrenic, spinal accessory, and other respiratory nerves. But these nerves are not the

exclusive channels by which emotional feelings can be conveyed, neither are the muscles which they supply the only ones which can exhibit emotional excitement. The thrill of horror or of joy, the involuntary shudder at the very idea of danger, involve a much wider range of command. The phrenzied gestures of violent rage, the mute dejection of abject despair, are the witnesses of its power. Call to mind the description of anger given by the old Roman moralist:—"Ut furentium certa indicia sunt; audax et minax vultus, tristis frons, torva facies, citatus gradus, inquietæ manus, color versus, crebro et vehementius acta suspiria; ita irascentium eadem signa sunt. Flagrant et micant oculi, multus ore toto rubor, exaestuante ab imis præcordiis sanguine! labia quatiuntur, horrent ac subriguntur capilli, spiritus coactus ac stridens; articulorum se ipsos torquentium sonus, gemitus mugitusque, et parum explanatis vocibus sermo præruptus, et complosæ sæpius manus, et pulsata humus pedibus et totum concitum corpus magnasque minas agens, fœda visu et horrenda facies depravantium se atque intumescentium. — (*Seneca de Irâ*, i., 1.)

The *second* circumstance which tends to prevent our being fully aware of the power of emotion over muscular action, is the fact of the propensities and passions frequently affecting the muscles through the will rather than directly. It is only in extreme cases that we recognise their full power—these extreme cases being of two classes, the one, those in which the vehemence of the emotion has overcome the will; the other, where the power of the will has been from any cause diminished, so that even comparatively weak emotions may act directly on the muscles.

The last class are sometimes interestingly illustrated in pathology. Thus Dr M. Hall records a case of hemiplegia, where the paralysed hand and arm were convulsed whenever the patient met an acquaintance (*Memoirs*, p. 102). Dr Magnus, of Berlin, had a patient in whom the muscles of the face were paralysed to volition, but she smiled, and even laughed, under the influence of mental emotion.—*Brit. and For. Med. Review*, iv., p. 500.

II. *Reflex Motions.*—These reflected motions are prompted by some sensitive impression made either on the organic or animal nerves. The healthy movements of this kind are not of much interest, as regards our present inquiry, but occasionally, as an effect of disease, phenomena are presented which may be found to have some bearing on the mesmeric state. Thus, under the head of "Case of Cerebral Reflex Action," Dr Laycock has published (*Lancet*, vol. ii., 1845, p. 364), a case communicated to him by the late Dr Cowan, of Reading, because "evincing very strikingly reflex, visual, and auditory phenomena."[1]

[1] When this paper was read before the Medico-Chirurgical Society, my friend, Professor Simpson, objected to this case being regarded as one of reflex action. It is, however, so considered by both Dr Cowan and Dr Laycock, and I think a

"The shadow of a bird crossing the window, though the blind and bed curtains are closed, the displacement of the smallest portion of the wick of a candle, the slightest changes in the firelight, induce a sudden jerking of the spinal muscles, extending to the arms and legs when violent, and this without the slightest mental emotion of any kind, beyond a consciousness of the movement. At times the vocal organs are implicated, and a slight cry, quite involuntary, takes place. At these periods she is usually equally susceptible of all noises, especially the least expected and least familiar. Movements in the next house, inaudible to others, the slightest rattle in the lock of the door, tearing a morsel of paper, and a thousand like sources of sound not to be catalogued, induce similar results to visual impressions. Tactile sensibility is also great at these periods, though not to an equal extent.

III. *Movements arising from Ideas in the Mind.*—This class merits our most attentive consideration. The physiological action is often seen. Let a ridiculous idea pass through our mind, instantly we smile, or even sometimes laugh outright, despite the opposition of the will. The idea of yawning will at once excite the act. The thought of a fearful or disgusting object will cause an involuntary shudder, and the recollection of the latter will often produce vomiting as readily as its actual presence.

It is recorded of the illustrious Boerhaave, that, riding in the heat of summer, he chanced to pass a place where the carcass of a horse was rotting in the sun; at the very moment, when distended by pent-up gases, it gave way, and such was the overpowering stench, that he fainted. So strong with him was the association of ideas, that ever after, on passing the same place, the mere recollection of what he had encountered, caused a repetition of the fainting. In hydrophobia, not only does the sight or contact of water produce spasms, but the very idea of it, whether obtained by the eye, the ear, or the memory, has the same effect.—(Vide case by Dr Marcet, *Lond. Med.-Chir. Trans.*, vol. i., p. 13; and case by Mr Thornhill, *Med. Gaz.*, vol. xvii., p. 220.)

IV. *Movements arising from Imitation.*—Its power is especially

---

consideration of the whole facts abundantly proves them to be correct. Dr Cowan expressly states:—"The sensorial impression, and the motion consequent upon it, appear irrespective of every painful sensation or mental emotion, and are only noticed by the patient in consequence of the resulting movement." The will did not produce them, emotion did not influence them; to what, then, are we to refer them, if not to reflex action? In an admirable article, to all appearance from the pen of Dr Carpenter, on the "Physiology of the Nervous System," in the "British and Foreign Medico-Chirurgical Review," No. ix., we find the following remarks, completely confirmatory of the view taken above, basing it on the data of comparative anatomy. "Interpreting the anatomy of vertebrated animals, then, by that of the articulated, we should view the spinal cord, the medulla oblongata, and the whole series of ganglionic centres connected with the nerves of sensation, and lying along the base of the skull, in man *as far forwards as the olfactive ganglia, as constituting the apparatus of automatic action.*"

strong in weak minds, and prompts to the performance of many actions independent of the will. "There is scarcely," says the author of the article "Imitation" in Rees' Cyclopædia, "an irregular action of any organ of the body, which has not been caught (to use a common phrase) in consequence of this tendency to imitation, by different individuals; thus *squinting*, *stammering*, *winking* with the eyes, and various unseemly habits, have been frequently acquired by associating with those to whom they were already habitual. Every physician knows the frequency of the propagation of hysteric and choreic affections, by imitation." These are the more common phenomena; but there are others, whose connection with this principle is not so readily recognised. Dr Symonds says, "Any set of muscles may acquire particular actions and assemblages of actions, by passive imitation only, and to such a degree, indeed, that desire (volition?) is often vainly employed in opposition to this principle. One person yawns, or sighs, or laughs, because another does so; a child or susceptible female, if frequently in company with a person who winks or stammers, or falters in his gait, will fall into similar habits, notwithstanding there may be a variety of inducements for attempting to avoid them."[1]

The well-known account of Boerhaave's cases at the orphan hospital at Haerlem, is illustrative, as are also those cases of epidemic convulsions, which, too often associated with religious excitement, have manifested themselves in various quarters of the world. Of this nature was the malady which appeared in 1796, in the Island of Anglesey, attacking *females* between the ages of ten and twenty-five, of which Dr Haygarth has left us an account. The Shetland epidemic, of which the minister of Unst contributed an interesting narrative (*Ed. Med. and Surg. Journal*, vol. iii.) or the Tenessee epidemic in 1803 (*Inaugural Essay on Chorea*, by Felix Robertson, Philadelphia)—as well as many others, reports of which have been handed down to us from remoter times.

There is one fact regarding imagination, which may perhaps explain the readiness with which the mesmeric state can be reproduced in one who has repeatedly been in it—the excessive or often-repeated imitation of mental emotions may in the end bring on similar ones. After Garrick had acted his Lear or Othello, he passed some hours in convulsions in bed.

V. *Influence of the Will of Another.*—This must chiefly affect us through the emotions, and therefore might have been considered under that head. The power of the mind of one man to sway that of another, and even of whole bodies of men, is of a complex character, and need not now be analysed. Those who thus acquire power to guide the understandings of others, display themselves a volition which overmasters all opposition. The instinctive propensities in them

[1] On the Relations between Mind and Muscle.—West of England Journal. Vol. ii. p. 169.

give place to the strong volitional power by which their every action is guided,—that power, however, acts under the direction of reason.

How willingly does the weak mind surrender itself to the guidance of the strong! or how instinctively does it render homage to that determination of character which claims universal obedience! How often does the physician recognise the mastery which this confers, when, at some moment of unforeseen and sudden danger, all around are yielding to the instinctive propensities, and acting by impulse rather than reason, his self-possession restores composure, and irresistibly compels the necessary obedience!

The poet, too, has taken advantage of this, and thus beautifully expresses it:—

> "Ac veluti magno in populo cum sæpe coorta est
> Seditio sævitque animis ignobile vulgus;
> Jamque faces et saxa volant, furor arma ministrat:
> Tum pietate gravem ac meritis si forte virum quem
> Conspexêre, silent, arrectisque auribus astant:
> Ille regit dictis animos, et pectora mulcet."—(*Virg. Æn. i.*)

To a similar principle, too, we believe the power of oratory may be ascribed. Its effect on the emotions is well known, and has often prompted to deeds which reason would have restrained; and which the judgment condemned.

"In the ancient republics," observes an acute and beautiful writer (Thomas), "eloquence made a part of the constitution. It was it which enacted and abolished laws, which ordered war, which caused armies to march, which led on the citizens to fields of battle, and consecrated their ashes when they perished in the combat. It was it which, in the tribune, kept watch against the tyrant, and brought from afar to the ears of the citizens the sound of the chains which were menacing them. In republics, eloquence was a sort of spectacle. Whole days were spent by the people in listening to their orators,—as if the necessity of feeling some emotion were an appetite of their very nature. The republican orator, therefore, was not a mere measurer of words, for the amusement of a circle or of a small society. He was a man to whom nature had given an inevitable empire. He was the defender of a nation,—its sovereign,—its master!"

Well might Milton speak of those whose resistless eloquence

> "Wielded at will that fierce democracy,
> Shook the arsenal, and fulmined over Greece
> To Macedon and Artaxerxes' throne!"

If, then, in all states of the mind, the influence of another acting with the concurrence of the mental associations, and chiefly through the influence of the emotions, has been found powerful in overcoming for a time the restraint of reason, can we wonder if, in persons prone to emotion, and in whom the will is already spell-bound, the same power should be adequate to compel obedience to its imperious mandates?

From the foregoing facts, it may be concluded, that the following sources of motion are found to exist in the body, independently of volition :—

1*st*, *Emotion.*—Often conjoined with volition, often independent of it, sometimes even opposed to it, always strongest the less powerful the volition.

2*d*, *Automatic Action.*—Usually confined to the muscles of respiration ; sometimes, in diseased states, involving others.

3*d*, *Movements from ideas in the Mind.*

4*th*, *Movements from Imitation.*—Accounting for the propagation of many hysterical and choreic affections, and often producing stammering, lisping, squinting.

5*th*, *Influence of the Will of Another.*—Controlling the weaker and less volitional minds of others.

II. WHAT ARE THE ACCREDITED MESMERIC PHENOMENA, AND HOW CAN THEY BE EXPLAINED ON THE PRINCIPLES LAID DOWN?

A. Summary of the phenomena observed.

"Among the instances supposed to be made good as facts during the late exhibitions," writes the clear and cautious Edinburgh correspondent of a London journal, "are the following" :—

"That the arm of a susceptible person being stretched out at right angles to the body, can be rendered cataleptic to the extent that it will remain in that outstretched state, in spite of the efforts of the owner to let it drop, till it please the mesmerist to dissolve the charm; that a susceptible person may be made to put one foot across the other, notwithstanding his efforts to keep it on the floor; and, *vice versâ*, that he may be deprived of the power to put the one foot across the other, if it be the mesmerist's desire that it should remain on the floor; or that the fists may be made to spin round each other against one's will, or be rendered powerless for that movement, at the desire of the operator. Again, that a susceptible person may be made to believe that plain water is sweet or bitter at the bidding of the mesmerist; that the arm, or any other part of the body, may be rendered insensible to the pain, for example, of pinching; or that the subject may be at once put fast asleep, so as to be incapable of being roused by violent shaking, or loud shouting, or ringing a dinner-bell in his ear; and, lastly, that he may be forced to laugh or weep, or may be restrained from laughing or weeping, as the operator thinks fit."—*London Med. Times*, vol. ii., *N. S.*, p. 129.

Such is a succinct summary of these wonderful manifestations, for the explanation of which philosophers have thought it necessary to "call spirits from the vasty deep;" although it appears that "these spirits" have not shewn a very decided inclination to obey the summons. Be it now our task to endeavour to see how far these phenomena are explicable on admitted principles;—how far what is mysterious in them can be solved by reference to laws known and recognised.

B. In the magnetic trance, the power of the will, over both body and mind, is either suppressed or greatly weakened.

To form an accurate view of the pathological state of somnam-

bulism, it must be traced through those gradations by which it is connected with health. Sleep and dreaming seem to form the connecting link. Let us examine,—1st, their causes; 2d, their conditions.

"The approach of sleep," says Stewart (*Elements*, p. 321), "is accelerated by every circumstance which diminishes or suspends the exercise of the mental powers, and is retarded by every thing which has a contrary tendency."

Its causes have been tabulated as follows:—

1. Fatigue, *i. e.* suspension of active power.
2. Absence of thought.
3. Stupefaction (through certain drugs).
4. Mechanical pressure on brain.
5. Monotonous sounds.
6. Yielding to reveries.

An examination of these, shows that certain of them act by diminishing the vital powers in the organs of sense and motion, and checking the manifestation of the will over both mind and body (1, 3). Others, again, without much lowering of power, impede the manifestation and activity of the will (2, 5, 6). They all have the effect of withdrawing the mind from its own thoughts, without at the same time supplying it with anything to engage its attention. Children, and those who are more occupied about external things than accustomed to mental exercise, fall asleep whenever the objects of perception cease to engage their attention. In the 4th, the connection between the sensorium commune and the mind is probably impeded by some physical change.

A consideration, then, of all these causes of sleep, leads irresistibly to the conclusion adopted by Stewart, "that the will loses its influence over all our powers both of mind and body, in consequence of some physical alteration in the system, which we shall never probably be able to explain."—(See, on this subject, *Stewart's Elements*, chap. v. *passim;* and *Feuchtersleben, Med. Psychology*, § 57, 58.)

In this state, agreeably to laws already laid down, dreaming takes place.

> "While o'er our limbs sleep's soft dominion spread,
> What tho' our soul fantastic measures trod
> O'er fairy fields, or mourned along the gloom
> Of pathless woods, or down the craggy steep
> Hurled headlong,—swam, with pain, the mantled pool,
> Or scaled the cliff, or danced on hollow winds
> With antic shapes, wild natives of the brain."—*Young.*

The mind, having no external objects to engage it, and no volition to control it, is entirely occupied with the pictorial world of fancy. The illusions of imagination are not dispelled by clearer and more lucid images; and hence the existence of realities corresponding to them is believed, and influences our actions. Just as in the body, during sleep, the vegetative powers go on unimpeded, while the volitional are weakened or suspended; so, in the mind, those powers which volition habitually controls cease to operate,

while those which can be exerted independently of the will continue in active exercise.

Carry the physiological state of dreaming into the pathological state of somnambulism, and you find, as usual in nature, no very abrupt distinction. No wide gulph separates at any time the limits of health from those of disease. The exercise of the external senses is usually either impeded or perverted. The images of fancy have become so strong, that, with volition weakened or suppressed, they now act as suggesters or exciters of the centre of motive power. The vitality of the cerebral nerves seems paralysed,—automatic activity is increased. These phenomena indicate, as Feuchtersleben has shown, "not a more exalted, but a more fettered, state of the mind, in which it is subject to the will of other men, to the sway of its own instinct, and of fancy."

Such we believe to be the real key to the solution of those appearances which have so delighted the foolish, and in some instances seemed even to mock proud philosophy itself. Let us now attempt to classify and explain them, agreeably to those principles which have been laid down.

C. Many of the phenomena are clearly traceable to the operations of fancy, the images of which are either developed spontaneously, in accordance with those laws which regulate the association of our ideas, or are suggested by the magnetiser,—the mind, in either case, being no longer under the control of volition.

First Class.—Perversions of the External Senses.—When the strong assertions of the operator cause the patient to believe that the same water is at one time sweet, at another bitter, it is evident that the only change is in the assertion, and therefore that it must be the cause of the phenomenon. In attempting to explain this, we must bear in mind, that the qualities of bodies are changes in the state of the mind which they affect. All sensation, therefore, resolves itself into a mental effect produced by a material cause; and by finding that the effect usually succeeds the operation of the cause, we come to associate them together. The feeling in the mind, then, is all of which we are conscious; and if it does not follow the application of the external body, there is no sensation. In somnambulism it does not. In the eye this is evident:—"A candle may be held so close to the eye, as to burn the eye-lashes and eye-brows without the slightest indication of sensation." (*Feuchtersleben.*) In the ear it does not:—"Even the report of fire-arms does not wake the dreamer." (*Ibid.*) In the smell it does not:—"Brimstone and phosphorous are said to have a pleasant scent to the somnambulist; often the sense of smell wholly fails, as in the case of the apothecary's assistant, who held to his nose the tincture of castor, and murmured to himself, as if in excuse, 'I have a cold;' or, as in the case of the man whose snuff-box was filled with coffee, which he took for tobacco." (*Ibid.*) And so in the case of taste. Of the two

parts necessary to produce sensation, one is wanting,—the external impression is made, but the mind is asleep, and does not follow it by the internal change. In this case no taste would be perceived; and, just as the dreamer may be unaffected by sound, although the undulations of the air reach his tympanum as usual, so may he remain unconscious of tastes. But at the fitting moment the operator suggests to the patient the idea of a taste,—sweet, sour, or bitter; this idea, following immediately the bodily act, takes the place of the internal change with which this is usually associated, and is thus mistaken for the ordinary consequent mental change, and appears to him to be the real taste.

In conversing with a victim of this delusion, he informed me, that while asserting before a large audience the sweetness and bitterness of the water, he was throughout satisfied that the strong affirmation of the magnetiser had caused him to believe what he was all along sensible was really not the case. In him, as was often seen in these experiments, the control of the will was not completely suspended, but only partially overcome, by the operation of a more potential agency.

Much of the accuracy of our taste depends on the exercise of volition, not over the mind alone, but over the bodily organ likewise.

The epicure, over his savoury dish, or while tasting the aroma of his wine, rolls about his tongue and smacks his lips by voluntary effort. Even in the waking state, and in the exercise of full mental powers, this sense is subject to various deceptions, of which many amusing examples are recorded. One may suffice, for which I am indebted to a friend, on whose veracity I can place the strongest reliance:—

> At a dinner-party given by a gentleman, himself a connoisseur, and at which, among others, a gentleman of acknowledged taste, often employed by the wine houses, was present, the host, after dinner, promised his guests a glass of "some superlative whisky."
>
> The dram went round, the mouths were duly twisted, the lips smacked in orthodox style, and the liquor pronounced to be "*first class.*" It may be imagined what surprise was depicted on the face of the host, and what must have been the mortification of these renowned judges, when it was discovered that a bottle of gin had been circulated by mistake for the whisky.

Not less amusing, apparently surprising, or readily explicable, are the apparent perversions of the sense of sight. Closed against real perception, the mind readily mistakes for the informations usually received through the eye the obscure ideas of fancy acting under suggestion. Thus, in the account of the Edinburgh experiments already quoted, it is stated—

> That an officer in Edinburgh was persuaded that a piano was a horse, which he intended to buy, for which purpose he went forward in a business-like way, and attempted to take up the feet of this supposed horse for examination.

Another case, also alluded to, I myself witnessed:—

> A young man was persuaded that a gaping audience was a murmuring stream, and a thorn stick a hazel fishing-rod. To this, in idea, he attached an imaginary

line, to which was suspended an imaginary hook, which he baited with an imaginary fly, and with inimitable gravity he proceeded to hook an imaginary fish, whose leaps he grotesquely followed over the stage. In him Johnson's definition of angling was scarcely realised; for though the fool existed in admirable perfection at one end of the apparatus, the fly was wanting at the other.

In these instances, no doubt whatever can be entertained, that an illusion was practised on the mind through the medium of the imagination, and that the manner in which this was done is readily explicable on the principles previously laid down regarding Perception and Conception.

Second Class.—Perversion of Common Sensibility.—One of the most remarkable effects of the magnetic sleep, is the complete insensibility to pain, which seems in certain cases to be produced, the patients bearing to be pinched, pricked, or even burnt, without any apparent suffering. Well authenticated instances are also recorded, of severe operations having been performed without pain while the patient was in this state; as, for example, an amputation of the leg by Mr Ward ("British and Foreign Medical Review," xix., 434), and another by Mr Toswill ("Zoist," October 1844), and a number of severe operations performed in the same way in India by Dr Esdaile ("Mesmerism in India"). This state of insensibility has its analogues in nervous disorders, and many cases accompanying hysteria have been recorded.

It may help to throw some light on this subject, if we remember that certain muscular actions seem to relieve pain, such as screaming out, a fact which Dr Darwin seeks to explain, by supposing that it carries off so much nervous energy, that it does not leave enough for common sensation.

In order that pain shall be experienced, two prerequisites are necessary:—

1st. That an impression should be made on the sensorium commune, or that part of the brain in which the sensory nerves are implanted; and,

2d. That that change should be recognised by the brain-proper, or, in the words of the report made to the French Academy of Sciences on the Memoire of M. Flourens, the "cerebral lobes are the sole receptacle where the sensations can be consummated and made perceptible to the animal."

If, then, we suppose that by some temporary cause the action of one or other of these is suspended, or the connection between them interrupted, it may help to account for the insensibility to pain which is displayed by many persons when in the magnetic sleep.

Third Class—Perversion of Voluntary Motion.—In certain cases, those operated upon appeared unable to assume or alter certain positions, or, in other words, to call into play certain muscles. Such experiments are equally explicable on the principles laid down. The power of volition being either weakened or suspended, the patient might believe that he put forth a volition to execute a

certain change, or might even feebly do so; but the firm assurance and determined aspect with which the operator asserted the impossibility, acting through the medium of his disordered fancy, produced the effect which his enchanter desired. This is surprising—not so much in the kind, as in the degree, of the effect produced. "The idea of our own strength," says Müller (*Physiology, by Baly*, vol. ii. p. 1398), "gives strength to our movements. A person who is confident of effecting anything by muscular efforts, will do it more easily than one not so confident in his own power. The idea that a change is certainly about to take place in the actions of the nervous system, may produce such a change in the nervous energy, that exertions hitherto impossible become possible. This is still more likely to be the case if the individual is at the time in a state of mental emotion." If an idea of the mind can, as is here stated, supply strength, there is surely no difficulty in the supposition that it may also abstract it. If, in the ordinary state of mind, with all the faculties in full play, the confidence of being able to effect anything by muscular exertion confers the power of doing so, there is surely no difficulty in admitting the possibility of the converse in the extraordinary state in which a patient is thrown while in the mesmeric trance. To the influence of the ideas on muscular motion, and to this alone, we believe that the phenomena of muscular rigidity, as well as muscular activity, are due.

One interesting experiment is sufficient to illustrate this.

At a private magnetic soirée, a celebrated operator, at the request of certain parties present, undertook to magnetise the arm of a young lady, whom he had previously affected in a similar way. She was seated on a chair in the centre of the room, the rest of the party stood in silent expectation round the observed of all observers. Opposite her was placed the operator: an expression of resolute determination was thrown into his countenance. His eyes were fixed on, and his hand pointed to, the arm. Not a pass, not a motion, was made. Slowly the muscles of the arm began to contract, the arm was gradually elevated, and eventually became rigid—so rigid, that an ordinary drawing-room chair being suspended over the wrist, no deflection was produced.

I ventured to express a doubt that this case proved too much for the maintainers of the doctrine of odyle or the magnetic fluid. How had it passed from the operator to the patient? No attempt was made to communicate it by the ordinary passes or wonted excitants. Of course, the ready explanation was, that the operator had willed it to pass from his eyes to the arm, and that, with that ready obedience so characteristic of all its movements in certain persons, it had complied.

Having cited this case, in illustration of rigid contraction of the muscles evidently produced by the influence of the imagination, I may also refer to the striking analogy which it offers to certain cases, arising from similar diseased states. "Jam autem in catalepticis vides dum e. g. elevamus brachium, id non delabi, sed tumere et durescere elevatores; manifeste enim Deltoidem turgere sentimus, et si relinquamus brachium non delabitur" (*Tract. Path. de Catalepsi, auctore C. Fabri*). Fernelius records the case of a student who was seized with catalepsy while at his desk, and who continued,

during the fit, with the pen in his hand, and in a fixed attitude of intense application. (*Ambians. Pathol.*, lib. v. chap. ii.) Sauvages remarks of the same disease, that if the patient happens to be seized while laughing or weeping, the laughing or weeping will continue throughout the entire attack. (*Pathol. Meth.*, p. i. class vi.) One attacked while in the act of drinking, continued to hold the cup to the lips, and apparently to continue drinking throughout the entire fit.

In the various anomalous cases of chorea recorded by authors, similar phenomena have been observed. Thus, in Magendie's "*Physiology*," two remarkable cases are preserved, one from M. Pudagnol, of a man who was seized with an irresistible tendency to move forwards, only stopping when exhausted; and another by M. Laurent of Versailles, of a girl who was irresistibly propelled backwards, and with some rapidity, being unable to avoid obstacles or hollows, and, in consequence, receiving falls and bruises in her course. M. Magendie also records a still more extraordinary case, justly entitling this disease to an appellation once given it, of "insanity of the muscles," in which the will entirely lost its control over them. "It is impossible," he says, "to paint by language the variety and oddity of his motions and positions. * * * * At certain times his motions would pass into the train of ordinary actions. Thus, without the least participation of his will, he was seen to rise and walk with rapidity, until he met with a solid body that opposed his passage. Sometimes he walked backwards with the same readiness, and was only stopped by a similar cause." Dr Watt of Glasgow has related, in the fifth volume of the "Medico-Chirurgical Transactions," the case of a girl from Ayrshire.

"After suffering from headach, vomiting, loss of speech and motion, for about a month, she was suddenly seized with a propensity to turn round on her feet like a top, with great velocity, always in one direction, and was gratified when the attendants assisted her in increasing the rapidity of her motions. After a time these motions ceased, and she would lay herself across the bed, and, turning round like a roller, move rapidly from one end to the other. The fits continued at first for about two hours; but they were gradually increased to six or seven hours daily. A month or six weeks later, the movements became of an entirely new kind. The patient now lay upon her back, and, by drawing her head and heels together, bent herself like a bow, and then allowing her head and heels to separate, her buttocks fell with considerable force upon the bed. She repeated these movements ten or twelve times in a minute, at first for six hours daily, and at last for fourteen."

Still more close is the resemblance presented to that disease called by some authors *malleatio*, and consisting principally of a striking of the knees with one or both hands like a hammer. Dr Andrew Crawford mentions a case of this kind which he saw in the Hampshire County Hospital, in which there was a constant and uniform striking of the palm of one hand upon the side of the thigh, to such a degree as speedily to wear out the part of the dress on which it fell.

In considering the cases above related, all of them can without any difficulty be classed under one or other of the heads to which

motion occurring independently of volition has been already referred; most of them would be ranged under the first or third. In some cases it would appear that choreic, and therefore other analogous affections, seem to arise from some obscure painful affection, or peripheral irritation, which might on further inquiry, enlarge the number of cases in the third section.

C. Certain of the phenomena produced during the magnetic trance are clearly referrible to the faculty of imitation which, always strong in weak minds, becomes especially manifested when the volitional control is withdrawn.—Fully to understand this, we must distinguish between that imitation exercised at will, as in the mimic, and that which involuntarily or even against every effort of the will, copies the peculiarities with which it is brought into contact. It is the latter to which reference is here made:—Cabanus gives an account of a man in whom the tendency to imitate was so strongly marked and active from disease, that he experienced insupportable suffering when he was hindered from yielding to his impulses.

One case will sufficiently illustrate its effect in mesmerism:—

At an evening party, at which a celebrated magnetiser was present, a lady long resisted his efforts to make her stammer. She was a bad subject for him,—not very susceptible of the influence. Again and again, he issued his commands,—again and again with considerable effort, she succeeded in disobeying; at length he thundered out his mandate, and immediately thereafter, putting his face close to hers, and protruding his chin, he made his own jaw to imitate the convulsive twitches of the stammerer. The effect was irresistible.

Laughing and weeping may either be produced in the same way, or arise from emotional excitement.

D. Attempt to explain the power manifested of controlling certain of the involuntary muscles.—Hitherto our attention has been confined to the muscles usually employed in voluntary motion, but the believers in odyllic influence appeal to the power they possess over organs not usually controlled by the will, as a proof of their theory, which cannot be gainsaid. "The heart and lungs can be affected by us" say they. "Power over the voluntary muscles may be explained away on your theory, but this you cannot account for." But we can. Dr Holland, in his Medical Notes and Reflections, observes,—"There is cause to believe the action of the heart to be quickened or otherwise disturbed, by the mere centering of consciousness upon it without any emotion or anxiety. The same may be said of the parts concerned in respiration. If this act be expressly made the subject of consciousness, it will be felt to undergo some change, generally to be retarded at first and afterwards quickened." Such facts readily explain a class of cases of which the following may be quoted as a fair example:—

The patient, a young gentleman who had been previously magnetised, was

seated opposite the operator; we were all told *in his hearing* that the motion of the heart was to be controlled; the magnetist stood with his hand and fingers stretched out and directed to the heart; I remained to watch the pulse. A few passes were made, and suddenly the pulse rose; a few others, and it fell.

E. Attempt to explain the cases in which one person supposes himself, while in the magnetic trance, to be another, and acts the character which he has assumed.—Of all the vagaries which a person in the trance can be made to play, this perhaps is the most amusing, but certainly it is not, as has been sometimes alleged, the most surprising. It might have been referred to a former head,—that, namely, in which the illusions of the senses were considered; but such has been the prominence assigned to it, and such the marvellous character attached to it, that it has been thought more advisable to make it the subject of separate consideration. We must go back to first principles, and determine on what the aberration depends. I dismiss all metaphysical subtilties at the outset; I will not discuss the question of intuitive truths at all; in reference to them, Reid and the Scottish school of philosophers have laid themselves open to the attack of Priestley and the Materialists, and the latter have not been slow to take advantage of the opportunity afforded them.

All this, however, need not occupy our attention, for it is evident that just as we derive our knowledge of the existence of the external world by our external senses, and just as the information which they afford is strengthened and corrected by experience, so we come to understand our own existence by internal sense, the ideas which it supplies being rendered more definite and distinct by our experience of the differences between our sentient body and the various objects by which it is surrounded.

And again, just as our external senses, or ideas supposed to arise from them, but in reality referrible to another source, may deceive us into the belief of the existence of some nonentity, or of the transformation of one object of perception into another,—whether of a horse into a piano, or a walking-stick into a fishing-rod,—so our internal sense may in a similar manner deceive us into the belief of changes and transformations in those states of the existence of which it is its office to inform us.

But further, metaphysicians have failed altogether to advert to the amount of evidence which the operation of our will affords us as to our self-consciousness. The condition of our body we change by an exercise of volition; things without us alter independently of this. We will an action, it is performed; that performance is attended with sensations, and these we learn to distinguish from the sensations caused by external objects. This means of distinction we lose, then, when volition is suspended; and just as formerly it was shown how its arrest suspended the tests we had for the truthful representations of one class of ideas, so now it will be seen how the same change deprives us of all means of ascertaining the fidelity of this other large and important class. Let us next glance at its de-

rangements in disease, from the slighter and more transient to those analogous to what are observed in the mesmeric trance.

A certain corporeal sensation seems to exist in all organised beings, whether they are possessed of a nervous system or not. Each individual having a ganglionic system of nerves, possesses farther a common feeling by which the state of the body is revealed to the mind.

When this common feeling is altered, it deceives the patient into the idea of the existence of corporeal conditions which are not real. Examples of this are frequent at magnetic exhibitions. A patient will at one moment be too hot, and throw off his coat; the next he shivers with cold, and buttons himself up; or a snow storm descends, and he seeks to protect himself from the cutting blast. In all these states it is the bodily feeling which is deceived, much in the same way as the external senses were in other experiments already referred to.

Passing on to a higher degree of the same derangement, we encounter in the diseases of hysteria (or hypochondriasis) the resemblance of many of the phenomena to those of the magnetic trance, which have already been considered. The will, and mental powers generally, are for the time weakened, the bodily sensations acquire an undue preponderance, and eventually the mind becomes its subject, —its slave. "Non raro aegrum ab hoc sensu, et medicum ab aegro falli, cum aeger ex sensu communi hausisse hinc inde affirmat, quod imaginatio et præcepta etiam opinio illi suggesit." (*Hartmann, Pathology.*)

Still further ascending to the higher degrees of disordered manifestations originally springing from the same root, we arrive at delusions. These are admitted by the most esteemed writers on these subjects to own two sources of origin,—1st. The alterations of the individual feeling which we have just been considering, and the delusions arising from them, as displayed in the severer cases of hypochondriasis and hysteria. 2d. The hallucinations of sense which, as has been already shewn, exist in persons in the magnetic state. "Un homme qui a la conviction intime d'une sensation actuellement perçue, alors que nul objet extérieur propre à exciter cette sensation n'est à portée de ses sens, est dans un état d'hallucination: c'est un visionaire."—*Esquirol des Mal. Men. Tom.* i. p. 159.

Instances of delusion of this kind have not been uncommon in the history of man. In Arcadia, as Sprengel informs us, the shepherds often believed themselves to be changed into wolves, and imitated the actions and howls of these animals.

According to Hippocrates, an epidemic insanity at one time prevailed among the Scythians, and those seized with it imagined they were changed into women, and this idea was so strong as to influence their actions. In modern times innumerable instances of a similar kind have occurred. "Bishop Warburton, in a note to one of his works, speaks of a person who thought he was converted into a goose pie; and Dr Arnold saw a man who fancied himself in the family

way. Pope describes, in his 'Rape of the Lock,' many of these fancies." (*Elliotson*). "All lunatic asylums," says an esteemed author already quoted, "are full of princes and princesses, kings, popes, seers, and even sons of God. The unfortunate poet Wetzel arranged his writings before him, and inscribed on the back of the binding '*Opera Dei Wetzelii.*' The more fortunate Professor Titel rejoiced as being a Roman emperor, possessed of an immense dominion."

Such are the analogues of this condition in disease, and they, like the others already considered, ought to suggest a caution to medical men how they trifle with patients, by producing phenomena so closely allied to the most serious and afflictive maladies. Such fancies, if encouraged and frequently repeated, may pass into fixed delusion, and then what distinction can be drawn between them and insanity?

It may be objected, however, that in all the instances given the delusion *was* already fixed. This is true; but it is not difficult to find similar ones of a less permanent character.

In intoxication, which resembles sleep and the magnetic trance in its power to suspend the control of the will, while it differs from them in the fact that it stimulates the brain, the fancy excited by the emotions, passions, and desires, propels through the mind a crowd of images which it cannot control. The similarity of many of the phenomena of intoxication to those of the magnetic trance need not be pointed out. And surely it is scarcely necessary to show that, in that state, self-deceptions, similar to those we have now been considering, frequently occur. On this Dr Geddes's celebrated Scottish song of the "Wee Wifukie" is founded.[1]

---

[1] We suppose, in the composition of this song, the author studied the facts rather than the philosophy of his subject. It is, therefore, all the more interesting to observe how the several stages of the mental state of the "Wee Wifukie" illustrate some of the views on which an explanation of the magnetic phenomena has been rested.

First we have the cause:—

> "There was a wee bit wifukie, was comin' frae the fair,
> Had got a wee bit drappukie, that bred her meikle care.
> It gaed about the wifie's heart, and she began to spew,—
> Oh! quo' the wee wifukie, I wish I binna fou."

The packman laddie having clipped her hair, she comes to the conclusion on which the song turns—

> "And when the wifie wakened, her head was like a bee,—
> Oh! quo' the wee wifukie, this is nae me."

The deceptions of fancy, however, are not complete, and she tries to test their reality,—1st, By memory; 2d, By appealing to the objects of sense.

> "I met with kindly company, and birl'd my bawbee!
> And still, if this be Bessukie, three placks remain wi' me;
> But I will look the pursie nooks, see gin the cunyie be,—
> There's neither purse nor plack about me!—this is nae me."

Still, however, not thoroughly persuaded, she seeks familiar objects to assure herself, but in vain, till, dropping asleep, the fumes of the liquor are dispelled, and

III. THE EXPLANATIONS OF THE PHENOMENA WHICH HAVE BEEN NOW OFFERED, ARE IN DIRECT ACCORDANCE WITH THE RECEIVED DOCTRINES OF THE ANATOMY AND PHYSIOLOGY OF THE NERVOUS SYSTEM.

The power of suspending certain functions of the nervous system, has, if not proved, been at least assumed; while, at the same time, it has been farther contended that the others still continued to act independently of this change. This necessarily presupposes the existence of separate and independent organs for each of these; for whatever be the change by which the magnetic trance is produced, the agent who produces it must act on the bodily organs of him who is its subject, and the effects which are observed are probably due to some inappreciable change in the nervous centres. It is not for us as pathologists to determine what this change is; the physiologist must pave the way for such an inquiry, by showing what those changes are by which ordinary nervous action is produced. If the one is undiscoverable or undiscovered, so must the other remain, for many errors would be avoided in pathology, did we sufficiently admit, that a knowledge of healthy function must precede all inquiry into its conditions under disease. The following facts, then, seem to be admitted in regard to those points in the anatomy and physiology of the nervous system which concern our present inquiry.

A. The brain-proper—the bodily seat of intellectual actions—has no power of directly effecting muscular movement.[1]

The grey tract in the spinal cord proves, that it is more than a mere conductor of nervous power from the brain,—while the demonstration by Mr Grainger of some of the roots of the spinal nerves terminating in the grey substance, seems to indicate that they are the conductors of a power generated in the cord itself. Besides, were the brain constructed, so as *directly* to operate on the muscles through the nerves, these nerves would have to go as *directly* to the brain, in which case the white matter of the cord would gradually increase in bulk from below upwards, which is not the case, as has been conclusively established by the observations of Volkmann. These facts seem conclusively to establish the doctrine, that the

---

Bessukie is herself again, to the great delight of Johnnie, who had been at the parson's, seeking advice in his perplexity.

"Now Johnnie he cam' hame again, and oh! but he was fain
To see his little Bessukie come to hersel' again.
He got her sitting on a stool, wi' Tibbuck on her knee.
Oh! come awa, Johnnie, come awa to me;
For I've got a nap wi' Tibbuckie, and this is now me."

[1] For fuller details, and for proof of the doctrines here advanced, I beg at once to refer to the articles from which I have borrowed them:—"Physiology of Nervous System," by Dr Todd, in *Cyclopædia of Anatomy and Physiology*; a review of "Noble on the Brain," in the *Brit. and For. Med. Rev.*, October 1846; a review of Dr Todd, in the *Brit. and For. Med.-Chir. Rev.*, January 1850.

spinal cord is the chief centre for the roots of the spinal nerves, and consequently, that in those acts called reflex, it operates directly on the muscles through the efferent nerves proceeding from it; and, also, that in muscular acts originating in, or controlled by the mind, the cerebrum does not itself act directly on the muscles, but merely exercises an influence on the spinal cord, from which their nerves directly proceed. Thus all muscular action proceeds directly from the spinal cord, and that portion of the nervous centre can be stimulated to act by any other portion which can originate such a power, and which has a medium of communication with the cord.

B. That the corpora striata and adjacent parts are the centre of volition; that this nervous centre is independent of, although connected with, the central hemispheres, and that it has ample communications with the spinal cord.

According to the experiments of Longet and Lafargue, animals remain immoveable after removal of the corpora striata; and pathology teaches us, that even a very slight lesion of that part of the nervous centres is invariably followed by paralysis. In short, the connection of these bodies with voluntary motion has been admitted by all physiologists, from the time of Sir C. Bell.

Then, supposing the corpora striata to be the seat of volitional impulse, that, as we have already shown, is closely connected with the judgment,—the material organ of which is in the cerebral convolutions, and, therefore, ample means of communication should exist between them. It is so in fact, extensive radiations of fibrous matter pass from the hemispheres to the corpora striata. But, again, it is no less evident that they must have the means of transmitting their volitional impulses to the spinal cord as the centre of motion, and so we find that the pyramidal bodies connect them with the grey matter of the cord.

C. That there also exists a centre of sensation, independent like the others, but at the same time closely connected with the centre of volition, because sensation is a frequent exciter of motion, and voluntary motion is always, in health, attended with sensation. This centre of sensation must also have ample communications with the hemispheres that the sensations received by it may be made to act on the mind.

These conditions are all found to exist in the thalami optici. All the nerves of sense are connected more or less directly with them, or with the olivary columns, which are continuations of them. These, conjointly, appear to form a ganglion for the sensations communicated by the nerves of touch, and therefore destined for the reception of sensitive impressions. The close association between them and the proper optic ganglia, is explained by the close association between the senses of sight and touch, which is apparent both from the manner in which our ideas of external objects are communicated to us, and also from the joint operation of these senses in

directing muscular movements. The first of these farther presupposes a close connection with the central hemispheres, which is found to exist through the inferior crus cerebri; the second with the centre of volition, which is maintained by the passage of innumerable fibres from one to the other; while, through the olivary columns, it is connected with the sensitive nerves which join the spinal cord, chiefly in the posterior horn of the grey matter.

D. That there farther exists a separate centre of the nervous system for the emotions, and that it too must have connections both with the seat of the intellect, and with the special originator of muscular motion.

This is to be found in the mesocephale, of which Dr Todd remarks—"Its influence extends upwards to the central convolutions —backwards to the cerebellum—downwards to all the nerves of sensation and motion. Through its connection with the posterior horns of the spinal grey matter, it can excite the sensitive as well as the motor nerves of the trunk. Hence it is not to be wondered at, that a highly disturbed state of this centre is capable of deranging all the sensitive as well as the motor phenomena of the body, and even the intellect. Hence we may explain the extraordinary movements in hydrophobia and general chorea, in both of which diseases this part of the nervous centre is doubtless affected."

E. That another centre, also independent, and at the same time closely connected with the others, is found to be the instrument of mental operations — perception—memory—judgment—imagination, &c., and that the cerebral convolutions form this centre.

This is abundantly evident from experiments, and still more from pathological anatomy. It is necessary, to perfect soundness of action, that these centres should be able to communicate with each other, and to co-operate in action, "that," to quote again from Dr Todd, "the centre of intellectual action should be capable of exciting, or of being excited by, the centres of volition and sensation. This connection and mutual influence is effected through the innumerable fibres which pass from the one to the other."

F. That, in addition to the four centres already described, there is also a centre for the co-ordination of muscular movement, and one for respiration and deglutition, but these have no sufficient connection with the present subject to require attention.

G. General conclusions in regard to the anatomical relations of this inquiry.

After reviewing these anatomical facts, we are, I think, entitled to come to the following conclusions respecting them:—

1*st*, That the great centre of muscular motion is the spinal cord, and that its functions are unimpaired during the magnetic trance.

2*d*, That the great centre of volition in the brain is found to be

in the corpora striata, and that their functions are impaired during the magnetic trance.

3*d*, That the centre of sensation lies in the optic thalamio and olivary columns, and that their functions are more or less impaired in the magnetic trance.

4*th*, That the close anatomical connection which subsists between the corpora striata and optic thalami explains the frequency with which lesions of the one affect the other, and also explains their joint affection in the mesmeric trance.

5*th*, That the true optic ganglia are very closely connected with the thalami optici, and hence the latter may be affected through them, and in their turn affect the functions of the corpora striata.

6*th*, That the central convolutions are the great centres of intellectual action. That their functions are not impaired, but that, owing to a temporary suspension of some of those sources of information which they ordinarily depend on, they may suggest erroneous ideas.

7*th*, That the upper and posterior part of the mesocephale is the seat of emotion, and that it can either act on the muscles by volitional impulse, or directly, as the case may be.

8*th*, That the central hemispheres can excite the cord to motion, independently of the other centres; hence ideas in the mind may produce motion, independently of volition.

IV. To what extent can the power of producing these phenomena at will, and the manner of their production, be explained on any known principles?

Hitherto, in tracing the phenomena of mesmerism, we have been guided at almost every step by analogical resemblances which have been found to exist between it and certain pathological conditions; and in this last branch of the inquiry such guides are by no means wanting, although the information they supply is less definite and precise than that which was procured in reference to some of the other points discussed. It has been shown that, in the magnetic state, the manifestations of volition are repressed, and therefore its power of directing the mind and body suspended; and that in it, therefore, the energy of some of the nervous centres is as it were paralysed, while that of others predominates. This cannot be regarded in any other light than that of a pathological state; and hence, in accordance with the usual arrangements of pathology, its production may be considered under the heads of predisponent and exciting causes respectively.

A. Predisponent Causes.—Every one is not found to be susceptible of the mesmeric state. There are certain individuals more prone than others to hysteria, chorea, and the other allied affections. The causes of the predisposition seem somewhat similar in all, viz.,—a delicate and susceptible state of the nervous system, a predominance of feeling over judgment, an absence of commanding energy of the

will. On the other hand, the mesmerisers, like all who in any way are destined to control and direct the minds of others, are usually persons of strong powers of volition, and whose wills exercise a supreme control over their own instinctive tendencies.

B. Exciting Causes.—It has already been shown that, in order to give free scope to the play of the imagination, the external world must be to a great extent excluded; for, otherwise, the perceptions of the true will neutralise the conceptions of the false, on which the subsequent delusions hinge. Of all the senses, those of sight and touch are the ones most evidently concerned, both in the origination of mental perceptions and in the direction of muscular movements. The sense of sight, then, must first be suppressed. To accomplish this end, the eye is directed with fixed and steady gaze, and, if possible, in a fatiguing position, to some object which is not of a kind to occupy the mind. In these circumstances the organ of sight is fatigued, it ceases to be able to distinguish, and demands that repose which is the consequence of all continued action; for tension must ever be succeeded by relaxation. Many who try the fixed gaze cannot succeed in maintaining it; their eyes seek relief by glancing to surrounding objects,—and in them the process fails. It is not difficult to understand the reason of this. The monotonous attention soon fatigues, while change and variety dispel the lassitude which would otherwise be induced. But this fixed gaze affects the mind as well as the body. It is sufficiently engrossing to withdraw it from other perceptions, and thus to suspend its activity, and throw it into reverie. We endeavour to induce common sleep by ceasing, as much as possible, to exercise our different powers; and the magnetic state, which in some respects would appear to be a deeper manifestation of the same phenomena, is probably produced in a similar way. Dr Braid informs us (*Neurypnology*, p. 58) that the magnetic sleep cannot be produced unless by the eye, except in persons who have been previously magnetised. May not this be in great part explained by the connection which, in the anatomical section of this paper, was shewn to exist between the thalami optici and true optic ganglia. I think one great difference between natural sleep and that produced in magnetism is this,—In the former, *all* the nervous centres are tired out, and *all* repose; in the latter, those of sensation and volition are chiefly affected,—and they being lulled to rest, the others retain their wonted activity. "It is long since it was observed that inordinate attention to one subject caused dreaming rather than sleep."—(*Braid, op. cit.*, p. 46.) Cullen also states, "If the mind is attached to a single sensation, it is brought very nearly to the state of the total absence of impressions,—or, in other words, to the state most closely bordering on sleep. Remove those stimuli which keep it employed, and sleep ensues at any time." In farther confirmation of this, it may be remarked that distraction of mind prevents it. By the kindness of Professor

Simpson, I am enabled to give the following case, which I saw along with him:—

A lady, after repeated experiments, became so susceptible, that she would go to sleep when desired. A painful operation was to be performed on her, and it was determined to try whether, during the magnetic trance, she could bear it without suffering. Every attempt, however, to produce insensibility failed, although sleep followed the efforts. The patient herself explained the cause:—"She was sure the mesmerism would not be a sufficient protection from pain; and to prove this to her attendants, and induce them to give her chloroform, she resisted as much as possible the tendency to sleep."

When induced by the fixed gaze, sleep seems to arise partly from the connection between the optic ganglia and the corpora striata through the thalami optici, and partly by the exhaustion of volition consequent on the powerful effort required to maintain the gaze. In this last way, of course, the corpora striata are directly affected. But again, recalling the intimate connection which has been shown to exist between the centre of volition and the centre of sensation,—so intimate, that disease can scarcely affect the one without impairing the functions of the other, we are led to see how the various medicines, such as opium and chloroform, which, when applied locally to a nerve of sensation, have the power of deadening it to impression, should, when carried to the central seat of sensation by the blood, have the power of suspending its operations, and, by virtue of its connection with the centre of volition, arrest the control of the will over mind and body. This explanation, so far as I know, has not been before proposed, and it seems to promise some assistance in clearing up some of the mysteries of the magnetic sleep. Opium, for example, blunts sensation, shuts up the avenues of perception, and excites the liveliest mental conceptions, which sometimes prompt even extravagant actions. Sir Humphrey Davy thus describes his sensations on inhaling nitrous oxide gas:—"By degrees, as the pleasurable sensation increased, I lost all connection with external things; trains of vivid visible images rapidly passed through my mind. I existed in a world of newly-connected and newly-modified ideas." But whatever be the manner in which the process is conducted, its effect is manifestly to deprive all those powers whose exercise depends on the will, of the influence by which the mind usually controls them. The precise manner in which this is accomplished, it may be less easy to explain. Certain it is, that the attempt to explain these phenomena by the supposition of a fluid allied to that developed by magnetism or electricity, and passing from the body of the operator to that of his victim, is the most improbable of all. In not a single experiment which I have witnessed, could the results have been satisfactorily explained on this hypothesis, and in most of them there were conditions observed, sufficient to neutralise the passage of any such fluid, if obedient to those laws by which the transmission of similar imponderables is usually regulated.

In conclusion it may be remarked, that it were well that both operators and patients were more distinctly aware that the pheno-

mena induced are those of disease. The predisponent causes are the same as those of chorea, epilepsy, and insanity. The phenomena accompanying the manifestations are, when otherwise induced, known to be most dangerous means of producing such diseases. Excessive development of the imagination, or undue subjugation of that judgment which should direct the will, can never be mentally beneficial, but will in all probability be the reverse. Intense exercise of the fancy has a powerful effect on the organs both of mind and body. "The visionary," says Feuchtersleben, "is a candidate for the lunatic asylum."

To the pathologist, such exhibitions are undoubtedly interesting, as specimens of disease, and he will no more be inclined to turn from them with disgust, than from the morbid preparations so loathsome to the unprofessional eye. The philanthropist, however, will regard them in other light. He will consider them as additions to the catalogue of human woes—morbid states induced to form the ground of philosophical investigation,—or, worse still, to wile away a passing hour. A calm consideration of them will serve to show that the physician is scarcely warranted to induce them, unless by their means he can banish a more powerful disease, or alleviate severe suffering; and were the non-professional public made aware of their real nature, they would surely come to the conclusion of the poet Spenser :—

> "Of all God's works which do this world adorne,
> There is noone more fair and excellent
> Than is man's body, bothe for powere and forme,
> Whiles it is kept in sober government;
> But none that is more foule and indecent,
> Distempered through misrule and passions bace,
> It grows a monster, free from all restraint,
> Doth lose its dignity and native grace."

ARTICLE III.—*Case of Wound of the Internal Carotid Artery, inflicted while operating from within the Mouth, for the removal of a foreign body deep in the Neck—Ligature of the Internal Carotid—Hemorrhage controlled by a Single Ligature, and Cure.* Treated by WILLIAM KEITH, M.D., M.R.C.S. London, Surgeon to the Royal Infirmary, Aberdeen, and Lecturer on Clinical Surgery there.

ELIZA KENNEDY, aged 25, native of Ireland, employed as a power-loom weaver, was admitted into the hospital on the 20th of December 1850, at 11 a.m., complaining of pain in the left side of the pharynx, and difficulty of deglutition, arising from the presence of a large pin which she had accidentally swallowed the day before, about nine in the morning; at least she infers it must have been at that time, as she remembers, faintly, having put a pin in her mouth, and of being at the time seized with a fit of coughing, when, she supposes, the pin had slipped down. It was, however, towards three o'clock in the

afternoon, before she was made sensibly aware of what had happened, by a pricking sensation on attempting to swallow. She came at that hour to the Infirmary—was seen, and examined by one of the resident staff, Dr Rattray, who felt the head of the pin below and behind the left tonsil, but entirely covered by the lining membrane of the pharynx,—the pin extending downwards and forward toward the upper edge of the thyroid cartilage, in which its point seemed lodged; because, although the shaft of the pin could be felt easily, at the same moment, from without and from within, yet no ordinary manifestation could in any way alter its position.

No operative interference was practised at that time. She was invited to return on the following morning at the hour of visit, and came accordingly, as stated above.

On examination with a finger down the throat, and another opposing it externally beneath the angle of the lower-jaw on the left side, a pin, at least one-and-a-half inches long, was distinctly felt, in the exact position which it had occupied on the preceding day. No part of the pin was bare, it was entirely hid, and covered by the mucous membrane of the pharynx.

Several efforts were made to dislodge the point of the pin from its insertion in the thyroid cartilage, but in vain; and efforts were made to let the head present in the pharynx, by scraping through the membrane covering it. These having also failed, the head of the pin was made to project inwards, and a nitch made with a probe-pointed scissors. This exposed the pin, but the opening in the membrane was fully half an inch below the head of the pin, so that, though a hold was got, the head was still entangled, and the scissors were again applied to liberate it. On the snip being made, by the blades closing on the pinhead, blood flowed in mouthfuls so large as to lead to the instant conviction, that the pinhead had been lodged sufficiently deep to lift the internal carotid, and present it to the scissor blades when the clip was made.

The thumb of the operator, applied instantly on the common carotid, effectually controlled the hemorrhage, and as no doubt existed as to the precise vessel injured, so, not a moment was lost in procuring the instruments necessary for taking up and tying the internal carotid. A scalpel, dissecting forceps, and aneurism needle, were got in two minutes. She was laid down on the floor of the physician's room, in which she was at the time sitting; her head was supported by a pillow; her face turned towards the right shoulder. An assistant having taken command of the common trunk, an incision was made from below the ear, in front of the mastoid process, along the ramus of the lower jaw to below its angle. No hemorrhage occurred to obscure the dissection, and the vessel was speedily exposed; a simple aneurism needle, armed with a double ligature, was passed under it with more ease than was anticipated, when the depth at which the vessel lay was first viewed in connection with the rather limited extent of the external wound.

The needle withdrawn, the least possible disturbance having been given to the artery, one ply of silk was drawn home on the vessel at the upper point of its exposure, and secured. The pressure being now wholly removed from the common carotid, no hemorrhage whatever recurred. The spare thread was, therefore, immediately withdrawn, both ends of the ligature preserved, and brought out near to, but above, the centre of the wound. The external wound was closed by three stitches, and the patient carried up-stairs to bed. She had lost perhaps six or eight ounces of blood in the first half minute—given forth in large gushes—between each of three or four sentences which she found breath to utter only singly after emptying her throat and mouth,—"Arrah, you've done for me now,"—"What will become of my child?" &c. &c. These, accompanied by the usual national gesticulations with head and arms, for a few seconds, forbade the possibility of commanding the bleeding vessel; but as life hung on a slender thread at that moment, and seconds were of vital importance, one peremptory order to sit still as she valued her life, and an assurance on honour that her child should be well cared for, calmed her at once, and the vessel was under complete control. Her quiescence, and the cessation of the bleeding, were no doubt much aided by decided faintness, which then came on, and which continued until some time after the operation. Her pulse, however, continued distinct the whole time. The facial and temporal arteries were found to pulsate firmly after the whole was concluded. Heat was applied to her lower limbs, and moderate re-action established by 4 A.M. The pulse firm, good, and slow. One sputum of coagulated blood came up within an hour after she was put to bed.

6 P.M.—Is comfortably warm. Pulse 60, firm. No other sputum has come up, and no other sign seen of further hemorrhage. The two house-surgeons, Messrs Thomson and Pirie, take watch about in her closet.

9 P.M.—One other small clot of blood has come up, but there is no appearance of any fresh hemorrhage.

21st December, 7 A.M.—Has slept pretty well through the night. Three colourless sputa of clean mucus have come up in the course of the night, without even a trace of blood. Breasts turgid with milk. She still suckles a child fifteen months old. Has, notwithstanding, menstruated regularly for four months past, and chanced to be in the middle of that process at the time of the operation.

10 A.M.—Going on most favourably. Allowed her a small basin of boiled "sowens," which she swallowed with considerable facility,—not, however, without pain. Pulse 60. Skin cool. Feels the pin at times; and has occasional, but transient, spasm of the glottis.

4 P.M.—Sleeping soundly.

8 P.M.—Seems much refreshed. Gave her "sowens" until she was quite satisfied. Sputum to-day comes up easily, clear and co-

lourless. The menstrual discharge, after having suddenly ceased, has come on again.

22d December, 10 A.M.—Slept well. Sputa moderate in quantity, and colourless. A small skin-looking clot came up in the morning, but has been lost. Pulse 60, soft. Can now swallow her "sowens" and drink fluids with more ease. Bowels not open since admission; an enema to be given.

1 P.M.—Enema has brought off a copious stool of scybala and digested blood.

23rd December.—Slept well. Pulse, skin, and tongue natural. The same soft diet continued.

24th December.—As at last report. Bowels opened in the evening by an enema.

25th December.—Removed the lowest two stitches in the external wound, and supported the edges by two fresh strips of isinglass plaster. The wound seems firmly united.

3 P.M.—Gave her some "sowens." She mentioned before beginning that she had a feeling as if she would not be able to swallow them, as she thought the pin had shifted its place, and was like to choke her. The first spoonful excited slight coughing, but the morsel went down, and along with it the pin. She felt a sensation as if it pricked her slightly some way down the œsophagus, and then all sense of its presence was lost. She drank her milk and swallowed her "sowens" with perfect freedom, expressing her entire conviction that the pin was gone.

9 P.M.—Throat feels raw. Complains of debility. Bowels moved by enema.

26th.—Egg allowed to breakfast and soup to dinner. Bowels moved naturally.

27th.—Removed the remaining stitch and all the plasters from the wound in the neck, and found it firmly united from end to end. One drop only of moisture, and that a surface ooze, seen at the inferior angle of the wound. No moisture or redness around the ligature, which comes out at and behind the angle of the jaw.

28th December.—Bowels three times opened, stools mucous and faintly tinged with blood. Pricking pains felt occasionally in right side of belly.

29th December.—A large consistent stool followed the administration of a dose of castor oil. Is quite easy since. The pin searched for, but not found.

30th December.—Going on well. Easy in every respect. Appetite good; bowels easily regulated; wound whole; requiring no dressing. The ligature retains its hold of the vessel.

In consequence of this fact, though up and going about, she was detained in the hospital.

10th February 1851.—The report is,—On trying the ligature, it came away easily—this being the fifty-second day since the vessel was secured.

17th February.—She was dismissed cured.

She continues well up to this date,—8th March 1851. The case is brought forward, chiefly as a duty to the profession,—to put on record a casualty, and that of a very grave nature, occurring in the midst of what might be deemed a trifling act of surgical manipulation—to show how it must be met should the like happen again, and to show that, in some circumstances, a single ligature may suffice to stop hemorrhage from the internal carotid artery when wounded.

While recording this case—all successful as it has been—I should ill discharge my self-imposed task, if I did not accompany it by the expression of my strongest conviction, that such a case must be exceptional. Early impressed by the writings of John, and the lectures of Sir Charles, Bell, with the absolute necessity of tying both ends of a wounded artery, it has ever been with me a downlaid *axiom;* and my wonder has often been excited by the alleged backwardness of surgeons to see and act upon it.

Deeply grateful ought the profession to be to the talented Guthrie, for so ably, graphically, convincingly, pressing on us this vital rule. It is surely admitted and adopted more generally than he imagines, for reason and common sense should settle the whole question, with any possessed of a tyro's knowledge of anatomy and physiology. In the case recorded, a double ligature was passed under the vessel, the object in view being, if the hemorrhage should not cease after one ligature was tied, to enable the operator to adopt Mr Guthrie's recent suggestion — (*See Lancet,* Vol. ii., 1850, page 145)—to bisect the lower jaw, and then lifting it out of the way, get free access to the wounded carotid, at a point sufficiently high to command the hemorrhage supplied by collateral circulation through the head.

The hemorrhage, however, ceased after the application of the one ligature, and therefore, according to all sound rules, further interference was at the time uncalled for. The pin was allowed also to remain undisturbed. No subsequent necessity ever arose to warrant the slightest interference; but it is fair to state, that the case was watched day and night for some time, with all the instruments in the ward adjoining, in order to complete, with as little delay as possible, an operation that was felt by the operator, for many days, to be, as it were, only half finished. To account, in the circumstances, for the complete arrest of the hemorrhage by one ligature, it may be supposed that the nitch (∧) shape of the wound was favourable to its closing; that the pin-head, lying in or near the wound, would help to entangle and support a clot when once formed; and, lastly, the wound inflicted by the scissors was so far down, that it was thought by the operator that he reached up very close to the wounded part, and that his ligature may have been near enough to give a solid rest to any clot so formed.

257, Union Street, Aberdeen,
8th April 1851.

ARTICLE IV.—*On the Pathological States of the Lung connected with Bronchitis and Bronchial Obstruction.* By W. T. GAIRDNER, M.D., Pathologist to the Royal Infirmary of Edinburgh.—(*Continued from Vol. XI., p.* 246.)

PART II.—SECONDARY OR PERMANENT DISORGANISATIONS DEPENDING ON BRONCHITIS.

IN two former papers on the affections of the pulmonary texture resulting from bronchitis (Monthly Journal, August and September 1850), I have treated, at considerable length, of the collapse of the air-vesicles connected with obstructed tubes,—a lesion of which, as I have endeavoured to show, the true pathological significance has been much neglected or misapprehended, even by authors who have correctly enough described some of the morbid appearances. The more chronic and permanent lesions connected with long-continued bronchitis have now to come under consideration; and although these affections cannot be said, for the most part, to have engaged less than a due share of the attention of pathological writers since they were made familiar by the descriptions of Laennec, yet it will appear from the sequel, that the links which bind them together in a series cannot be understood without reference to those primary results of bronchitis to which I have already adverted. In describing these affections, therefore, I shall avoid as much as possible dwelling on descriptions already familiar to well-informed medical readers, confining myself, for the most part, to the pathological considerations which flow more or less directly from what has already been advanced in the first part of this memoir.

Results of Bronchitic Collapse of the Lung—Curability of Pulmonary Collapse.—There can be little doubt that the condition of collapse of the air vesicles, from obstruction of the bronchi, may, when recent, be completely removed, and give place to the normal condition of the pulmonary texture. The imitation of this result, by forcibly inflating the lung affected after its removal from the body, not only proves the absence of any organic change, but shows conclusively that it is in many instances only a sufficiently strong inspiratory force which is required to disperse the obstructing mucus, and make a free passage for the entrance of air into the lung. The collapsed lung, however, is placed under a most serious disadvantage, as compared with that which contains air, in freeing the bronchi from causes of obstruction. The latter can render available the expiratory force, and this, when aided by the impulsive effort of coughing, is by far the most efficient agent in displacing and removing the bronchial plug, which it carries outwards, and expels altogether from the system. The completely collapsed lung, on the other hand, can only bring into play the inspiratory force, a weaker power acting against

a greater resistance, and able, at the best, only to disperse inwards, never directly to remove, the cause of obstruction. Under these circumstances, it might seem probable, that a lung when once brought into a state of complete collapse, should be mechanically incapable of perfect recovery; and even in minor degrees of the affection, it would appear that the tendency *inwards*, or towards the lung, of mucus, and all other obstructions, must greatly increase in proportion as the residual quantity of air in the vesicles diminishes. Again, where the obstruction is much localised, as in limited and lobular collapse, the mechanical forces tending to remove it, whether inspiratory or expiratory, will at all times be apt to diffuse themselves over the surrounding normal or comparatively unobstructed lobules, so that the removal of the bronchial plug under such circumstances is not easily understood, if we take into account only the forces we have hitherto been considering.

Does collapse of the lung, then, necessarily, or in the majority of instances, lead to organic and permanent change of structure? This doctrine I should be very slow to admit. A consideration of the cases in which bronchitis occurs, and is even repeated frequently in the same individual, without appreciable permanent change, while we know, from post-mortem appearances, and understand on mechanical grounds, that accumulation even to a very moderate extent in the bronchi is often sufficient to cause a certain amount of the lesion, will, I think, even in the absence of more detailed clinical experience, constitute a strong case for believing in the existence of some more active remedial and conservative mechanism in such cases than that of the inspiratory and expiratory forces. Such a view is altogether borne out by the observations of writers on the bronchitis and lobular pneumonia of children, which, though often a grave, and even a fatal affection, is never regarded as being, in favourable cases, less capable of perfect resolution than any other form of pulmonary condensation. The remarks of most other practical writers are so much governed by pathological views, differing from those we have been considering, that it is quite impossible to eliminate the information they may contain as to the results of pulmonary collapse. I have already remarked that a great number of the varieties of so-called catarrhal and typhoid pneumonia are undoubtedly affections of this kind, sometimes combined with genuine pneumonia, and sometimes uncomplicated; and in particular, that the hypostatic pneumonia of M. Piorry, and the "peripneumonie des agonisans" of Laennec, are generally instances of the diffused form of pulmonary collapse. The former observer has devoted so much attention to the observation of this particular form of disease, as to render his remarks valuable, even though probably modified by an erroneous pathology. He says:—"The first stage of hypostatic pneumonia, while the blood is still contained within the vessels (*i.e.*, while no exudation has occurred into the air cells,) is *very susceptible of cure;* indeed, it may be

said that this state exists *in a large number of invalids* (chez beaucoup de malades), and *is dissipated during convalescence.*"[1] The other stages, especially the third and fourth, in which solid or purulent deposits exist in the air-cells, are, according to M. Piorry, more grave, and even generally incurable; but these again are obviously not uncomplicated instances of pulmonary collapse. I shall not at present enter more fully into the discussion of M. Piorry's views on this subject, than to remark, that the passage above quoted is in harmony with all that has been already submitted to the reader, more particularly with the observations alluded to in the last volume of this Journal, pp. 234–6. Indeed, I cannot entertain a doubt, judging from the facts there mentioned, that a more extensive and exact clinical experience bearing on this subject will demonstrate the extreme frequency, and in many cases the easy and rapid removal, of a certain degree of pulmonary collapse, which may or may not have led to serious symptoms during life.

De-obstruent Function of the Bronchial Tubes.—Supposing these views correct, the mechanism by which the viscid mucus is expelled to such an extent as to permit the return of air into the occluded vesicles, demands further consideration. We have seen that the expiratory forces are, under such circumstances, thrown out of action; while those of inspiration, even if strong enough to displace the obstructing plug, can never permanently remove it. Under these circumstances, it seems to be reasonable to ascribe to the bronchi themselves an active part in the expulsion of obstructive mucus, by means of the slow contraction of those circular fibres, the muscular character of which was demonstrated by Reisseisen, and whose physiological properties have been fully illustrated by the experiments of Dr Williams and others. It is now well established, that these fibres have no such vital endowments as would enable them to co-operate with the movements of respiration, influenced as these are by the will. "The contractility," says Dr Williams (of the bronchi), "resembles that of the intestines or of the arteries more than that of voluntary muscles or of the œsophagus, the contractions and relaxations being gradual and not sudden. They are, however, much less tardy than those of the arteries."[2] This kind of contractility is precisely that which empties the arteries of their blood after death, and which, in all probability, contributes to the passage of calculi along the ureters or gall-ducts. It is also more or less analogous to the peristaltic contraction of the intestines, or of the elongated tubular uterus of many of the lower animals, by which the solid or fluid contents of these viscera are gradually expelled towards their outlet. The experiments referred to appear to prove that the contractility of the air tubes is readily excited, not only by galvanism applied externally, but by mechanical and chemical stimuli in con-

[1] Piorry—Pathologie Iatrique, vol. iv., p. 411.
[2] Williams—Diseases of the Chest, 4th Edition, p. 330.

tact with their mucous membrane. It is easy, therefore, to understand, that the bronchi (or at least those which have not cartilaginous walls) may have a most important power of dislodging obstructions altogether independent of the forces of respiration. When these forces are in active operation indeed, the tonic or slow contraction will be in abeyance, or very slightly manifested, as the air-tubes will then be dilated to their full extent at each inspiration and expiration. But, according as the admission of air to any part of the lung becomes less from obstruction, the detrusive action of the bronchial muscles will increase, being thus called into effective action precisely at the period when most required. Perhaps, also, the slighter contractions of these muscles may be in almost constant operation in the normal condition, to aid, by a kind of peristaltic movement, the outward passage of the physiological secretion. This secretion, comparatively small in quantity as it is, would almost necessarily tend to accumulate in the air-tubes (seeing that no efforts of coughing or forced expiration are made for its removal); and this would take place, particularly in the smaller bronchi, which we know to be especially subject to mechanical obstruction, and in which the ciliated epithelium, so abundant in the cartilaginous bronchi and trachea, gradually gives way to transition forms, not constantly furnished with cilia.

It may not be easy to adduce direct proof of the theory here proposed, as to the function of the bronchial muscles in health and disease; but as *no* theory upon this subject has yet been found consistent with our present physiological knowledge, and as the above speculation appears in all essential points to correspond with what is already known of the action of these muscles, it may be worth while to give it consideration, were it merely to rescue us from the unphilosophical predicament of supposing the circular fibres of the bronchi to be endowed with contractility solely for the purpose of producing the asthmatic paroxysm. That these fibres are probably perfectly passive, as regards the respiratory act, is now generally admitted (contrary to the ancient opinion) by physiologists; and under these circumstances the theory of their de-obstruent action, even in health, but more especially in the diseased states of the pulmonary texture above described, appears to supply a gap in the chain both of physiological and of pathological phenomena.

The ordinary form of the paroxysm of spasmodic asthma, of the humoral kind, is full of instruction, when considered by the light of the preceding views. Notwithstanding the extremely doubtful and difficult pathology of this disease, it seems impossible to avoid referring its most obvious symptoms to some kind of irregular action of the muscular apparatus of the air-tubes. The copious expectoration, again, with which the attack concludes, and by which it is immediately relieved, appears to indicate that undue accumulation of mucus has been taking place; while the absence, in some instances, of all considerable catarrhal symptoms appears to demonstrate that

this accumulation is directly connected with the spasmodic derangement which produces the paroxysm. The connection of these two phenomena it is by no means difficult to understand, according to the principles already laid down; in fact, if the removal outwards of the pulmonary mucus depends, in the normal state, upon the regular peristaltic contraction of the bronchial muscular fibres, it is obvious that accumulation must accompany the derangement of that action, just as constipation is the invariable concomitant of the analogous derangement of colic or ileus. In both cases the paroxysm ceases when the normal action is restored; and in general there is in both a copious discharge of the previously retained excretions.

Asthmatic persons are often subject to a slight habitual wheezing in some part of the chest, and also to an occasional cough, with or without slight expectoration, but with no other symptom of catarrh. These symptoms have been described to me as occurring on exertion in the open air after prolonged rest; they are accompanied with slight dyspnœa, and this, together with the rest of the symptoms, ceases when the exertion is continued long enough to produce some degree of re-action. These phenomena are unquestionably the minor degree of the paroxysm; they are probably caused by the same irregular action of the bronchial muscles as causes the latter, but do not reach the climax, because the nervous centres are awake to the first approaches of disorder, and the excitement and quickened respiration consequent on exertion produce the cure. The aggravated asthmatic paroxysm always occurs during sleep, when the energy of the nervous system is at the lowest, and the comparatively quiescent condition of the respiratory function favours the accumulation of mucus. It seems probable that the asthmatic paroxysm is attended with more or less of pulmonary collapse, the consequence of the accumulation in the bronchi; but I have not had an opportunity of direct observation on this point. It is certain, however, that this accumulation must seriously contribute to the production of the most distressing symptoms of the paroxysm. The spontaneous cure in the real paroxysm, as in the minor attack, or threatening of asthma, above referred to, usually takes place when the nervous centres have been thoroughly roused, and the whole system brought into a state of reaction by the exertion consequent on the dyspnœa.

An interesting fact, in connection with asthma and other spasmodic respiratory diseases, is the frequent occurrence of vomiting during the paroxysms,—a fact which points to the probable dependence of all these affections on some morbid condition in the communication of which the pneumogastric nerve and the medulla oblongata are the principal parts concerned. A phenomenon exactly the converse of that just alluded to, is the profuse and immediate expectoration in cases of obstructive bronchitis after the administration of an emetic. Now it is interesting to observe, in relation to both these facts, and their bearing on the subject we have been considering, that Volkmann has apparently succeeded in demonstrating the influence of

stimuli applied to the trunk of the vagus nerve upon the muscular contraction of the bronchi,—a point left open to doubt, both by the experiments of Williams and by the subsequent ones of Longet.[1] The expeditious and complete relief afforded by an emetic in cases in which there has been extreme difficulty of expectoration, is one of the most striking phenomena connected with bronchitis; and one of which, I believe, no sufficient explanation has yet been afforded. It appears, however, to be completely in harmony with the theory I have advanced in the preceding pages.

Another fact tending still further to illustrate this view, is found in the experiments of Reid, Longet, Schiff, and others,[2] on the effects of section of the pneumogastric trunk, or of its visceral branches, on the lungs and bronchi. All experiments concur in proving that these operations are followed by a very large accumulation of frothy mucus in the bronchi. Changes in the lungs have also been observed, which seem to be of the nature of congestion and collapse, but are imperfectly described. M. Longet has also found emphysema of the lungs, the relations of which to pulmonary collapse will in these cases hereafter be considered; and there can be little doubt that we have all the phenomena of bronchial obstruction and collapse following the division of the nerve which, according to the views above proposed, is the chief regulator or excitor of the bronchial de-obstruent function.

To sum up the results of this discussion, as respects bronchitis, I would recapitulate the following points, which, if not established, seem to be at least rendered highly probable. *Firstly*, That pulmonary collapse from bronchitis, when recent and uncomplicated, appears to be susceptible of cure, on removal of the bronchial obstructions. *Secondly*, That this is usually effected, not so much by the agency of respiration, as by the muscular contractions of the obstructed bronchi themselves. *Thirdly*, That the derangement or paralysis of this de-obstruent function becomes a cause of bronchial accumulation even in the normal state of the mucous membrane, and, *a fortiori*, in cases of bronchitis. *Fourthly*, That the de-obstruent function of the bronchial tubes may be impaired by various causes acting on the pneumogastric nerve, either directly or through the nervous centres. And *Fifthly*, that it may be stimulated by remedies or other agents acting in a similar manner.

The application of these principles to pathology might be almost indefinitely expanded, if it were desirable at the present stage of the

---

[1] Volkmann introduces into the trachea of a decapitated animal a tube having its outer end tapering and perforated by a rather small opening. This being placed opposite a flame, he isolates and galvanises the vagus nerve, when, at every application of the stimulus, the flame is observed to be blown aside.—*Wagner's Handwörterbuch der Physiologie*, vol. ii., p. 586.

[2] Edin. Med. and Surg. Journal, April 1839; or Reid's Anatomical and Physiological Commentaries. Monthly Retrospect, 1849, p. 3. Longet—Systeme Nerveux.

inquiry to indulge in much farther speculation. But enough has probably been brought before the reader to show that the symptoms, causes, and cure of bronchitis and other allied affections, even when not resulting in demonstrable or organic disease, are illustrated by a clear conception of the phenomena of pulmonary collapse and its attendant conditions.

I now proceed to the consideration of some permanent disorganisations, for the most part well known to anatomists.

Permanent Lesions of the Air-Vesicles and Bronchi depending on Bronchitis.—Those which are enumerated by authors, and which I have myself seen, may be described as being principally of two kinds,—viz., permanent obliteration or constriction of these parts, and permanent abnormal dilatation of them.

The obliteration of the air-vesicles, leading to *atrophy of the lung*, has been viewed as a consequence of bronchitis by Dr Stokes, and in this respect he stands almost alone among the authors who have treated of this disease. In his work on "Diseases of the Chest," he says:—"Atrophy of the lung has been recognised in a variety of diseases, such as tubercle, pneumonia, cancer, and pleurisy; but its direct connection with bronchitis has not been sufficiently examined."—P. 203. Dr Stokes argues for the possibility of this connection, on the ground, that "it is easy to see that when the air-tubes are obstructed, the cells to which they lead will diminish in volume." He gives, however, no experience of his own upon the subject; and in speaking of emphysema, he adopts Laennec's opinion of its origin, which ascribes an entirely opposite result to obstruction of the air-tubes. The views of Dr Stokes upon this subject cannot, therefore, be considered as very clear or consistent, though I believe the merit of suggesting that atrophy of the lung may depend on bronchitis, belongs, in the first instance, to him.

Dilatation of the air-cells, or *emphysema of the lung*, a condition mentioned by Sir J. Floyer, Morgagni, Baillie, and others, was first accurately described by Laennec, and was by him considered as a consequence of bronchitis. Almost all pathological writers since Laennec's time have concurred with him in this opinion, with the exception of M. Louis, who, on grounds which will be hereafter noticed, has cast a doubt over the alleged production of emphysema by catarrh, while admitting their frequent co-existence. I have already alluded to some points in which Laennec's view of the connection of these two affections is open to attack; but the correctness of his observation, as to the dependence of emphysema in many cases on bronchitis, is, I think, quite unquestionable, notwithstanding the arguments of M. Louis. This will, I hope, appear clear from the sequel.

Along with this affection may be placed the "interlobular emphysema" of Laennec, which is produced under nearly the same circumstances, and often simultaneously with the dilatation of the air-vesicles. It is, however, an essentially different affection, not only in

its site, anatomical character, and mode of production, but also in its result,—inasmuch as it is never, I believe, a truly permanent lesion, and would, therefore, have been more correctly, though not so conveniently, noticed in the former part of this memoir.

Obliteration of the Bronchi is somewhat slightly and confusedly alluded to by Andral as a consequence of bronchitis, having been altogether passed over by Laennec. It is treated anatomically as an independent affection by Reynaud at great length;[1] and the conclusions of Reynaud's memoir have been elaborately adapted to the subject of bronchitis by Dr Stokes. With these exceptions, the obliteration of the bronchi has received, like the atrophy of the lung, but little attention, except as a sequel of tuberculous and other primary diseases of the lung itself.

It is very different with the opposed condition of the *dilatation of the bronchi*, which, since its elaborate description by Laennec, has been almost universally admitted by pathological writers to be connected with, and probably caused by, bronchitis, although the nature and origin of the affection have led to much difference of opinion, and are still among the obscurest points of pulmonary pathology.

In addition to these lesions, I have likewise to include, as a consequence of bronchitis, the pulmonary concretions which are frequently found in the midst of atrophied and indurated portions of pulmonary tissue, and which have been described by Bonetus, Morgagni, and almost all the earlier pathological anatomists, as often connected with asthma. It is well known that the tendency of modern opinion has been to follow Laennec in considering these bodies as the result of cured tubercle,—a view of their origin which, though undoubtedly correct in many cases, I am induced to regard as too exclusive, and as therefore requiring modification.

Upon the anatomy of these affections I shall have so much to say in my next paper, as may serve to illustrate their relation to one another, and to the primary results of bronchitis already described. I propose, however, to treat chiefly of the causes and mechanism of their production,—a pathological difficulty to which the knowledge of pulmonary collapse, as a frequent result of bronchitis, furnishes, I believe, the sole and indispensable key. Those who are acquainted with the confusing and apparently almost hopeless discordance of opinion on most of these subjects, among both practical and systematic writers, will not look upon any attempt at their reconciliation as a superfluous task.

For the convenience of the printer, I insert here a table of cases of emphysema of the lung, to which reference will be made in next Number.

---

[1] Memoires de l'Acad. de Méd., T. iv.

TABLE OF FORTY CASES OF PULMONARY EMPHYSEMA, SHOWING ITS CONNECTION WITH OTHER AFFECTIONS, ESPECIALLY OF THE LUNGS.

Vol. and No. in Path. Reg.	Age.	Sex.	Amount, Position, and Extent of Emphysema.	Collateral Affections of Lungs.	Collateral Affections of other Organs.	Duration and Character of Symptoms.
XII. 10	37	F.	In L. L. sub-pleural bullæ at apex, &c. In R. L. irregular emph. of inferior borders.	Condensation, non-granular at posterior portion of R. L. Lobular condensation. Cretaceous tubercle in apices.	None mentioned.	Cholera. No detailed history of symptoms. Died in reaction, with considerable dyspnœa.
XII. 25	31	F.	"Anterior part of both L. highly emphysematous."	"Posterior lobes much congested. Lower and back part of R. L. *somewhat hepatised (?)* and studded sparingly with miliary tubercles." Muco-purulent matter in bronchi.	None mentioned. "The abdominal organs healthy."	"Subject to want of breath from childhood." Dyspnœa, cough, lividity. Thick white sputa.
XII. 32	39	M.	The sub-pleural vesicles mostly solitary, — not larger than a barley-corn.	None mentioned. Upper lobe of R. L. compressed by a neuromatous tumour.	Neuroma of vagus nerve. Necrosis of scapula.	Frequent difficulty of respiration. Great exhaustion.
XII. 36	49	M.	Position and extent not mentioned. (The chest arched in front.)	L. L.—a cavity at apex, with irregular condensation. R. L.—miliary tubercles with condensation, as in L. L.	Cysts in kidneys.	Fever succeeded by bronchitis. No old history.
XII. 47	69	M.	Anterior edges of both lungs.	Chronic gray induration of upper and posterior parts of both L., with cavities and cicatrices of pleura (non-tubercular).	Hypertrophy of heart.	Six months' (?) Cough, dyspnœa, &c.
XII. 67	22	M.	"Ant. edges very emphysematous." (Chest arched in front.)	Adhesions. L. L. hepatised in lower part of upper lobe. R. L., scattered lobular condensation in posterior part. Bronchi containing much mucus.	None mentioned.	Acute attack; 8 days. An *equestrian* in Batty's circus—intemperate, and exposed to cold.

XII. 108	19	M.	"Exceedingly emphysematous at ant. edges."	Sulci between emphysematous portions. Gray condensation. Tubercle, with small cavities.	Extreme emaciation. Hypertrophy of liver and spleen. Tubercle of intestines.	Chronic phthisis; 4 years.
XII. 169	17	M.	Anterior portions. (Sternum arched.)	Tubercular condensation with cavities, scattered through both lungs, except a small part of anterior edges. Adhesions.	Tubercle of mesenteric glands, and slight ulceration of intestines.	No history. Symptoms acute.
XII. 220	—	M.	Whole anterior part of both lungs.	Some recent tubercle. Chronic and irregular condensation. Old puckering in apex of L.	Aneurism of aorta opening into œsophagus.	No information.
XII. 222	—	—	"Highly emphysematous at some points."	"Lobular condensation, alternating with crepitating and emphysematous lung. Outline very irregular, from sinking of the condensed, and prominence of emphysematous parts."	Softening of cerebellum.	No information.
XII. 272	—	—	Emphysematous at anterior edges; presenting irregularities of surface.	Lobular condensation "frequently in the midst of the emphysematous portions." Bronchial abscesses.	None mentioned.	Acute affection, succeeding measles. Fatal in 3 weeks.
XII. 274	38	F.	"Considerable emphysema of both lungs anteriorly."	Calcareous deposits scattered through both L., especially at apex. No condensation or tubercle. Adhesions.	Calculus in left kidney.	No information.
XII. 275	38	M.	"Both lungs very emphysematous anteriorly." (Chest considerably arched in front.)	"Slight condensation of both L. posteriorly." "R. L., a portion completely condensed. Bronchi full of muco-pus."	Hypertrophy of heart.	No information.
XII. 288	50	M.	"Air-vesicles much enlarged beneath the pleura at whole anterior part."	"L. posteriorly condensed, and scarcely crepitating." In the midst of the crepitating and emphysematous portions small lobular condensations. Adhesions.	Hypertrophy of heart.	Repeated attacks of chest symptoms for many years.
XII. 317	45	M.	"Ant. and lower edges excessively emphysematous."	Lobular condensation. Bronchi clogged with thick mucus. Soft lymph on pleura.	None of importance.	No information.

TABLE OF FORTY CASES OF PULMONARY EMPHYSEMA, SHOWING ITS CONNECTION WITH OTHER AFFECTIONS, ESPECIALLY OF THE LUNGS.—(*Continued.*)

Vol. and No. in Path. Reg.	Age.	Sex.	Amount, Position, and Extent of Emphysema.	Collateral Affections of Lungs.	Collateral Affections of other Organs.	Duration and Character of Symptoms.
XII. 323	60	F.	"Both lungs emphysematous anteriorly."	Bronchi contain much mucus. "Right side yields copious frothy serum. No condensation."	Hypertrophy of heart (right auricle).	No information.
XII. 326	—	—	Both lungs very emphysematous.	Irregular lobular condensations. Bronchi contained much mucus. Bronchial abscesses. Adhesion at apex of R. L.	Hypertrophy of heart.	No information.
XII. 327	35	M.	"L. emphys., particularly the anterior borders."	Lobular condensations; bronchial abscesses; cretaceous matter in bronchial glands.	Granular kidney. Necrosis of thigh.	No information.
XII. 345	45	M.	Both L. remarkably emphysematous. (Sternum very arched; sides of chest flattened.)	Hepatisation and collapse. Cicatrices at apex of R. L. Bronchi congested and thickened. Adhesions.		Cough *constant* for a year. Hæmoptysis. Grad. emaciation.
XII. 349	36	M.	L. L. generally emphysematous.	L. L. containing miliary tubercle. R. L. contracted. Empyema, with contraction of right side of chest.	None mentioned.	3 months. Acute attack. Signs of empyema.
XII. 350	30	F.	Anterior edges partially emphysematous.	Between emphysematous portions, collapsed lobules; and a considerable amount of condensation post. Bronch. abscesses.	Ulceration of colon.	No information.
XII. 352	17	M.	R. L.,—intense emphysema of upper lobe; marked also in lower. L. L.,—much less emphysema, but in same situations as in R. L.	R. L.,—deep furrows between emph. portions, corresponding to dark atrophied parts. L. L.,—upper lobe much atrophied, and containing calcareous matter. Adhesions.	Hypertrophy of heart. Firm indurated deposits in spleen.	No information.

XII. 355	—	—	Great dilatation of air-cells (to size of a bean) in the anterior prolongation of upper lobe of right lung.	Collapse of upper lobe of R. L., with bronchial abscesses. Diffused collapse posteriorly, and lobular anteriorly, along with emphysema. Adhesions.	Slight hypertrophy of right side of heart. Stricture of urethra, &c.	No information.
XII. 360	7	M.	Interlobular emphysema confined to anterior prolongation of upper lobe of left lung.	R. L. healthy, except imperfect and partial collapse. L. L. contains a mass of tubercle in lower lobe, obstructing some of the bronchi. At the anterior edge, opposite this mass, lower lobe completely collapsed.	Tubercular lymph at base of brain.	Sub-acute hydrocephalus.
XII. 364	30	F.	Both lungs very emphysematous at anterior edges. Some air-cells dilated to size of walnuts.	Posterior portions of lungs completely collapsed. Emphysematous prolongation of upper lobe of L. L., connected with the rest by partially collapsed tissue.	Cancer of uterus, ovaries, and peritoneum.	No information.
XII. 390	16	M.	Both L. highly emphysematous at anter. borders, especially at the anterior part of lower lobes.	Condensation of posterior portions. Adhesions.	Contraction of mitral orifice. Slight hypertrophy of right side of heart, with granular degeneration of fibres.	No information.
XII. 400	50	F.	Lungs very emphysematous anteriorly.	Atrophied portions mingled with the emphysematous. Posterior parts but slightly crepitant.	Contraction of mitral orifice. Hypertrophy of heart. Disease of kidneys.	No information.
XIII. 1	40	M.	Anter. portions in both; in left, all the lobes anteriorly, and the lower and outer border of lower lobe.	Collapsed, intermixed with emphysematous portions. Firm concrete pus in some parts, forming small roundish masses of nearly cartilaginous consistence. Partial hepatisation. Adhesions.	Purpura spots on surface. The intestines healthy.	No information.

TABLE OF FORTY CASES OF PULMONARY EMPHYSEMA, SHEWING ITS CONNECTION WITH OTHER AFFECTIONS, ESPECIALLY OF THE LUNGS.—(*Continued.*)

Vol. and No. in Path. Reg.	Age	Sex.	Amount, Position, and Extent of Emphysema.	Collateral Affections of Lungs.	Collateral Affections of other Organs.	Duration and Character of Symptoms.
XIII. 9	—	F.	Highly emphysematous in anterior half.	Lobular collapse at edges, alternated with emphysematous portions. Posterior part of left lung crepitated imperfectly. Bronchi contained much muco-purulent matter. Adhesions.	Tubercle in bronchial glands and kidney.	No information.
XIII. 10	—	M.	In both lungs, but greatest in left; in upper part, anterior and lower borders, and at the diaphragmatic surface.	Emphysematous parts had furrows corresponding to parts affected with lobular collapse. A part of the lower margin of R. L. crepitated very sparingly. Bronchi filled with pus, and dilated.	Bronchial glands enlarged and dark. An old sinus opening into right bronchus at the root of the lung. Old miliary tubercle of kidney.	No information
XIII. 18	40	M.	"Highly emphysematous in their anterior half."	Lobular collapse and atrophy at edges. Posterior half of both L. slightly collapsed, but still crepitating. Bronchi contained much muco-purulent matter.	Commencing cirrhosis of liver. Very slight and doubtful hypertrophy of right ventricle of heart.	Bronchitis. Duration not stated.
XIII. 27	45	M.	Both lungs very emphysematous anteriorly.	Posteriorly, imperfect collapse, with œdema. Small points of extravasation.	Hypertrophy of heart (20 ozs., chiefly left). Bright's granulations of kidneys.	No information (Report of examination imperfect in many points.)
XIII. 30	45	M.	Lungs emphysematous over anterior third.	Atrophied portions in emphysematous edges. Collapse of posterior third. Mucus in bronchi.	Clot in left hemisphere of brain.	No information.

XIII. 34	40	F.	Lungs highly emphysematous over ant. third, and in lower border. Air-vesicles dilated to size of mustard seed.	Completely atrophic portions in midst of emphysematous. Mingled collapse and atrophy in post. two-thirds. Hard encysted nodules scattered through both L., containing much carbonaceous pigment.	None mentioned.	No information.
XIII. 49	68	M.	Both lungs extremely emphysematous anter. In upper lobes, bullae size of a hazel-nut.	Diffused and limited condensation, collapse, atrophy, induration, with encysted concretions in the midst of emphysematous parts. Bronchi loaded with muco-purulent matter.	Superficial fibrous degeneration of brain, &c.	No information.
XIII. 74	40	M.	Lungs emphysematous anteriorly.	Emphysematous edges present scattered collapsed lobules. Posterior—incomplete collapse of right, and hepatisation of left, lung. Lymph on pleura.	Hemorrhagic ulcers of stomach, &c.	No information.
XIII. 81	22	M.	Lungs at anterior part pretty generally emphysematous.	Considerable collapse at posterior part, and scattered in midst of emphysematous portions. Tubercles—miliary, yellow, softened; small cavities, with puckering of pulmonary tissue (atrophy). Encysted masses, scarcely cretaceous.	Ulcers of intestines. Bright's disease of kidneys.	Symptoms acute; lasted 10 days.
XIII. 84	30	F.	Extensive emphysema at ant. edges. In left L. several pedunculated lobules, and greater dilatation than in right.	Collapse, diffused and scattered among emphysematous parts. Hemorrhage into bronchi.	Hypertrophy of right side of heart.	Pain in chest, cough, and expectoration, for 6 months.
XIII. 86	54	M.	Extensive and extreme emphysema; at upper part of left lung, one bulla, size of a pigeon's egg.	Much atrophy, with induration at apices, and scattered through emphysematous parts. Some concretions, cicatrices, a single small cavity (bronchial abscess) in left lung. Chest almost cubical in form.	Bright's granulations of kidney, &c. &c.	Symptoms of asthma of many years' standing.
XIII. 88	25	F.	Extensive and tolerably uniformly diffused emphysema of ant. edges. Nowhere great dilatation of air-vesicles.	Traces of atrophied lobules at anterior edges. Diminished crepitation of lungs behind.	Other organs not examined.	No history beyond the ultimate attack.

ARTICLE V.—*Statement in Reply to Mr Syme's "Case of Stricture of the Urethra Treated by External Incision," Published in the April Number of the Monthly Journal* By JAMES MILLER, F.R.S.E., Surgeon in Ordinary to the Queen for Scotland, Surgeon in Ordinary to H.R.H. Prince Albert for Scotland, late Senior Surgeon to the Royal Infirmary, Professor of Surgery in the University of Edinburgh, &c.

IN reply to those portions of the paper by Mr Syme, published in the April Number of the Monthly Journal, which seem to require notice by me, I beg to submit the following narrative:—

At a meeting of the Medico-Chirurgical Society, on January 15, 1851, Dr R. Mackenzie read an account of a case of strictured urethra, in which, after the performance of Mr Syme's operation, the patient died of pyæmia. A case closely analogous had occurred to myself, in hospital practice; and, had I been present, I would have communicated the particulars of that case to the Society,—as tending to relieve the specialities of the operation from connection with the fatal issue in Dr Mackenzie's patient. Having been absent, I resolved to mention the case at next meeting, and also to embrace that opportunity of stating my views in regard to the question of perineal section in general.

Accordingly, I gave notice of "A Case of Perineal Section"[1] for next meeting; and on February 19th, read the following short account of it to the Society:—

"The patient was a porter, aged 35, of bulky frame and intemperate habits, admitted into hospital 13th March 1850, on account of stricture, which he declared had proved impenetrable to many surgeons. A hard circumscribed swelling could be plainly felt through the perineum in the region of the bulb, about the size of a hazel nut; and the smallest catheter, passed down to this, obstinately refused to enter any part of the stricture. The swelling, so hard, and circumscribed, and resisting, was thought by myself and colleagues, in consultation, to be a calculus impacted in the urethra; and accordingly it was resolved to cut it out. The patient having been placed as for lithotomy, and under chloroform, a central incision was made; but instead of a stone, I found only gristly induration of the perineal tissues. Accordingly, I immediately changed the plan of procedure; and having, by cautious continuance of the in-

[1] The phrase "Perineal Section,"—in which there really seems to be nothing very "foolish," and as to whose origin I do not know where the blame or merit rests,—I understand to include the whole subject of external incision applied to the cure of stricture behind the scrotum. Mr Syme, though disapproving of the term as "foolish," seems to think, notwithstanding, that when employed it must always mean his particular operation. Others, again, apply the term exclusively to the old operation.—*Vide* WAKLEY, *Lancet*, March 22, 1850, p. 315. I have always used it in its general or comprehensive sense.

cision, exposed what I believed to be the shrivelled remains of the urethra—looking like a narrow lacuna,—I divided the whole constricted part, and then had no difficulty in passing a silver catheter of medium size from the orifice of the urethra on to the bladder, retaining it for nearly three days. No bad symptom occurred for five days after the operation, urine flowing freely, partly by the urethra and partly by the wound. On the fifth day, however, symptoms of pyæmia set in with great severity; beginning with rigors and great pain in the calves of the legs, which soon suppurated; running the ordinary course of oppressed respiration, delirium, prostration, leaden colour of face, &c.; and proving fatal on the nineteenth day after the operation. There was no swelling of the perineum or scrotum; and the wound retained a perfectly healthy appearance, presenting no sign of infiltration or abscess. The case seemed one of pure pyæmia, at that time unfortunately prevalent in my wards; and I believe that the unfortunate issue might have resulted as readily from any simple wound as from that of the perineum. The friends obstinately refused to permit post-mortem examination."

To this I added another case, more fortunate in its termination, in which I had performed "the old operation" in March 1847.

"The patient was a gentleman, about 50 years of age, who had laboured under bad stricture for a long series of years, deriving imperfect relief from many surgeons, both here and in London. At last retention occurred, and the surgeon, in passing a small catheter, which he did with difficulty, felt a grating in the passage. On account of this, I was asked to see him; and recognising the presence of a calculus impacted behind the stricture, which was of some extent, and felt dense in the perineum, it was resolved to remove the calculus by excision, and at the same time to cut through the callous stricture in its front. This was accordingly done without difficulty, and a catheter of medium size was retained in the bladder for about two days. On withdrawing it, all advanced favourably; the wound soon healed, health was speedily regained; and although from that day to this no catheter or bougie, so far as I know, has ever been passed, the patient continues 'in perfect health, without the least feeling of inconvenience from his former ailment.'"

After narrating these cases, I entered upon the general subject of external incision applied to the treatment of stricture, and dwelt especially on the modern variety of the operation proposed by Mr Syme,—not to decry it, but, while speaking in its praise, expressing, at the same time, fear lest it should be applied more extensively than circumstances absolutely required. In noticing the risks to which such procedure seemed liable, I alluded to a case in which Mr Syme and myself had a common interest;[1] the patient having all along

[1] I may mention, that some days before the meeting of the Society, I consulted two professional friends, in whose opinion I place great confidence, as to

been (and still remaining) under my care, but Mr Syme having performed the operation at my suggestion. Some time before the meeting, I had asked the patient—having unqualified confidence in his accuracy and intelligence—to furnish me with his own narrative of what occurred in the course of his cure. This narrative I read to the Society, pledging myself to its correctness, explaining those parts where explanation seemed necessary, and observing that I preferred it to any statement of my own, on account of its being more likely to be regarded by my hearers as beyond the possibility of bias or partiality. A short discussion followed, some account of which is published in the April Number of the Monthly Journal, p. 383. At this meeting Mr Syme was not present.

Having been assured that, in the opinion of those well qualified to judge, this paper was of such importance as to justify, or even require, publication, I prepared a digest of it, and communicated this to the "Lancet," in which Journal it appeared March 22d.

On March 6th, the following correspondence commenced:—

No. 1.

"Rutland Street, 6th March 1851.

"MY DEAR SIR,—I have been informed that, at the last meeting of the Medico-Chirurgical Society, you stated that I had of late years resorted on one occasion to the 'old operation' for the remedy of 'impermeable stricture.' I beg you will have the goodness to acquaint me with what this statement referred to. I have also to beg that you will favour me with the address of Mr ——, whose stricture I divided.—Yours very truly, "JAMES SYME.

"James Miller, Esq."

No. 2.

"51, Queen Street, 6th March 1851.

"MY DEAR SIR,—At the last meeting of the Medico-Chirurgical Society, at which I am sorry you were not present, I stated that you had performed the 'old operation' for impermeable stricture, 'with the questionable exception of rendering the incision subcutaneous instead of direct.' My ground for that statement is your paper in the Monthly Journal, October 1844, p. 821. Mr ——'s[1] present address is ——— ———.—Yours very truly, "JAS. MILLER.

"Jas. Syme, Esq."

No. 3.

"Rutland Street, 7th March 1851.

"MY DEAR SIR,—In the case to which you refer, there was a stricture at the distance of two inches from the orifice of the urethra, so tight as not to admit even a probe, and which I attempted to divide without the assistance of a director, by guiding a knife through the contracting ring, while it was held between my finger and thumb. This surely was not the 'old operation,' which essentially

---

whether or not I should formally acquaint Mr Syme of my intention of alluding, illustratively of the general question, to the case of my patient on whom he had been asked to operate. They both agreed that such a step on my part was not necessary; that the ordinary intimation of my paper in the billets of the Society was enough. And that they were right in this advice, Mr Syme himself seems to have established; by showing that he at least suspected that the "Case of Perineal Section" announced by me in the Society's circular referred to the patient on whom he had himself operated, instead of (as was really the fact) to a case exclusively my own.

[1] The patient in whom we had a common interest.

consists in trying to find the canal by *cutting upon the point of an instrument passed down to the seat of obstruction*. On the contrary, it was a proceeding, I believe, altogether unprecedented in the practice of any other surgeon, founded upon a principle entirely different from that of the old operation, and which proved the happy step that led to the method at present employed by me. For you will remark, that the result was not satisfactory, in consequence of a tendency to contraction, which soon appeared, and which I attributed to the 'incision not having been carried completely through the indurated textures surrounding the contraction,' and that being thus induced to distrust the plunge of a sharp-pointed knife, as a secure means of attaining this object, I resolved upon all future occasions to take the assistance of a grooved director. Accordingly, in the next case, related at page 822, where the stricture was also in the anterior part of the canal, and no less tight than in the former one, 'after trying various instruments without success, I passed an exceedingly slender catgut bougie through the stricture, and then a succession of larger ones, until at length a metallic bougie of the smallest size could be introduced,' when finding that no more ground could be gained by dilatation, I passed a grooved director, such as the contracted urethra admitted, and upon it divided the stricture.

"I am quite sure that you could not intend to mislead the Society, but have ascertained that you certainly did leave an impression to the effect, that after establishing the principle of treating all strictures as permeable, I had been forced to perform the old operation. The cavils, misrepresentations, and falsehoods of those base spirits in our profession, who have vainly attempted by these means to resist a great practical improvement, gave me little concern. But the statements of a colleague, and especially one who teaches the elements of surgery, cannot be treated with disregard. I beg, therefore, you will remark that the case which you quote as a dereliction of my principle, was precisely that which led to the establishment of this principle.—I remain yours very truly,

"James Miller, Esq." JAS. SYME.

No. 4.

"51, Queen Street, March 7, 1851.

"MY DEAR SIR,—To my apprehension, the essential difference between the 'old operation,' and your own, is two-fold:—1. In yours, the stricture is cut on a director; in the old, there is no such guide. 2. Yours is applied to permeable strictures; the old has been reserved for those found to be impermeable.

"According to this view, I was surely warranted in calling the case to which you refer, an example of the 'old operation.' The stricture was found to be impermeable; 'the smallest probe could not be passed through;' and the incision was accordingly made without any guide *in* the contracted part.

"Literally, it was the 'old operation,' with two points of exception: viz., the cut was subcutaneous, and there was no sound introduced to indicate the anterior termination of the stricture.

"The want of success seems to me to have depended on imperfect division of the stricture, arising from your having been trammelled and deceived by the subcutaneous puncture. Had the incision been open and direct, I am strongly inclined to infer that the result would have been otherwise.

"As to your next case, you assume that the stricture was '*no less* tight than the former;' and you argue that because the one yielded to instruments, the other would have done so likewise. Forgive me for saying, that this reasoning seems to me altogether inconclusive; and that I feel myself fully warranted in still considering your first case as an example of 'impermeable stricture.'

"You say that I have left 'an impression (in the Medico-Chirurgical Society), to the effect, that after establishing the principle of treating all strictures as permeable, I (you) had been forced to perform the old operation.' This misapprehension must surely be the fault of the Society itself. How is it possible for me, or any one else, to lay to your charge 'dereliction of principle' in regard to an operation, at a time anterior to the adoption and enunciation of that principle by you?

"Rest assured, I have entered on the public discussion of this question solely under a sense of duty; anxious, on the one hand, to assist in securing for my colleague the merit which is justly his due; and, on the other hand, to adduce arguments for restraining the operation within narrower limits than those which the enthusiasm natural to the originator might wish to assign.—Believe me, my dear Sir, very truly yours,

"Jas. Syme, Esq." "JAS. MILLER.

"*P. S.*—By a note this evening, Dr Robertson informs me that you still wish, through me, to have access to Mr ——'s MS. In consequence, I have returned it to Mr ——, from whom, I have no doubt, you will at once receive it, on application being made. I prefer this to sending the MS. to Dr Robertson, for several reasons; and all the more, because, in the same note, he complains of having been already much troubled by this matter.

"I write this late; consequently it will not reach you till Monday morning."

No. 5.

"Rutland Street, 10th March 1851.

"MY DEAR SIR,—If you merely wished to inform the Society that I at one time believed in the impermeability of stricture, and practised accordingly, I think you might have chosen a better text, than the one selected, from the following extract of my 'Principles of Surgery':—

"'If the urethra is altogether obstructed anterior to the fistula, so that even the smallest instrument cannot be passed through it, the only remedy is to introduce a catheter as far as the canal permits, and then cut upon its extremity from the fistula, so as to allow its passage into the bladder.'

"Like my neighbours, I did formerly believe in the existence of 'impermeable strictures,' and have performed the old operation. But ever since adopting the principle that every stricture, through care and the use of sufficiently small instruments, might be rendered permeable to a grooved director, I have not met with one which proved impermeable or required the old operation.—Yours very truly, "JAMES SYME.

"James Miller, Esq."

No. 6.

"51, Queen Street, March 13th, 1851.

"MY DEAR SIR,—A smart attack of illness has prevented my acknowledging your last note (March 10th) till now.

"One of my objects certainly was to prove that 'impermeable' strictures had occurred, and might be expected to occur again, even in the ablest hands. And, had I observed the quotation which you give from your 'Principles,' I would most probably have used it as an additional weight to my argument.

"I dread much any broad announcement that 'all strictures are permeable to very *small* instruments.' For these, we know, in inexperienced hands, are apt to go without much pressure where no canal previously existed. In your hands, of course, I do not doubt their safety; but I candidly confess that I expect to hear some day that, even with them, you have again encountered an 'impermeable stricture.'—I am, my dear Sir, very truly yours,

"J. Syme, Esq." "JAS. MILLER.

No. 7.

"Rutland Street, 15th March 1851.

"MY DEAR SIR,—I think it right to acquaint you, that I propose to give the Society *my* account of Mr ——'s case; and also to read the letters which have passed between us relative to '*impermeable*' strictures.—Yours very truly,

"JAS. SYME.

"Jas. Miller, Esq."

No. 8.

"51, Queen Street, March 15, 1851.

"MY DEAR SIR,—I am obliged by your note, and shall, if possible, be present to hear *your* 'account of Mr ———'s case;' although at a loss to see how that can differ, in any essential point of narrative, from the account which the Society has already heard.—I am, my dear Sir, very truly yours,

"Jas. Syme, Esq." "JAS MILLER.

I introduce this correspondence for two reasons:—1st, Because it was read by Mr Syme to the Edinburgh Medico-Chirurgical Society, with the exception of the two last notes; and 2d, Because it shows that up to March 15, no mention was made of unprofessional conduct, or want of courtesy, in regard to my public allusion to Mr ——'s case; and Mr Syme had been put in possession of all the facts, including perusal of the patient's manuscript, previously to that date.

On March 19, Mr Syme read a statement to the Medico-Chirurgical Society, and quoted also the greater part of the above correspondence. In this written statement, he attempted to substantiate the following charges against me:—He asserted that the case which I had announced and communicated to the Society at its previous meeting was "not mine, but his; read to the Society without his sanction or knowledge."[1] This statement is totally without foundation; the case in question, as has already been mentioned, being one which occurred in my hospital practice, and in which Mr Syme had no interest whatever, never having seen it but once, in consultation. Accordingly, this charge was sufficiently met at the time by requesting the secretary to read the following extract from the Society's recorded minute of the previous meeting:—"Professor Miller narrated a case of perineal section, which he would have noticed at last meeting had he not been absent. The operation had been performed, according to the old method, without a director, the stricture being impermeable. The patient died, on the nineteenth day, of pyæmia."

Mr Syme's statement evidently implied further that I had committed a breach of professional propriety and etiquette, by narrating, in the words of the patient, a case of perineal section after Mr Syme's method; the patient being my own, but Mr Syme having performed the operation. As to this, I showed that I had at least an equal right with Mr Syme to make use of the case; the patient having devolved on me the responsibility of his cure from beginning to end of the treatment, and Mr Syme having merely performed the operation at my suggestion and request.[2] And I might have added, that

---

[1] Monthly Journal, April, p. 333.

[2] "Mr Syme was consulted at *your* suggestion, and on the express condition that if he should perform the operation, you should be present, and thereafter continue your attendance as if it had been (as was originally intended) performed by yourself."—*Letter from the Patient*, April 1.

upwards of twelve months having elapsed, and many other examples of the same operation, of more recent date, having been reported by Mr Syme in the interval, it might have been not unreasonably inferred that he had thereby tacitly fallen from any presumed proprietorship of the case.

Mr Syme further complained that I had declined to furnish him, through the secretary, with the patient's manuscript which I had read to the Society. The facts in regard to this particular I stated as follows:—On application having been made to me, by the secretary, I answered that personally I had no objections to Mr Syme's obtaining access to the MS.; but before parting with it I felt it to be necessary to consult the patient.[1] Having done so, the patient declined to submit his MS. to public inspection; and I communicated this to Dr Robertson. Dr R. again applied, assuring me that Mr Syme wished to make no public use of the document. To this I rejoined that, from a note from Mr Syme (*vide* No. 1 of correspondence), I inferred that he was to place himself, in connection with this matter, in direct communication with the patient. Again, however, Dr R. preferred a request, on the part of Mr Syme, for the MS.; adding, at the same time, his wish that I should send my answer to Mr Syme himself. I accordingly returned the MS. to the patient, intimating to Mr Syme that I had done so; and adding, "from him I have no doubt you will at once receive it, on application being made." And Mr Syme obtained possession of the MS. accordingly.[2]

Mr Syme's statement further implied, exaggeration and general unfaithfulness on my part, in the report of the patient's case on whom he had performed the operation. This I met by appealing to the patient's own MS.,—not then in my possession, however.[3] I

---

[1] "Mr Miller could not, except with my sanction, have parted with the paper, it being a privileged communication."—*Letter from Patient*, quoted by Mr Syme, Monthly Journal, April 1851, p. 333.

[2] It is right here to state that the secretary of the Edinburgh Medico-Chirurgical Society has not, for some time past, been "usually supplied with the papers produced at meetings," from which he "may make out his abstract," to be "recorded in the minutes" (as has been alleged by Mr Syme, Monthly Journal, April, p. 333); but that, on the contrary, these minutes have been constructed by the secretary, from abstracts of the papers furnished by the authors themselves.

[3] According to Mr Syme, my use of the patient's MS. is a "novelty in professional conduct" (Monthly Journal, April, p. 334). The profession, however, do not need to be told that the public use of such manuscripts is no new thing in surgery, as a glance at the page of its records will abundantly show. *Inter alia*, see Syme on Stricture, &c., p. 32, where the narrative is almost entirely in the words of the patient, "a retired member of the legal profession." Mr Syme has also characterised my conduct in this matter as "a precedent of the most dangerous character." It seems to me that there may be a precedent more dangerous still,—namely, to adduce written personal charges against a colleague; to hear these met and refuted, in public discussion, at the time of their being made; tacitly to fall from them in the course of debate; and then

re-asserted the facts as they did occur; and Dr Matthews Duncan, who had attended the case along with me (having been appealed to), publicly confirmed my statement in every particular. All this Mr Syme met with unsupported assertion, and bare denial.

In the course of his reply, however, these charges against me were virtually, though tacitly, departed from by his saying, that, "after all, he could not acquit Mr Miller of the charge of having read to the Society a case for whose particulars no one was responsible." To this I replied, that in narrating that case mainly in the words of the patient, I had assumed the entire responsibility of its accuracy; and that this responsibility would be still more formally undertaken on the appearance of my paper in the "Lancet," to which journal it had been sent for publication.

Notwithstanding what had occurred at this meeting of the Medico-Chirurgical Society, Mr Syme deliberately published, in the April Number of the Monthly Journal—of which he is one of the conductors—the paper which he had read; containing the personal charges against me—"substantially identical"[1]—which had been met and refuted at the time of their first production.

These may be considered as resolving themselves into three,—1. Misrepresentation of a case; stating it to be my own, while it was not so. 2. Exaggerated or false reporting of another case, in which Mr Syme and I had a common interest. 3. Breach of professional etiquette and courtesy, in having made this latter case public, and in my mode of doing so.

As to the first, I need not again produce the simple yet satisfactory refutation afforded by the Society's own minutes, as already quoted (p. 461).

As to the second, I presume it will be sufficient to present the following evidence,—letters written by the only other persons, besides Mr Syme and myself, to whom an appeal can be made as to the facts of the case in question,—namely, the patient, and Dr Matthews Duncan.

Letter from the Patient.

"March 25, 1851.

"My Dear Sir,—I have perused with interest, and no less surprise, the proof sheets[2] of an article to be published in the 'Monthly Journal' for April, and purporting to have been read before the Medico-Chirurgical Society of Edinburgh, on the 19th ult., containing Mr Syme's account of my own case.

"I have likewise read in the 'Lancet' of last week your statement of the same case; and, under the circumstances, I cannot hesitate to volunteer my testimony to the literal and strict accuracy of the latter, notwithstanding that I

---

deliberately to print and publish these personal charges "substantially identical,"—means being at the same time taken to prevent, if possible, any contradictory statement accompanying such publication.

[1] *Vide* Monthly Journal, April 1851, p. 399.

[2] These I had submitted to the patient, on the day after I had received them from the Editor of the Monthly Journal. *Vide* Monthly Journal, April, p. 397.

thereby incur the responsibility of questioning and contradicting facts alleged in the former.

"I feel, that in matters of professional etiquette and scientific controversy, my interposition would be impertinent. I must, however, be permitted to remark, with respect to Mr Syme's observation, that 'the case was not Mr Miller's, but one of his own, the whole responsibility of which rested upon himself,' that, having been for several years in the habit of consulting you, on the 30th January 1850 I came to Edinburgh by appointment, for the purpose of having the operation performed *by yourself;* and that only upon my arrival in Edinburgh, and *at your suggestion,* and without any wish or bias upon my part, was Mr Syme called into consultation, with the view that he should operate; and this with the distinct understanding that you should be present, and continue your attendance subsequently.

"I did not see Mr Syme above four or five times between the 31st January and 18th May; and hence, undoubtedly, it is not competent to him to speak accurately (albeit confidently) of the intervening occurrences. I thus apologise for Mr Syme's flippant description of what he did *not* witness; but, with regard to which, and the other consequences of the operation, ignored by Mr Syme, Dr Matthews Duncan (having been in daily attendance throughout the whole period), yourself, the patient, and his friends, could not be mistaken, and which they could have no motive to misrepresent.—I am, my dear Sir, very faithfully yours,

"To Professor Miller."

"6, Wemyss Place, March 26,[1] Vespere.

"MY DEAR SIR,—I have read, in the 'Lancet' of last Saturday, an account of your case of aggravated urethral stricture, in which Mr Syme operated by his own new method.

"I was present at, and assisted in, the operation.

"In the course of the third night afterwards, I was called from my bed by the patient's attendants to see him.

"I found him in the most serious condition of collapse (coldness of skin—feebleness, almost absence, of pulse at the wrist—vomiting, rigors, &c.)

"From this time till the period of his leaving Edinburgh, I saw the patient daily.

"I can confidently state, that your account in the 'Lancet' of the sequence of phenomena, and their interpretation, in this case, during that period, is to my certain knowledge faithful, unbiassed, and correct in every particular.

"I am, my dear Sir, yours very truly, "J. M. DUNCAN.

"To Professor Miller."

I make no comment upon this further than to observe,—that should Mr Syme, *in plain terms,* deny the occurrence of the serious evils thus distinctly maintained, by those best acquainted with the facts, to have followed his operation in this particular case,—I fear some allowance must in future be made in reading his statements regarding the other patients, in whose treatment "no unpleasant consequences" are said to have occurred.

---

[1] This note appeared in the April Number of the Monthly Journal, as part of the short reclamation I then made. The date is March 26, while my letter bears that of March 24. This discrepancy it is necessary to explain. My original communication to the Journal contained the letter of the patient (just quoted), which I obtained on the morning after I had written my own hurried statement. But, at the request of some of the conductors of the Journal, that letter was afterwards withdrawn from the proof, and the note of Dr Duncan substituted.

As to the third charge, I have the satisfaction of adducing evidence, which I have no doubt will be considered equally conclusive with the foregoing. I addressed the following letter to two gentlemen of the highest professional standing in this kingdom:—

"51, Queen Street, March 22, 1851.

"MY DEAR SIR,—Forgive my intrusion on your leisure; and, at the same time, permit me to say, that an answer to the subjoined will be a great obligation.

"A gentleman has been my patient, on account of obstinate stricture, for a good many years past; latterly the stricture and system both so irritable as to forbid the use of instruments. It occurred to me that, under such circumstances, perineal section might be expedient; and I advised the patient to come to town accordingly. At my urgent desire he came; and I explained to him the nature of the operation, at the same time requesting that he would see Mr Syme in consultation,—deeming such conference due to the patient on the score of safety, and due also in courtesy to Mr Syme, as the projector of the peculiar mode of treatment. The consultation was held; and, Mr Syme agreeing with me that the case was one well suited for the operation, the operation was performed *by him*, on the last day of January 1850. After the operation, Mr Syme's attendance was short and occasional; the whole responsibility of after-treatment having been thrown upon me by the patient. After a lingering and interrupted recovery of about three months, the patient left town, corresponding with me. After a time he returned, expressly to redeem a promise made to Mr Syme, of permitting a bougie to be passed; and Mr Syme, seeing him with me, passed a bougie accordingly. The patient again left town, resuming correspondence with me. But, after some time, having begun to dread return of the stricture, he, at my request, came once more to reside in Edinburgh. On examination, I found the urethra so much contracted as to admit with difficulty a bougie of medium size. This was in November last. Since then he has been, and is now, under my exclusive care, having the cure perfected by bougies.

"In January last, a paper was read in the Medico-Chirurgical Society here, by an Hospital Surgeon, narrating a case of perineal section after Mr Syme's method, which had proved fatal by pyæmia. Had I been present, I intended to have spoken of a case very similar, which had happened to myself in hospital practice. Having been absent, it occurred to me to narrate the particulars of the case at next meeting, and to append some general observations on the treatment of stricture by incision. I did so; and, at the end of my paper (of which the usual notice was given in the Society's circulars, and which notice Mr Syme received), I mentioned that another case—hitherto unpublished—in which Mr Syme and myself possessed a common interest, seemed to me to have an important bearing on the safety of the operation; and that therefore, with Mr Syme's and the Society's permission, I would read the patient's own account of his cure, at the same time vouching for the narrator's intelligence and integrity. I paused for a moment, to ascertain whether Mr Syme concurred. He was not present; and I proceeded to read the patient's manuscript, making such short comments as seemed to be necessary in explanation.

"The paper having been considered important by my friends, I subsequently drew up a condensed statement of it; containing, among other things, a short account of this particular case, in which Mr Syme and I were both concerned; this being adduced for the sole purpose of illustrating the accidents which seem likely to occur, even in the most skilful and experienced hands. The paper is published in the 'Lancet,' of this day's date.

"Under these circumstances, have I, in regard to this case, in which the operation was performed by Mr Syme, transgressed the strict rules of professional etiquette and propriety?—I am, my dear Sir, most truly yours,

"JAS. MILLER."

The replies are as follow:—

"14, Saville Row, March 25, 1851.

"MY DEAR SIR,—I do not understand on what grounds it can be supposed that you have transgressed the most strict rules of professional etiquette and propriety, in publishing an account of the case which you have mentioned. In fact, the patient was more yours than Mr Syme's; but even if it had been otherwise, I hold that no information which tends to illustrate any important point in surgical practice should be withheld from the profession.—I am, dear Sir, yours very truly,

"B. C. BRODIE.

"Professor Miller."

"Dublin, April 2, 1851.

"MY DEAR PROFESSOR MILLER,—I have, as you desired, read your paper on 'the treatment of stricture of the urethra'[1] with great care; and I am bound to say that, while I found much to commend, I found nothing in it that could give just cause of offence, or that at all transgressed the rules of professional etiquette and propriety.

"I collect, however, from your letter, that it is not the paper itself, but its publication, which has given the offence.

"Now it appears to me, that a case which, from its commencement to its termination, has been under your especial care, may (professionally speaking) be strictly considered as 'your case;' and the incident of an operation having been performed in the course of it by Mr Syme, at your instance, and in your presence, in no degree transfers the responsibility of the case from you to him.

"If this be the case, it must, I think, be conceded, that you had a perfect right to bring the case under the notice of the Medico-Chirurgical Society; and if your intention of so doing was intimated (as it appears to have been[2]) to Mr Syme, by the Society's circular, I think all the claims of professional etiquette, and even of courtesy, were fully satisfied.—Believe me, my dear Professor, very faithfully yours,

"PHILIP CRAMPTON.

"Professor Miller."

But one other circumstance seems to require notice. Mr Syme has alluded to my having "met with some" impermeable strictures in hospital practice; in regard to which he indulges in the following remarkable observation,—"If, instead of resorting to extreme measures, he had tested my power to deal with them as permeable,"[3] &c. These cases were but two. The one, that which has been already narrated, as to the nature of which Mr Syme and myself were mistaken; and, in the belief that the perineal swelling and obstruction were caused by urethral calculus, excision was determined on. The other was a case of tight stricture, in which the crisis of retention occurred. I was summoned to the hospital *at*

---

[1] The paper in the "Lancet," March 22.—J. M.

[2] My notice in the circular embraced only my own case of operation, by the old method, as has already been stated. But it must, under any circumstances, have surely occurred to Mr Syme, that if I read a paper on "Perineal Section" at all, I would not fail in the course of it to allude also to the case in question. And, in point of fact, as I have formerly observed, Mr Syme did apparently understand my "case of perineal section," announced in the circular, to mean that case which he calls "one of his own."—J. M.

[3] Monthly Journal, April, p. 336.

midnight, after my clerk had failed in all attempts at relief. There was no time for delay, even if—reposing the same confidence in Mr Syme's manipulative skill which he himself seems to do—I had thought of sending for him, in his distant dwelling, at that unseasonable hour; and having been foiled in the use of the catheter, I was constrained in prudence to puncture the bladder.

Having now disposed of the personal charges adduced against me, I refrain, at present, from noticing the many points of Mr Syme's paper, which invite criticism in a surgical point of view; not without expressing the hope, however, that on some future occasion I may not be debarred from entering on a fair discussion of these, or any other professional matters, without thereby incurring the risk of having again to disencumber myself of what is gratuitously personal and offensive.

Part Second.

REVIEWS.

Pharmacopœia Collegii Regalis Medicorum Londinensis. 1851.

THE appearance of a new London Pharmacopœia, and the recent publication of one in Dublin, are fresh barriers raised against the fulfilment of the hope long cherished and not yet abandoned by us, of seeing, sometime in our lives, a pharmacopœia for the United Kingdom. Some fifteen years ago, we had hoped that this would be accomplished by a voluntary act of the English, Irish, and Scotch Colleges of Physicians, but this we have long since given up as desperate. These learned bodies are too far separated, and have too little communication with each other, to render it likely that they will spontaneously undertake what must of necessity be a laborious, and also, to some extent, an expensive business; and therefore, although we believe that there is no aversion on the part of the colleges generally to such a measure, we do not expect to see it accomplished until the legislature or the government order it to be done.

And we believe that at least a good opportunity for doing so will present itself ere long. Whatever be the result of the still unsettled question of medical reform in relation to existing medical corporations, the chemists and druggists must, we apprehend, soon be

provided for in some way or other by act of Parliament. We trust that it will be on the broad principle of equality of education, of privilege, and of responsibility, throughout the whole United Kingdom. With these, let there be uniformity in the duties required of them; let them not have three separate pharmacopœias to study; let there be one code for their guidance, and thus the same knowledge and experience, which will make a druggist good for London, will fit him equally for Dublin or Edinburgh.

We truly believe that we do not exaggerate when we affirm that the existence of three pharmacopœias in Britain has exerted an injurious effect on the progress of pharmaceutical science in this country. Our druggists are too much taken up with the study of details to have time for the mastering of general principles. It will not do for an Edinburgh druggist to know merely the indigenous formulary; he must know those of London and Dublin also. English and Irish prescriptions come to be dispensed in Edinburgh; Scotch ones in London and Dublin. A druggist, who forgets that within the shores of Britain two names may belong to the same preparation, or two preparations may be known by the same name,[1] may give to a purchaser, who comes with a prescription from the other side of the Tweed, an article quite different from what the said physic-taker has been used to, and yet may have made up the prescription quite according to (his own) rule. The purchaser is disappointed, perhaps alarmed, when he swallows the first dose of what he thinks to be "the wrong physic;" he frequents this shop no more, and probably denounces it as untrustworthy. The druggist knows that he is exposed to this mischance; to avoid this, he conns over the triplicate pharmacopœia, generally using a dispensatory, or some book where all three are collected together, in preference to the pharmacopœias themselves; he becomes, after a sort of empirical fashion, learned in minute details, whilst the scientific principles which ought to regulate his whole proceedings are left comparatively unheeded, and of course go to the wall. Had our rising pharmaceutists fewer formulæ to learn by rote, they would have more time and zeal for studying their important profession scientifically.

Nor do we see that there should be much difficulty in the way

---

[1] *Spiritus ammoniæ aromaticus* is an example. In Edinburgh, this means a spirit flavoured with the oils of lemon and rosemary, containing per fluid ounce about 20 grains of ammonia in the caustic state. In London, it means a fluid flavoured with lemon, clove, and cinnamon, containing per fluid ounce about 8 grains of ammonia in the state of carbonate. It means yet a third thing in Dublin, being there a fluid flavoured with lemon, nutmeg, and cinnamon, containing per ounce about 8 grains of ammonia in the caustic state.

We have known flatulent ladies from London much astounded by the superior potency of the Sal-volatile in Edinburgh, and heard them blame London druggists for making it badly. The fault lies, not with the druggist, but with the College of Physicians, who order him to keep an article of very inefficient character.

of a national pharmacopœia, if it were only set about in an earnest and cordial spirit. If a few general principles were established by one or two meetings of a deputation from each of the Colleges, the adjustment of the mighty conflicting claims of individual pills and mixtures could easily be settled by correspondence.

These general principles, however, must be agreed upon *in limine;* and it is precisely upon some of these that the greatest differences exist among our three pharmacopœias. We purpose, therefore, in the present notice, to direct attention to one or two of these points, taking, as the text for our remarks, the preface of the work now before us.

But ere we go further, we feel bound to say, that so far as we have yet seen what is in the new London Pharmacopœia, we have to congratulate the College from which it emanates on an evident improvement on their last production. We do not by this pledge ourselves to a general approval of the details;—a pharmacopœia is to be judged of not by a mere perusal, but by trial; but in many points, especially the list of the Materia Medica, there are evident signs of a more careful hand having been applied to the work than there was in 1836.

The points which we apprehend require to be settled before we can hope to see the construction of a national pharmacopœia entered on, may, we think, be pretty nearly comprehended under the following questions:—

1st. What ought a pharmacopœia to contain? Is it to be merely a receipt-book for facilitating the writing and dispensing of prescriptions? or is it to be a guide for directing towards the right procuring or preparing, as the case may be, of the natural or artificial productions which are to be dispensed?

2d. In what form, as regards language and nomenclature, is it to appear?

3d. Of what authority is it to be?

1st. What ought a pharmacopœia to contain?

We need hardly remind our readers that a pharmacopœia consists of two parts,—1st, A list of the Materia Medica—*i.e.*, a well-defined catalogue of substances furnished by commerce or manufacture, which are thought to be essential for the medical service of the country. 2d. A formulary containing recipes for making from these articles of the Materia Medica certain preparations of acknowledged utility. Now the question at issue respecting these two parts of the book is, How much is to go into the first and how much into the second part? Natural productions, whether indigenous or foreign, such as leaves, roots, barks, resins, of necessity go into the first part, and along with these certain manufactured articles, which are never prepared except upon the large scale, such as the carbonates of soda and potash, sulphuric acid, &c. These are thus said to be articles of the Materia Medica; and the druggist

obtains his supplies from the wholesale merchant. In the second part of the pharmacopœia are placed those preparations and compounds which it is the special business of the dispensing druggist to prepare, such as pills, tinctures, infusions, &c. So far there is no dispute; but there is a numerous and generally very important class of preparations which the ordinary retail druggist and the surgeon-apothecary are, or at least should be, perfectly competent to prepare for themselves, but which, nevertheless, they find it more economical, and in every respect better, rather to obtain by purchase from the manufacturing chemist. Such are the vegetable alkalies and their salts, and many metallic compounds, as calomel, red oxide of iron, white bismuth, &c.; and with regard to these, the views of the three Colleges are all at variance, as the following extracts from their respective prefaces will show.

The following are the London ideas on this subject:—

"Id imprimis notandum est, ut in priorem ejus partem (viz., the list of the Materia Medica) multa retulimus medicamenta, quæ olim in secundam partem (preparations and compounds) relata erant. Quippe haud iniquum fore arbitrati sumus, ut si quæ sint apud mercatores chemicos accuratissme et limatissime praeparata, ea quidem enumeremus potius et describamus, quam quâ quseque ratione et quibus modis preparanda sint exponamus; utpote quæ nostris usibus eousque inserviunt ut eadem a nobismetipsis preparari minime necesse sit."—*Præfatio, Phar. Lond.*, 1851.

The Edinburgh Pharmacopœia, however, takes a different view of the question.

"It has occurred to the College that, as the chemical preparations now in use are prepared in the present day, not by practitioners or druggists, but in a great measure by chemical manufacturers, who will scarcely be guided by colleges in their processes, a considerable proportion of the chemical formulæ might have been omitted without injury. On the whole, however, it has been thought advisable to put it in the power of every medical man to prepare his own chemical compounds if he chooses; and with that view we have taken care to select those processes which are the most simple and the most certain, though they may not be always either the cheapest or the most productive."—*Preface, Ed. Pharm.*, 1836 and 1841.

The Dublin College advocates and follows the same practice as that of Edinburgh, but on somewhat different grounds.

"And here it will be proper to observe, that formulæ are given for the preparation of numerous medicines, which are at present seldom made by the apothecary, being fabricated on the great scale by the manufacturing chemist. This is the course adopted by the College in the pharmacopœia of 1826, and it is one which should not be lightly abandoned; for he who occasionally manufactures his own chemicals, will obviously be the best judge of the quality of those which he purchases, and will be possessed of standards of comparison by which the numerous frauds and adulterations at present practised will be most readily and certainly detected."—*Preface, Dublin Pharm.*, 1850.

We at once avow our hostility to the course adopted by the London College, and think with the two other learned bodies, that formulæ for these preparations ought to be given, and that they ought not to be disposed of by making them mere articles of the Materia Medica. It is true, that it is perhaps better to have no

process at all for a chemical preparation, than to have a positively bad one, and in placing Muriate of Morphia, for instance, in the Materia Medica, the London College have done better than they did in 1836, when they gave a method of preparing this important salt, which no one would ever dream of adopting, and whose admission into any pharmacopœia was always a mystery to us. They have, therefore, wisely exercised that discretion which is the better part of valour, and have avoided some criticism and much difficulty by turning tail upon it altogether. But in so doing, they have taken the easiest, though not in our eyes the most creditable course. Some of their formulæ in 1836 were bad, but that is no reason why in 1851, they should have given none at all; their obvious duty was to have worked at the subject and given better processes, and not to have shirked the contest, and taken shelter under the wing of the manufacturing chemists. The fact is, that the College almost avows that it has been guided in selecting the formulæ which it should give, by their comparative facility or difficulty. The following sentence follows that which we quoted above:—

"Quod si nonulla sint apud eosdem mercatores chemicos haud minori cura ad nostros usus comparata, quorum tamen preparandorum rationem exponere maluimus, qualia sunt Hydrargyri Chloridum et Bichloridum, idcirco hoc factum volumus, ut nemini non in manu sit hujusmodi remediorum formula, quæ, ut potentissima ideoque gravissima, ita præparatu minime sunt difficilia; præsertum si quando ea remedia aut obtinere nequeat, aut venalia suspecta habeat."—*Praefatio, ubi supra.*

Now, we cannot translate the words *minime difficilia præparatu* in any other way, than as applying to the devising of the processes much more than to the performing of them, when we have before us the examples given by the College. We asseverate that in no respect is the execution, by a person not having a regular chemical establishment, of the process for Calomel, which is given, much less difficult than that for Muriate of Morphia, which is omitted. We will venture to assert, that if any one, not a professed manufacturer, takes to making his own chemicals at all, Calomel and Corrosive Sublimate are about the very last things he will try his hand on, and just for the very opposite of the reasons which have induced the College to give formulæ for them; first, that they are, though not abstrusely difficult, yet annoyingly troublesome in their preparation; second, that they are such common articles of merchandise, that there is no corner of the land where they cannot be got at a few hours' notice; and third, that neither one nor other of them is the subject of adulteration. We can, therefore, make nothing more of this part of the preface of the London Pharmacopœia, than that the College avoid giving any formulæ which required careful consideration in their devising, but are quite willing to give a few which will seldom be acted on, but about which they (the College) could not make any great mistake.

We have said that we agree with the two other Colleges, in think-

ing it right, that when chemicals are admitted into the pharmacopœia, they ought to have formulæ given for their preparation, excepting always, of course, such substances as oil of vitriol, carbonate of soda, and others, which are articles of general commerce, and which the manufacturer of medicinal chemicals himself purchases from the merchant. But whilst we approve of the practice which has been followed by the Irish and Scotch Colleges, we think that they might have assigned a better reason for it than they have done. The Edinburgh College gives these formulæ in order to "put it in the power of every medical man to prepare his own chemical compounds, if he chooses." Now few medical men,—that is, practitioners,—in this country, will ever avail themselves of this method of supplying their wants, and in thus consulting the interests of a class of people who will care little for the boon, the Edinburgh College has landed itself in the position of being obliged to "select those processes which are the most simple and the most certain, though they may not be always either the cheapest or the most productive." The Dublin College, again, looking to the interests of dispensing apothecaries, rather than those of medical practitioners, assign as their reason for giving the formulæ, that "he who occasionally manufactures his own chemicals will obviously be the best judge of the qualities of those which he purchases, and will be possessed of standards of comparison by which the numerous frauds and adulterations at present practised, will be most readily and certainly detected." To a certain extent this is true, an occasional manufacturer will no doubt be more likely to detect a falsified article, than one who never practises any chemical manipulations; but if this be the whole that is to be gained by the formulæ, it is hardly worth while giving them at all; *le jeu ne vaut la chandelle.* At all events, their having afforded certain rules for the guidance of occasional manufacturers, in no way justifies the Dublin College in omitting to give, like the other Colleges, a series of short tests, by which those who do not think it worth while to manufacture, and who are the parties most likely to be imposed on, may be enabled to detect some of these numerous frauds and adulterations.

We hold that there is a better reason than either of the above, for giving processes for the chemical compounds, and it is this—that the existence of a formula in a pharmacopœia is the very best way of defining and describing what the product of the process ought to be; and though the chemical manufacturer should never adopt it at all, still, with the formula before him, he knows that if he wishes to produce an article corresponding with that which the pharmacopœia authorises, he must, whatever way he takes, operate in such a manner, as to produce a result identical with that which the pharmacopœia formula would have given, had he followed it.

This remark will best be understood from examples, and we select as our instances, Muriate of Morphia, Strychnia, and Veratria.

What do we understand by Muriate of Morphia, when we send an

order for it to a manufacturing chemist? Is it Muriate of Morphia and nothing else; or is it Muriate of Morphia containing a proportion of Muriate of Codeine? On this point, the London Pharmacopœia, which has no formula for Muriate of Morphia, gives us no information. It tells us that by Morphia Hydrochloras it means a crystalline salt got from opium, and the characters appended to this definition serve to show that it does contain Morphia and Hydrochloric acid, but do not inform us whether it may or may not contain something more than these. We turn to the Edinburgh Pharmacopœia, and there we have a formula, and we see from it that the salt thus obtained, must contain a certain proportion of Codeine; and from this we infer that the Edinburgh College of Physicians consider that the presence of some Codeine is of no importance as regards the virtues of the medicine. We now take up the Dublin Pharmacopœia, and there we find a formula also, but from it we gather that Muriate of Morphia is to be prepared so as to contain no Codeine, from which we infer that the Dublin College does not think the presence of this substance a matter of indifference. Which of those rules, then, is the chemical manufacturer to follow, if you leave it to him to adopt his own ideas on the matter?

What do we understand by Strychnia? If we order it from a manufacturer who follows either the Dublin or Edinburgh Pharmacopœia, we get not Strychnia, but under this name a mixture of Strychnia and Brucia. If we procure it from a good foreign manufacturer, such as Merck of Darmstadt, we get pure Strychnia without a trace of Brucia. The Edinburgh and Dublin Colleges tell us precisely what we are to expect if we come to them for it: it is a mixture of two alkaloids, for such is the result of the formulæ which they have given. If we go to the London Pharmacopœia for it, we get we know not what. There is here no formula to tell us what they intend to be prepared for us; all we learn is, that it is an alkali got from nux vomica; that it dissolves in spirit, and burns in the fire; that it is very bitter and very potent, and must be used cautiously; and this is equally true of Strychnia containing Brucia, or of pure Strychnia, or indeed of Strychnia with a goodly per centage of other impurity—for the characters given for it are wofully imperfect.

What do we understand by Veratria? The Dublin College eschews Veratria and Veratrums altogether, and perhaps is right enough in so doing. The Edinburgh College orders it, but is content with an impure article; which is pure enough, however, for any useful purpose. This we learn from their formula, which is a simple and easy one. The London College orders Veratria, and in the absence of a formula, tells us that it is an alkali prepared from the seeds of Asagraea officinalis; that it dissolves very slightly in water, more freely in ether, but very freely in rectified spirit; that it has no smell, but irritates the nose powerfully, has an acrid taste,

and is to be dealt with cautiously. Now this may be either Veratria, as obtained pure by Couerbe's method, or it may be the impure article admitted by the Edinburgh Pharmacopœia; so that in the absence of a formula, and with such an imperfect description of it, we get either one or other of these according to the fancy of the chemist to whom we apply for it.

It may be said in reply to all this, that in none of these instances can any great harm result from these variations in quality, for that the presence of a little Codeine in our Morphia, or of Brucia in our Strychnia, or of the so-called Sabadillina and resinoid matter in our Veratria, do not really affect their medicinal qualities. We are not prepared to admit this, but even if we were, we think that there can be no hesitation in affirming this much, that it is better that there should be no doubt about the matter at all, and that, especially such medicines as are potent in small doses, had best be at least uniform in their qualities. We hold, therefore, that the exclusion of so many of these formulæ from the London Pharmacopœia is a great defect, were it for no other reason than that which we have now assigned.

But we by no means wish to maintain that this is all that can be obtained by giving formulæ for preparing chemical products and compounds. We see no reason why these processes should not be such as to be intrinsically valuable as chemical authority to our wholesale manufacturers. These are the people to be most thought of in the matter, for it is far more important that their methods of operating should lead to good and uniform results, than that an amateur chemical doctor or retail druggist here and there, should learn how to work on the small scale, by which he will certainly lose his time and probably his money, in doing unprofitably, however successful his manipulations may be, what the manufacturing chemist can do for him a great deal better, and a great deal cheaper.

We hold then that the answer to our question, What should a Pharmacopœia contain? ought to be this,—That with a list of the Materia Medica, and a good set of tests for detecting adulterations, and the usual recipes for pills, mixtures, &c., there should be formulæ for all those chemical medicinal compounds which it recognises, and that these formulæ should as far as possible be adapted to the guidance of chemical manufacturers, working on the large scale. We except of course those chemicals which are not prepared solely for medicinal use, such as oil of vitriol, aquafortis, carbonate of soda, alum, &c., which being articles of general commerce, only require to be recognised by placing them in the list of the Materia Medica, and guarding them by sufficient tests to insure their being of requisite quality.

The second question regarding a Pharmacopœia which we have proposed to ourselves is,—In what form, as regards language and nomenclature, is it to appear?

We have a deliverance on the subject of nomenclature from the London College in the following sentences :—

"Nomina medicaminum is usu hodierno habita temere immutari noluimus, quæ profecto ut plurimum aut scienter ab antiquioribus data erant, aut diuturnitate quadam temporis confirmata. Attamen haud ignari fuimus, quodcunque incrementi rerum scientia ceperit, id omne nominibus nostris complecti nos decere; modo caveatur, ne in iis aut deligendis aut faciendis, quod adhuc incertum sit pro certo et comprobato habeamus, quippe quos historiæ naturalis vestigia sequi potius quam prævenisse oportet. Utcunque se ea res habeat, nomina antiqua nominibus novis ita adjunximus, ut quod immutatum fuerit nemini non pateat."—*Praefatio.*

This is not very intelligible to us. We do not know what they mean by "nominibus nostris," whether it is the short pharmaceutical names used to designate the substance in prescribing, as "Alumen," "Borax," "Ærugo," "Quassia," &c., or the strictly scientific names which they append to the above for the sake of definition, when they state them to be respectively "Aluminæ et Potassæ Sulphas, *crystallina;*" "Sodæ Biboras *crystalli,*" "Cupri Diacetas;" "Picræna *(quassia)* excelsa, *Lindl.*" If their doctrine, that their names ought to embrace all that is new in physical science, applies to the latter terms, we assent to it most cordially; but if it is meant to apply in any respect to the pharmaceutical designations, we dissent from it entirely. Their practice, if judged of by the examples quoted above, would seem to show that they intend to do as we would have them, viz., exclude science from the pharmaceutical designations, and use it solely, but as profusely as may be, in the scientific definitions. But then they follow still a half-and-half course; some of their pharmaceutical names are such as no mutations of science can interfere with; "Alumen," "Ærugo," &c.; others are, or rather were, scientific, and are now held to be erroneous, as Hydrargyri Chloridum and Hydrargyri Bichloridum; and some are neither one thing nor another, and seem to be selected upon no intelligible principle at all, as when they designate their old "Bismuthi Trisnitras," by the singularly misapplied name of "Bismuthi Nitras." It is strange that they will not use the very obvious and unmistakeable names,[1] Calomelas, Sublimatum Corrosivum, and Bismuthum Album, for the compounds to which we now allude. They may then define them by the very newest terms of chemical science, and state Calomel and Corrosive Sublimate to be Dichloride and Chloride of mercury, if they choose to take 100 as the atomic weight of this metal; and White Bismuth to be a hydrated Subnitrate of Bismuth, if they have any confidence in the analyses by Becker of this substance. We see no reason why, in relation to chemical

[1] By a strange perversity, they use as pharmaceutical designations,—*i. e.* those which are to be used in writing prescriptions,—the theoretical names of these two salts, and use as synonymes the old unmistakeable names quoted from their pharmacopœias of 1746 and 1788. It should be just the reverse.

compounds, pharmacopœias should not, for greater precision, append the symbols to the definitions, and also quote the chemical authority for their constitution, as they quote a botanical authority when referring a vegetable substance to the plant which yields it. We say, then, that in selecting names to be used in prescribing, designations totally unscientific, and thus not liable to mutation, should be used whenever they can be found, and that the scientific terms should only be employed in the descriptive definitions which accompany these.

On the subject of the language in which a pharmacopœia should be written, the London College is, in one sense, explicit enough, inasmuch as they have again put forth their book in Latin. There is something exquisitely amusing in the obstinacy with which they cling to their Latinity; but still more amusing is the coolness with which they do so, without offering one word of apology or even of explanation for this outrage upon common sense. As the College does not vouchsafe to us any account of the reasons which have induced them, in the year 1851, to persevere in this absurd practice, we are led to assume that they have no better reasons to give than that which their present president, Dr Paris, has assigned in an appendix to his Pharmacologia, published by him in 1838. There, in replying to the criticisms of a reviewer, who seems to have fallen foul of the London Pharmacopœia of 1836, Dr Paris thus writes:—

"Why written in the Latin language? It is asked, 'For what purpose this national work is written in a dead language, and one spoken in purity by no nation on the earth?' If not spoken, it is understood throughout the civilised world, and that cannot be said of any other language; but the reader will be pleased to understand, that the committee of the College were appointed to remodel the Pharmacopœia, not to subvert the custom of the profession. Physicians have ever been, and continue, in the habit of writing their prescriptions in Latin,—whether laudably or otherwise is not the question; as long as the custom continues, the Pharmacopœia Committee can have no option. Were it necessary, I might adduce many sufficient reasons for the continuance of the practice, but I should be thus led into a digression inconsistent with my object."

The two reasons here given, and we know of none given since then, are,—the custom of the profession, and the fact of Latin being understood throughout the civilised world. Let us look into these reasons for a moment. We shall not debate whether the custom of writing prescriptions in Latin be good or bad; but, assuming it to be so good that it is a thing never to be abandoned, we ask, what has this to do with the propriety of writing a whole pharmacopœia in Latin? Could a physician not prescribe Calomel in Latin, if he find the word Calomelas given as its designation, without having all the directions for preparing it in the same language? Do the Edinburgh and Dublin Pharmacopœias, because written in English, in the least prevent Scotch and Irish physicians from prescribing in Latin? But are the customs of the profession so sacred that, without any inquiry as to whether they be good or

bad, they are to be assumed as reasons for continuing an absurd practice? If so, when are we to improve, or how comes it that we have improved in any of our professional practices at all? The "custom of the profession" would have been just as good a reason for having continued to inflict upon us knee breeches and silk stockings, a full-bottomed wig and a cocked hat, as for continuing to write a whole pharmacopœia in Latin.

But, a great reason for continuing to write in Latin, appears to be, that it is understood throughout the civilised world. We shall assume it to be proved, though we hold it to be no such thing, that the London Pharmacopœia will be better understood in France, Germany, or Italy, because in Latin. How many copies of this work go to the whole Continent of Europe? Do fifty, or twenty, or ten? Go to a Parisian Pharmacien, and ask him to make up a prescription according to the London Pharmacopœia, and see whether he will ever think of looking at the original. If he can find the formula at all, he will get it most assuredly rather from some Pharmacopée Universelle, or some other translation into French, which would have been made just as correctly from an English, as from a Latin original. And do the French people show, by their own practice, that they value Latin pharmacopœias? Look at the Codex, and see whether it is not written, after a common sense fashion, in French.

But is the London Pharmacopœia intended for foreign, or for English apothecaries? Is this Latin, which is understood throughout the civilised world, understood by the very people for whom it is written? Are our druggists' apprentices so learned, that there is not the least doubt upon this point? It is notoriously the reverse. Who are chemists and druggists? We speak not of the Morsons, and Bells, and Squires, and Redwoods; but of the people who take upon themselves, in country places, to dispense drugs, and call themselves apothecaries. Are these the people to be addressed in Latin, when it concerns processes where accuracy and a distinct understanding of what is meant, are of the first importance? A very trifling amount of honesty and candour, will teach us to reply that they are not, and that, until the highly laudable endeavours of our really good druggists to exalt and establish their profession, have completely succeeded, the general standard of learning in those who are found behind the counter in druggists' shops, is not such as to render it safe to address them in a language not their own. Even if it be true, which we do not admit, that all foreign people who may have to do once or twice in their lives with a London Pharmacopœia, are so classically educated, that Latin is quite adapted to their capacity; why make the book fitted for those whom it does not so closely concern, when it is unfitted for those for whose daily use it is intended?

The two reasons for writing in Latin, given by Dr Paris, the only authority on the subject which we possess, are so futile, that

there surely must be some others which influenced the College in their present proceeding. What they are, it is not so easy to divine.

Is it because it can thus be made more clear and unmistakeable? Let any one, in reply to this, say, whether he thinks it more intelligible to call a thing a "demissum," than to name it a "precipitate"; or to speak of "patinas fictiles planas," than to use, with the Dublin Pharmacopœia, the more homely phrase of "delf dinner-plates."

Is it because the writing in Latin is more elegant than it would be in English? Let any one honestly say what he thinks of such canine Latinity as this. Treating of weight, they say, "nos priore utimur quod et *Troy weight* vocatur"; and speaking of vessels for pharmaceutical purposes, "hæc autem rite vitrifacta[1] habemus, quod vulgo Anglice *Porcelain* vel *Stoneware* nuncupantur." But what better than a hotch-potch of this kind could we expect from writing about modern technicalities in an ancient language?

Is it from a mere desire to keep up, what Dr Paris himself calls, the "semblance of learning"? Then why not Greek rather than Latin? It is an older language, more connected with the early history of medicine, and one less commonly cultivated than Latin, and therefore more likely to give the "semblance of learning" to a body which wrote in it. It would be a fine thing to see a work bearing some such title as, Ἡ Φαρμακοποιία της των Ιατρων Λονδινου Ακαδημιας της Βασιλειας. And there would be a positive advantage in it. There may be an occasional mistake just now, from a half-educated druggist's apprentice working with a Latin pharmacopœia, which he thinks he understands, but does not. With the Greek, this would never happen; it would prove at once more than a match for him; he would give it up altogether, and quietly wait (as, however, it is fair to admit he generally does, even with the Latin) till Dr Steggall or Dr Collier, or some other compassionate man of letters, favours him with a translation.

Is it sheer love of antiquity that makes them refuse to write in modern English? Then again, why Latin? or why Greek? why not go back to Egypt, from which, it is said, the Greeks first learned the art of medicine, and let us have a truly antiquarian pharmacopœia in hieroglyphics? We could not pretend to give the hieroglyphic title for a pharmacopœia, but we should think a "cock and a bull" would be among the signs.

The real thing which keeps alive this ridiculous custom of writing in Latin, is none of these. It is the pedagogue spirit which still haunts Pall Mall East. The London College of Physicians (we speak of them only corporately, for many of them, individually, we esteem and honour), have ever been slow to per-

[1] We do not believe that the word *vitrifacta* is Latin at all. Where did the College get it? Did they take it or make it?

ceive what was for the advancement of the profession in usefulness; more taken up with the "semblance of learning" than with the reality of progress; and often found in the way, like a fat lounger in a thoroughfare, making little progress himself, obstructing the *trottoir*, and impeding those who are anxious to go on.

Let them drop the Latin, and write in the vernacular, and they will make a still better pharmacopœia of it next time.

We have no space left to discuss our third query, Of what authority should a pharmacopœia be? The London College threaten all who do not follow their injunctions, with the Queen's displeasure, and open their work, as usual, with a formidable minute of the Privy Council authorising this. The Dublin College are armed with an act of Parliament, and come down upon the offender, shillelah fashion, with a fine of ten pounds. The Edinburgh Pharmacopœia uses no such arguments, and yet is, we believe, as well followed as either of the others. We think it a pity that her gracious Majesty should be required to knit her brows, or that ten pounds should be exacted, if a druggist chooses to make, for instance, his Tincture of Calumba with rectified instead of proof spirit (which, by the way, makes a much finer preparation); and yet, if these legal documents mean anything, this is the case. We do not see that a pharmacopœia need have any other authority than that which flows from intrinsic excellence. If it have this quality, it will be followed; if it have not, it will be departed from, let the Privy Council say what they like.

Such are some of the general points which we think ought to be attended to in attempting to obtain for us a national pharmacopœia. Let it contain:—

1st, A good, correct, well-defined catalogue of the Materia Medica.

2d, Good manufacturing processes for the preparation of chemicals.

3d, Plenty of formulæ for galenical preparations, so as to embrace the best of all that are in use in the different parts of the kingdom.

4th, Let it be written in our mother tongue.

5th, Let its nomenclature be simple and unmistakeable.

6th, Let it be so good that it shall need no authority but its own merits to recommend it for universal adoption.

Lastly, Let the Royal Colleges set about this, if they are asked to do it, in a cordial spirit. They would find it a good plan to have associated with them some of our leading chemists and druggists, whom we hope to see ere long holding a proper professional status. And thus let us have a National Pharmacopœia, both useful and creditable to the united kingdom of Great Britain and Ireland.

On the Causes, Symptoms, and Treatment of Spermatorrhœa. By M. LALLEMAND. Translated and Edited by H. J. M'DOUGALL. Second Edition. London, 1851.

WE are sorry to see this work in an English dress, believing that the tendency of all such productions is very seriously to aggravate the evils for which they profess to afford a remedy. The filthy advertisements—and still more filthy publications to which they refer—that daily disgrace the newspapers, are well known to be the most fruitful source, of those monomaniacal delusions, which commit so many unfortunate victims to the rapacity of unprincipled quacks. But we do not hesitate to say, that the minute details and highly-coloured portraits which M. Lallemand has given to illustrate the alleged effects of youthful indiscretion, are in no respect less calculated to produce the same baneful effect, while the position and scientific character of their author render them even more dangerous than the publications of avowed empiricism.

"I am clear in my own mind," says the shrewd and honest John Hunter, "that the books on this subject have done more harm than good;" and we feel sure that every practitioner who directs much attention to the subject, must be of the same opinion. Indeed, as the mind and not the body requires correction, in cases of the kind in question, it is plain that everything which tends to cause or increase the impression of corporeal infirmity, must oppose the patient's recovery. While writing these remarks, we have seen a gentleman who wrote from the country a few days ago, to intimate his intention of seeking advice for the most complete prostration of energy, and depravation of all the bodily functions, resulting from some early imprudence. He tells us that he first became aware of his distressing state from the perusal of a book—that he corresponded with the author,—and spent some money in taking medicine, which had done him no good. Now this patient is obviously in good health, and suffering merely from mental uneasiness, on account of what he, in his ignorance of physiology, supposed to be a symptom of disease, while truly a sign of health. Having had the truth explained to him, he returns home completely free from all his gloomy fancies, and safely protected from empirical imposition. But if, instead of this reception, he had had his secretions subjected to microscopical examination, and been supplied with a copy of M. Lallemand's work for private study, we think the probability is, that his hallucination, so far from being dissipated, would have been confirmed, and increased the morbid craving for remedial treatment.

As to the cauterisation of the prostatic portion of the urethra, which is the great practical principle that M. Lallemand has endeavoured to establish, we beg to remark in the first place, that the very worst cases of the derangement for which he considers it

essential, have recovered without any such ministration. Secondly, That although we have employed it with the same view that we have applied the actual cautery for hysterical simulation of articular disease,—that is, to make a forcible impression on the patient's mind,—we entirely distrust the benefit supposed to result from the effect directly produced on the orifices of the seminal vessels. On the whole, then, we do not think that this book should be regarded as a creditable contribution to medical science; and we consequently cannot congratulate the translator either on the service which he has rendered to his profession, or the line of practice with which he has thought proper to connect himself.

Part Third.

PERISCOPE.

PHYSIOLOGY.

EXPERIMENTS ON THE PANCREATIC FLUID OF THE CALF. BY M. J. L. LASSAIGNE.

Since it has been shown that the pancreatic fluid extracted from the dog, enjoys the remarkable property of making an emulsion with oil, and of causing its transformation into fatty acids, either at ordinary temperatures or at the temperatures of the bodies of mammalia; it has become matter of interest to submit to similar experiments the pancreatic fluid obtained from a large herbivorous animal, with the view of ascertaining if it acts in the same manner.

M. Colin, director of the anatomical department of the school of Alfort, having procured some of it, by the method shown to him by M. Bernard, and which he had successfully put in practice with the dog, a small quantity was sent to M. Lassaigne, which he immediately examined.

The pancreatic fluid of the calf, which has an amber yellow colour, is inodorous, is transparent and limpid, and possesses a tolerably marked saline taste; when tested with reddens litmus paper, it restores the blue tint. Heated, it becomes opalescent and turbid, and deposits some coagulated white flakes, which attach themselves to the sides of the vessel in which the experiment is made.

The small quantity at the author's disposal not permitting of a direct analysis, he thought it right to examine, in concert with M. Colin, the action which it exercises on oils.

For this purpose he divided it into several portions in glass tubes, and added to each portion a drop of pure olive oil. The agitation to which he subjected the tubes, formed no emulsion—the oil separating speedily, and floating on the top.

The prolongation of the experiment did not produce any effect, even after eight hours of contact at a temperature of 60° Fahr., the oil having remained neutral, and the liquid preserving its primitive alkalinity. Subsequently exposing these same tubes in a salt-water bath to 100° Fahr., and keeping up this temperature for fourteen hours, produced no change in the properties of the oil, or in those of the pancreatic juice subjected to the experiment.

This result is opposed to that obtained from the pancreatic juice of the dog,—is it constant in the herbivorous animal, or is it an exceptional occurrence due to an alteration in the pancreatic juice of the animal, caused by the painful opera-

tion to which it had been subjected? The author does not admit the latter hypothesis, seeing that the animal survived, and did not appear to suffer in health. —*Journ. de Pharm.*, March 1851.

FURTHER EXPERIMENTS ON THE PANCREATIC JUICE OF THE DOG. BY M. J. L. LASSAIGNE.

We know from the interesting experiments of MM. Bernard and Bareswil, that the pancreatic juice possesses the remarkable property of forming an emulsion with fatty bodies, either vegetable or animal, and of transforming them into fatty acids and glycerine, at the temperature of from 95° to 100° Fahr.

M. Bernard, in subsequently repeating, at the school of Alfort, along with M. Colin, the process by the aid of which he obtained the pancreatic fluid from animals, remitted to M. Lassaigne a portion of this juice, which he had extracted himself from a dog of middle size.

The experiments to which he submitted it, allow him to add some facts to those already known, and of which the principal may be stated in the following propositions:—

1. The action of the pancreatic juice upon oils is produced even at the temperature of 53° to 60° Fahr. in some hours. In fact, on moistening several points of blue litmus paper with the emulsion produced by the olive oil and pancreatic juice, the moistened portions of the blue paper reddened shortly, from the circumference to the centre, on exposing to the air; and to produce this effect, it was not found necessary to leave the mixture during twelve or fourteen hours at the temperature of the bodies of mammalia, as has been advanced by the authors quoted above.

2. The mixture of the pancreatic juice and oil becomes acid at ordinary temperatures, as the author has ascertained after an experiment of similar duration.

3. This acidification is produced in oxygen, air, hydrogen, nitrogen, and carbonic oxide gases; the air does not seem to participate in this singular change, which is perhaps due to a force of the same nature as that designated by Berzelius, "catalytic force," to explain certain re-actions in organs and inorganic chemistry.

4. The pancreatic juice can preserve its alkalinity and property of acting on oil for several days.

5. Under the same circumstances in which the oil is modified by this fluid, sugar and gum undergo no alteration, a circumstance which points out the speciality of the action which it exercises on fatty bodies.—*Journ. de Pharm.*, March 1851.

MATERIA MEDICA.

ON THE OXIDATION OF AMMONIA IN THE HUMAN BODY, WITH SOME REMARKS ON NITRIFICATION. BY DR H. B. JONES.

The author having shown, in a paper lately communicated to the Royal Society, that the effect of tartrate of ammonia on the acidity of the urine was totally different from that of tartrate of potash, and that carbonate of ammonia taken in very large quantities, did not produce any alkaline re-action of the urine, but that, on the contrary, the acidity was rather increased than diminished by such doses, repeated the experiments with carbonate of ammonia, hoping to obtain more decided results. Although, from these experiments, it was again apparent that no diminution of the acid re-action resulted from taking carbonate of ammonia, yet the fact of any great increase in the acidity of the urine could not be determined. In his former paper, the author suggested that an inquiry into the occurrence of nitric acid in the urine would probably give the solution of this unexpected effect of carbonate of ammonia; and he was led to undertake the experiments described in the present paper with the view of detecting the presence of that acid under particular circumstances.

The indigo test for nitric acid being more delicate than the proto-sulphate of iron test, it was chiefly employed; but a mixture of starch with a drop or two of solution of hydriodate of potash and hydrochloric acid was found to be a far more delicate test than either. Beginning with 10 grs. of nitrate of potash added to 10 oz. of urine, it was found at last that as little as 1 gr. of nitre to 10 oz. of urine could be detected with the greatest certainty and clearness when the starch test was used; but this quantity could not be detected as surely by the indigo test.

Experiments are described in which carbonate of ammonia was given, in doses varying from 40 grs. to 7 grs., to a healthy man in whose urine no nitric acid could previously be detected; and the urine was tested at intervals of several hours after each dose. From these it appears that 10 grs. was the smallest quantity that gave decided evidence of nitric acid by both tests.

Having satisfied himself that when carbonate of ammonia was taken, small quantities of nitric acid passed off in the urine, the author made similar experiments with tartrate of ammonia, administered in doses of 60 and 40 grs.; and in each case the starch test gave evidence of the presence of nitric acid in the urine some hours after. Similar experiments with the muriate of ammonia are next described; and in these the presence of nitric acid in the urine was readily detected three hours after the administration of the dose, even when it was so small as 10 grs.

By an experiment described in the paper, it was shown, that by a simple combustion of ammonia out of the body, as well as in the body, nitric acid was produced. From other experiments it appears that urea, also, by oxidation, whether in the body or out of the body, gives rise to nitric acid.

Having found that nitric acid was produced more readily and frequently than had been supposed to be the case, the author was led to try whether combustions in the atmosphere without ammonia could not give nitric acid. The presence of this acid was, in consequence, detected in the products of the combustion of alcohol, of coal, of a wax candle, and of hydrogen.

As this led to the supposition that nitric acid might exist in rain-water at all times, experiments were made on the rain-water collected on wet days in London, and the presence of nitric acid was discovered by the starch and also by the indigo test.

The conclusions the author comes to from his experiments are:—

1. That the action of oxygen takes place in the body, not only on hydrogen, carbon, sulphur, and phosphorus, but also on nitrogen.
2. That in all cases of combustion, out of the body and in the body, if ammonia be present, it will be converted partly into nitric acid.
3. That the nitrogen of the air is not indifferent in ordinary cases of combustion, but that it gives rise to minute quantities of nitric acid.

He further remarks, that the production of nitric acid from ammonia in the body adds another to the many instances of the action of oxygen in man; and that the detection of nitric acid in the urine may lead to the conclusion, that the blood is being freed from ammonia, or from substances closely related to it, as urea, or possibly caffeine and other alkaloids.—*Chemical Gazette*, April 1851.

ON THE INFLUENCE OF MERCURY IN THE PRODUCTION OF CANCRUM ORIS. BY DR DUGAS.

The author states that in his district (Georgia) the occurrence of gangrenous affections of the cheeks, lips, and gums, in children, is by no means uncommon, and there are few communities in which there may not be found some living evidences of its havocs upon the face, as well as of the possibility of occasionally preventing a fatal result. He does not know of any satisfactory explanation of the fact that it affects exclusively those of tender years, and most frequently those between five and eight years of age. The object of his communication is to direct attention to a corresponding susceptibility of children of this age to mercurial salivation and sloughing, and to elicit the inquiry into the probable influence of

mercurials in the occasional causation or excitation of such a state of things. Without denying, for a moment, that sloughing phagedena, cancrum oris, or gangrenopsis (as the affection is variously denominated) may and does occur in individuals who have never taken mercurials, he inquires whether it is not probable, that the use of an agent which does of itself sometimes induce a very similar destruction of tissues, and which is especially prone to do so at the very period of life most subject to gangrenopsis, may increase the tendency to this disease, if any exist in the system? Having had his attention very early drawn to the danger of giving calomel to children during the period of second dentition, he is disposed to attribute to it the fact, that during a practice of twenty years he has never had a case of gangrenopsis which had not originated in the hands of others. He relates the case of a family of five children, three of whom had during the autumn successively taken with remittent fever, and died with most awful sloughing of the cheeks and lips. They were all treated with calomel. Discouraged at the result, the parents determined to change their medical adviser, and he was requested to see the other two children when similarly affected with fever. These were treated without mercurials, and recovered without gangrenopsis.—*Southern Medical and Surgical Journal*, October 1850, and *Provincial Medical and Surgical Journal*, March 19th, 1851.—Pp. 164.

THE DISINFECTING PROPERTIES OF CHLORINE. BY DR WEINSCH.

The author, who considers cholera to be infectious, believes that the contagion is completely destroyed by chlorine. He advises the placing in the sick-rooms flat vessels with solution of chlorinated lime, washing the clothes and utensils of patients with this liquid, enveloping the bodies of the dead in winding-sheets soaked with it, and impregnating the atmosphere of the apartments with gaseous chlorine. He experimented also with contagious matter, especially vaccine lymph, the matter of scabies and of gonorrhœa, and found that chlorine completely destroyed the activity of these contagions. In his experiments, he evolved the gas from a bottle, carried it by a tube, fitting by a cork, into a glass-bottle, with the bottom cut off; this rested in a cup filled with water, and the matter to be experimented on was placed upon a glass plate within the bottle, and was thus exposed to an atmosphere of chlorine. The chlorine appears to act very energetically upon the contagious matter, which seems to be rapidly so completely decomposed that it loses all its characteristic property. The matter commonly becomes thicker, more viscid, and acquires a darker colour. Vaccine virus, the matter of itch and of gonorrhœa, thus treated, were applied by inoculation to several individuals, but were quite innocuous.—*Schmidt's Jahrbücher*, No. I., 1851.

Part Fourth.

MEDICAL NEWS.

EDINBURGH MEDICO-CHIRURGICAL SOCIETY.

MEETING V.—*March* 19, 1851.—Dr BEGBIE, President, in the Chair.

Dr Robert Hamilton read some extracts from a medical report of the Edinburgh Eye Infirmary for the year 1850. The report had been drawn up by the medical officers of the institution—Benjamin Bell, Esq., F.R.C.S., &c., and himself. It has since been published.—(See Monthly Journal, April 1851.)

Mr Syme read a case of stricture of the urethra treated by external incision. The subject of Mr Syme's observations was one of the cases alluded to in Mr Miller's communication of the 19th February. The patient was a gentleman under forty years of age, during twenty of which he had suffered from stricture of the urethra. He had been under the care of various surgeons, and treated by the old method of dilatation without relief. In these circumstances, he submitted to the operation recently proposed by Mr Syme, which was performed by that gentleman. During the progress of recovery, symptoms manifested themselves which created some degree of alarm, but which Mr Syme thought of little consequence. A small abscess formed in the scrotum, and was evacuated by incision eight days after the operation.

The points of the case most deserving of attention were, in Mr Syme's opinion,—1st, That a stricture of twenty years' duration, which had resisted prolonged treatment by different practitioners, and progressively increased in severity, with serious derangement of the general health, was completely relieved by external incision; and 2d, That twelve months having passed without any re-appearance of the peculiar characters denoting the irritable or contractile stricture, the relief thus obtained might, he thought, be reasonably deemed permanent.

In conclusion, Mr Syme remarked that the mode of treatment he proposed was intended for the relief, not of stricture in its ordinary form, which readily yields to dilatation, but of that which resists this and all other known means of remedy. Unless a stricture resists dilatation, or speedily contracts after dilatation, he does not resort to division.

Mr Miller, *Dr Matthews Duncan*, and *Mr Syme*, took part in the discussion which followed.

SPECIAL MEETING.—*April* 2, 1851.—Dr DOUGLAS MACLAGAN, Vice-President, in the Chair.

CALCULI IN KIDNEY.

Dr Omond exhibited a kidney, containing a number of calculi in a progressive state of development, taken from a female patient, aged 50, in the Perth Infirmary. When admitted she was in extreme exhaustion, and low delirium and coma supervened. The urine, when examined shortly before death, showed traces of albumen, without any crystalline deposit, while, under the microscope, it was seen to be loaded with pus globules.

MELANOSIS.

Dr W. T. Gairdner exhibited a specimen of melanotic tubercle of the pleura and lung, from a patient who died in the hospital under the care of Professor Syme. The melanotic deposit existed in the penis, urethra, lymphatic glands of the groin and of the lumbar region, in the liver, spleen, kidneys, and in both lungs and pleuræ. The cause of death was serous and sanguineous effusion into the left pleura. Dr Gairdner also exhibited, without remark, a specimen of Bright's disease of the kidney, presenting unusually distinct granulations.

ON ELECTRO-BIOLOGY.

Dr Alexander Wood read a communication (see p. 407), entitled "Contributions towards the Study of Certain Phenomena which have recently been denominated Experiments in Electro-Biology." In the introduction of the paper, after some remarks on the common concurrence of scepticism in matters resting on authority, with credulity as to the marvellous, and on the peculiar tendency in this direction of the present age, he divided mesmeric phenomena into two classes:—

1st. *Analogous*, or those presenting characteristics showing their affinity with certain states of the body existing in health or disease.

2d. *Heterologous*, or those, such as clairvoyance, for which we had no analogues at all.

He assigned reasons for declining to consider the latter at all, in the present state of evidence regarding them; and therefore, restricting the inquiry to the first exclusively, he discussed it under the following divisions:—

1. Certain physiological and psychical laws were laid down, which appeared to the author to form the data on which a probable explanation of the phenomena exhibited by patients in the mesmeric trance might be founded.

2. He next gave a summary of the more ordinary mesmeric phenomena, attempting to refer them to the principles laid down.

3. It was shown that the views put forward were in strict accordance with the recently received doctrines of the anatomy and physiology of the nervous system. And

4. He discussed to what extent the power of producing these phenomena, and the manner of their production, could be explained on any known principle.

Dr Bennett stated that previous to the present session he had been deterred from investigating this subject by the gross charlatanism with which it was accompanied. It had so happened, however, that a very diligent and well-informed medical student, who acted as his assistant, had been found to be very sensitive, and having satisfied himself of the reality of certain morbid phenomena in his case, he had considered it due to his position to investigate the matter thoroughly. In conjunction with several members of the Royal Medical Society, he had instituted numerous experiments in private; and latterly Mr Lewis had been invited to exhibit what were called the higher phenomena of mesmerism. All these last had uniformly failed. But the nervous phenomena which had been produced opened up a wide field for speculation and medical observation; and the fact, that they could be induced in so large a proportion of the people as one in twenty, was one of considerable importance, and, he thought, new to medicine. Notwithstanding the variety the phenomena might be made to assume, they were all referable to increase, loss, or perversion of motion, sensitiveness, and intelligence. All of them could be shown to be analogous to some one of the many forms of monomania, delirium tremens, hypochondriasis, hysteria, ecstasy, somnambulism, and other morbid conditions with which medical men were familiar. The only novelty consisted in their production in many persons who were apparently in sound health. Their reality, however, being ascertained, what was their nature? They were always produced, in the first instance, by exhausting the attention, by looking for a longer or shorter time at some object. A condition of the cerebral functions was in this way produced, analogous to that in somnambulism or reverie, in which the individual could be governed by suggestive ideas, however communicated. These ideas, under such circumstances, have all the effect of reality, and influence their conversation and conduct. The condition was identical with monomania, with the difference that it was temporary, and that the predominant idea might be changed at the suggestion of others. Dr Bennett then exhibited copies, on a large scale, of the dissections made by Gall, Mayo, and Solly of the white fibres of the cerebral lobes; and stated that, in his opinion, these, like other nervous fibres, served the purpose of conducting the influence of impressions. One set of these fibres ran between the spinal ganglia and the hemispherical ganglia, and seemed to keep up a connection between the sensory and motor organs and the brain; but two other sets of fibres ran between different parts of the hemispherical ganglion, and these he considered served to convey *psychical* influences, and unite together the various mental acts, in that uniform whole which constituted thought. In the condition of the cerebral functions alluded to, these fibres were apparently paralysed, or their functions were suspended, in consequence of which the predominant idea, uncorrected by the other mental faculties, caused the individual to act under a mental illusion. There were mental as well as sensory illusions. The former were corrected in sound minds by the faculties of attention, comparison, judgment, and the latter by the application of the other senses. If the proper balance between all the mental faculties, and all the senses is destroyed, illusions of one or the other kind are likely to arise. He thought an analogy in this respect existed between the cerebral or mental, and the spinal or senso-motory functions, which, though often confined in action, were not only capable of being performed separately, but possessed an apparatus peculiar to each.

Dr Macdonald preferred the demonstrations of Foville to those exhibited by Dr Bennett. He could not concur in the explanation offered, either by Dr Wood or Dr Bennett, of the mesmeric phenomena; and made various remarks.

Dr Alison said that the only objection he saw to the statements of Dr Wood and Dr Bennett were, that they seemed to him to yield too much to the notion prevalent in general society in this city at present, that the phenomena under consideration were new to medicine. All that he had heard described seemed to him to be substantially the same as he used to describe in his lectures more than twenty years ago, under the name of Magnetic Sleep or *Extase;* and to be illustrated (particularly when heightened by the principle of imitation and by religious enthusiasm), by reference to such facts as the "Conversions at Cambuslang," recorded in the "Statistical Account of Scotland," and the history of the "Tremblans de Cevennes," the "Convulsionnaires de St Medard," and other persecuted sects in France, of whom (and indeed of the whole subject, and the inquiries of many eminent men in regard to it) we have a very distinct account in the work of Bertrand "Du Magnetisme Animal en France."

All these observations had established the existence of a very curious and peculiar modification of all the functions of the nervous system, to which these names had been given, and had shown that this condition may be produced (not always safely) in a large proportion of persons, and by various means, in influencing the minds of the persons so affected; but all had left doubtful the ulterior questions, whether a peculiar influence, passing from one person to another, could produce this state? and whether, during this state, what had been called "clairvoyance," ever really took place? This he had regarded as the state of the question about twenty years ago, and it did not appear to be further advanced at present.

Dr Bennett explained that it was not the nature of the phenomena he considered to be new, but the manner and frequency of their production in apparently healthy persons.

Dr W. T. Gairdner adduced an example of the operation on one individual of the "epidemic influence," referred to by Dr Alison, in relation to mesmerism and the allied affections:—A gentleman, who had been much in the company of mesmerisers and of believers in the extreme doctrines on this subject, though professing himself a sceptic up to the advent of Mr Lewis and Dr Darling, soon became one of their most sensitive subjects. There was no doubt of the reality of the control over his will and sensations, exercised, not only by these operators, but by several others. Soon afterwards he became seriously ill. Sensitiveness continued during some part of his illness, but the experiments were discontinued, and on his convalescence it was found that he had ceased to be sensitive, the epidemic influence having at the same time blown over. So marked was the change in the space of a few weeks, that his medical attendant was suspected of having "demesmerised" him, and Dr G. admitted that this charge was founded in truth, although no occult process had been employed.

Dr Simpson referred to what his experience of the reputed phenomena of biology had been in former years. Some fifteen or sixteen years ago he had assisted in writing a somewhat bitter article against the so-called phenomena of animal magnetism, and it was not till after it was published that he saw any of the phenomena themselves. There was an epidemic of mesmerism at that time in Edinburgh of a character similar to the present. In saying so, he meant merely to express that the study of these physiological phenomena, like the study of most other physiological and scientific phenomena, did not go on always regularly and methodically, but occurred, as it were, *per saltum*,—interest in it, as in histology, &c., being alternately revived and lost from time to time. Dr S. considered that the present epidemic of it had been useful in this respect, that it had convinced Dr Bennett, and most other physicians here, of the reality of some of the alleged phenomena to which they did not give credit six months ago. Ten or twelve years since Dr S. had seen Mr Dove produce phenomena in all respects similar in their causation to those so frequently produced in this city of late by Dr Darling and others. In relation to all these phenomena he believed

this, that though they appear very strange and very extraordinary, there were none of them whatever that indicated the presence or action of any influence passing from the so-called mesmeriser to the so-called mesmerised. In these experiments it was not the mind or the will of the mesmeriser that influenced his subjects; it was merely the words or the command spoken by him. At different times, or in different epidemics, the phenomena had varied; at least so the history of animal magnetism showed. At one time the principal symptoms were symptoms of convulsion, at another time of sleep, and at another time of somnambulism, &c. Still these and all other results were results produced by the mind of the individual himself operated upon, and not in any degree by the mind of the operator. The whole secret (if secret it may be called) in electro-biology, seemed to him to consist in the operator getting the person operated upon to *believe* in his complete influence over him. The acquisition of that power was, as was well-known, promoted by the subject being made to gaze intently and willingly upon any object for ten or fifteen minutes, and by other analogous means. Whether these means produced their effect by exhausting in some way the powers of mental self-control in the subject, was a problem which was too long to discuss. When once, however, the mind of the individual was rendered sufficiently impressible by the measures adopted, or in other words, when he was fully biologised, he, though quite awake and conscious, was so much under the command of his biologiser, that by the mere words of the latter, his muscles were fixed in particular ways, or forced to act in particular ways and directions; or by a dogmatic order to that effect, the biologiser could deprive him of the power of speech, or the power of recollection; or entirely pervert his senses of sight, smell, taste, &c.; or make him suppose himself another individual; or send him into a profound sleep; or drive him to fancy that bees were stinging him, or vipers pursuing him, &c., &c. Whatever, in short, was suggested was believed in and acted on as reality. In all these results, the rapidity with which the effect followed the suggestion, appeared almost less remarkable than another circumstance,—namely, the rapidity of the release from the effect which the observation, "Now, all's right," or the like, on the part of the biologiser, produced. The instantaneous production of any result that was ordered, was not less strange than the instantaneous removal of the result by another order. There were no analogous states in disease that could be thus artificially exorcised, or suddenly dispelled by a mere word. It had been averred, that in several instances here, the production of these phenomena as morbid phenomena, had proved hurtful to the individual biologised. Perhaps, *a priori*, we might expect this; but experience, on the contrary, certainly went altogether to show that the expectation was wrong, and the averrment of its occurrence perfectly gratuitous. Dr Macdonald had brought him, as a proof of the truth of electro-biology, a case printed and attested by many people in Cupar, showing that a man, ordered the day before, when biologised, to go to sleep at a certain hour next day, did fall asleep at that hour. But that was no proof whatever that this second sleep was the effect of aught else but an operation in the mind of the patient himself. In a sensitive and intellectual lady, who was twice or thrice the subject of biological experiments two or three months ago, Dr S. told her that next day she would fall asleep at one o'clock, because she would be mesmerised at that hour from a distance. At one o'clock she certainly did fall asleep, but as certainly without any one mesmerising. It was here, as it was elsewhere, a self-mental act in the person affected. Dr S. mentioned a case witnessed by Dr Arbuckle, Dr Wood, and Dr Taylor, in which a biologised patient slept, as commanded, for thirty-five hours, with two short intervals of permitted awakening. In that and other cases of deep biological sleep, the voice of no person, except that of the biologiser, seemed to have any effect in producing re-awakening. They were deaf for the time to other sounds. Bells may be rung in their ears, strong noises of all kinds made, tickling, shaking, rubbing the cornea, &c., practised, but they sleep on, apparently listening alone to the voice that sent them asleep, to summon them to the wakening state. We all know something of the same kind in our own experience. Many

doctors will awaken at the sound of their own door-bell, while louder sounds fail in the same effect. Dr Wood and Dr Bennett had endeavoured to explain the phenomena, but their explanation seemed limited to the explanation of the muscular motions, or want of motions only. Dr S. had no doubt that these were quite explicable, by taking into account the facts, that the influence of the emotional feelings was stronger than the influence of volition, and that the action of the muscles was interfered with in consequence of this circumstance. But this explanation did not account for other phenomena, which had been seen in this city during the last two or three months by most members of the profession. Many of the phenomena produced were apparently entirely mental, and not physical or muscular, and would seem to require a deeper psychological analysis to account for them than the doctrine of emotion merely, or the anatomical arrangement of the fibres of the brain. You tell a subject that he can recollect nothing, and instantly he forgets even his own name. This, and sleep, and local anæsthesia, and other mental phenomena, it seemed to him, could not be explained by the mere doctrine of excited emotion. The mere word uttered by the biologiser produces this and other strange effects, even when his own will wished that the word uttered should *not* produce these effects. It was always *the* word spoken, and not the will that spoke the word. Dr S. concluded by stating that he totally disbelieved in any of the so-called higher phenomena of animal magnetism, as lucidity, transference of the senses, clairvoyance, &c. If a person could see with the tips of his fingers, or the pit of his stomach, as was averred, it would be an enormous fact, and would require an enormous amount of proof to substantiate it as a fact. But the evidence adduced was generally of a very imperfect kind, and melted down before accurate inquiry. He had such a conviction of this, that he offered to place in the President's hands one hundred pounds, which he would give to Dr Macdonald for the scheme for which he was lecturing on "clairvoyance," if Dr Macdonald could bring any "clairvoyant," who could read a line of Shakespeare, or two or three words out of the Dictionary, which Dr S. would shut up in a box. If there was any truth in clairvoyance, why should not a clairvoyant be able to gain for Dr Macdonald this sum of money?

Mr Syme thought it needless to repeat experiments of the kind just alluded to by Dr Simpson, so long as the hundred pound note lay unseen and unclaimed by any clairvoyant in the bank at Dublin. Any further experiments, he thought, should be of an entirely new character.

Dr Wood, in reply, stated that he could not take so hopeless a view of the probability of explaining some of the phenomena as Professor Simpson. That he had never professed more than merely to indicate the course in which inquiries were likely, if followed, to lead to success. The cases of mistaken personality, though amusing, were not difficult to explain, and had many analogues. In regard to falling into the state, and wakening at hours predetermined by the operator, he thought it more perplexing. Two circumstances, however, seemed to tend in the direction of an explanation,—1st. The power we all possessed of wakening at an unusual hour, if predetermined to do so previously to falling asleep. This could not be by an exercise of volition, because during sleep volition is evidently suspended. 2d. The transition which, even in ordinary somnambulism, is made between two states, the one that of ideal existence, the other that of conscious personality. Dr Wood said this certainly did not explain this obscure phenomena, but it might serve as a guide to future inquiry, which, indeed, was all that in his paper he had ever presumed to do. In regard to Dr Bennett's theory, he did not think it went very far to explain the difficulties of the subject; indeed, he scarcely thought it went so far as his own. He deprecated altogether the tendency of philosophers of the present day to believe in nothing which could not be seen and handled. Granting the existence of the fibres exhibited by Dr Bennett, and all the motions he had attributed to them, he did not see that their real knowledge of the subject was advanced a single hair-breadth.

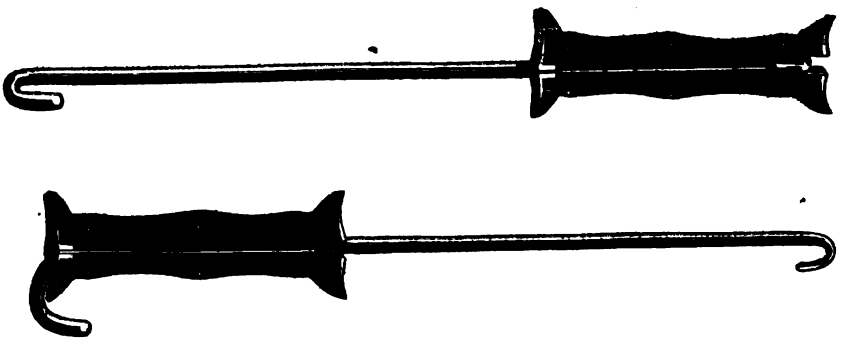
EDINBURGH OBSTETRICAL SOCIETY.

MEETING II.—*January* 22, 1851.—DR SIMPSON, President, in the Chair.

CASE OF SPONTANEOUS EVOLUTION. BY DR ADAMS, OF BANCHORY.

[This case will be found among the original articles of our February Number, detailed at full length, see p. 126.]

IMPROVED BLUNT HOOK AND CROTCHET. DR GRAHAM WEIR.



Dr Weir exhibited to the Society a new instrument, combining in one the blunt hook and crotchet, and having a handle so constructed as to slide along the body of the instrument. This handle, when adjusted at either extremity, became fixed, and thus the single instrument could be used either as a blunt hook with a fixed handle, or as a crotchet with a fixed handle.

ON FISSURING AND LACERATION OF THE STRUCTURES OF THE PERINEUM AND CERVIX UTERI IN NATURAL LABOUR. BY PROFESSOR SIMPSON.

Dr Simpson directed the attention of the Society to this important subject; and, as the result of a long series of observations on the matter, he drew the following conclusions:—

First, Fissuring and laceration of the cervix uteri and perineum are not, as is generally conceived, rare lesions during labour; on the contrary, they are very common occurrences; especially in primiparous labours.

Secondly, These lesions are not, as has been often alleged, necessarily the result of mismanagement, but they occur constantly in practice, despite every modification of management; and in cases also in which no kind of management has been adopted.

Thirdly, Evidence of the great frequency of laceration of the anterior structures of the perineum is furnished by,—1. Almost every careful autopsy of women after delivery, whether assisted or not assisted during their labour; 2. By the contracted or shortened state in which the perineum is almost always found, when vaginal examinations are made for uterine disease in women who have borne a family; and, 3. By the fissuring or laceration itself being usually traceable (under careful tactile examination), particularly in first labours, when that examination is instituted in the interval of pain, immediately before the passage of the child's head, or after its birth.

Fourthly, Lacerations of the perineum may be often felt beginning in the form of slight roughish rents or fissures upon the mucous surface of the perineum, and these may extend either backwards or forwards; and if they extend forwards, they at last run over the edge of the perineum, and along its cutaneous surface; the mucous and cutaneous structures of the perineum being thus sometimes lace-

rated, while its middle, cellular, and fascial tissues are comparatively entire, or at least not so deeply and extensively injured.

Fifthly, The proper management and support of the perineum no doubt modifies and diminishes this form of perineal lesion, but it fails far more frequently than is generally supposed, in entirely preventing it.

Sixthly, The evidence of the frequency of fissuring of the os and lower or vaginal portion of the cervix uteri is the same in character, and consists principally —1. In the frequency with which slight laceration of the edges of the os, and of the mucous and middle coat of the cervix, is detected in autopsies after natural labours, and particularly with first children; and, 2. In the permanent marks of its previous occurrence, as exhibited in those cicatrices and irregularities of the cervix uteri, which anatomists have long empirically, but correctly, laid down as proofs that they, in whose bodies they are found, have been previously mothers.

Seventhly, Fissures and lacerations of the vaginal portion of the cervix uteri not unfrequently occur to a very considerable extent in cases in which the tissues of the cervix have been rendered rigid by previous inflammation, by carcinoma, or by other morbid causes; and in such cases this fissuring or laceration, if limited to the lower or vaginal portion of the cervix, seems to be accompanied with little or no danger.

Meeting III.—*February* 26, 1851.—Dr Graham Weir in the Chair.

CASE OF PUERPERAL CONVULSIONS. BY DR KEILLER.

Dr Keiller read the following case to the Society:—

Eliza Quirk, æt. 24, the wife of a soldier, was admitted into the Edinburgh Maternity Hospital, May 27th, 1850, being in the ninth month of her first pregnancy. When interrogated, she stated that for some time past she had been in delicate health, and on examination her legs were found to be slightly swollen, in consequence of which her urine was tested three times within ten days, without any albumen being detected.

Labour commenced about three o'clock a.m., of June 11th; the pains were at first trifling and irregular, and when examination per vaginam was made at ten o'clock in the morning, the os uteri was found of the size of a sixpence. The labour progressed slowly, the pains becoming more regular, and about half-past three in the afternoon of the same day, the first stage was completed without any unusual symptom. About four p.m., the head had fairly entered the pelvis, and changed from the third to the second position. All at once, the patient turned half over in bed, and became convulsed, with the face much congested, and the eyelids twitching. In about half a minute the convulsion ceased, and the breathing became stertorous, with the eyes staring, the patient remaining unconscious.

A vein was opened in the arm, but before six ounces of blood had been abstracted, the congestion of the face disappeared, the breathing became natural, and the patient burst into tears. The arm was then tied up, and on cold water being dashed in her face, she became conscious, and inquired if the labour would soon be over. In about twenty minutes from the first attack, she had another in which the face was not changed in colour; the body and extremities were in violent motion, the chest heaved, and she cried and sobbed very much. This paroxysm was soon subdued by dashing cold water liberally on the face and chest. During this time the labour went on favourably, and the case appearing to be hysterical, and accompanied with much nervous irritability, chloroform was administered, and the patient was kept under its influence until delivery, which took place in about four hours, during which time she had six or seven attacks of a character similar to the second, but apparently much softened by the chloroform. The child—a male—was expelled a little after nine o'clock in the evening, in a state of asphyxia; but after the ordinary means had been employed for half an hour, it began to cry.

The chloroform being withdrawn on the birth of the child, the patient continued quiet, breathing naturally. The uterus contracted well, and the placenta came away in about a quarter of an hour. For an hour after this the patient appeared to be enjoying a comfortable sleep; but about half-past ten, she was again seized with violent convulsions, with pale face, eyes staring and prominent, eyelids twitching, mouth drawn to one side, lips slightly darker than natural, tongue protruded and covered with bloody froth, breathing stertorous, chest heaving very much, thumbs bent on the palms of the hands, and grasped by the fingers. Immediately after the paroxysm—the pulse being 130, small and oppressed—she was bled to about twenty ounces, two drops of croton oil were placed on the back of the tongue, mustard poultices applied to the lower limbs and between the shoulders, bottles of hot water to the feet, and cold lotions to the head. After the bleeding, the pulse fell to 112 and became fuller, the breathing continued stertorous, and the patient remained insensible, with dilated and fixed pupils.

From this time until two o'clock in the morning of the 12th, there were four or five fits, similar to the last in character but less severe; the bowels were well moved by the croton oil, and she had passed water very freely.

At two o'clock there was another fit, in which the face was livid and frightfully distorted, the lips purple, the breathing still stertorous, and the pulse very quick and feeble. On this, twelve leeches were put to the temples, the mustard cataplasms were repeated, and cloths dipped in hot turpentine applied over the chest. The convulsions returned at intervals of about three quarters of an hour, until seven o'clock in the morning, when the last occurred, and left her very weak, with fluttering pulse; dyspnœa supervened, owing to the accumulation of mucus in the bronchial tubes, and as the patient appeared to be in a sinking state, with the upper as well as the lower limbs slightly swollen and cold, small frequent doses of ammonia were administered.

The patient never having been conscious during the intervals from the time of the first fit after the labour, and still continuing in the same state, a blister was applied about nine o'clock in the morning, over the whole of the front part of the scalp; and as the pulse had become fuller, firmer, and slower since the exhibition of stimulants, a table spoonful of brandy diluted with an equal quantity of water, was directed to be given in divided doses, every half-hour.

A catheter being about this time introduced into the bladder, a few ounces of highly coloured urine were drawn off; on testing which with nitric acid there was no change, while heat gave a milky precipitate, which was re-dissolved on the addition of nitric acid.

Towards afternoon, the mucus disappeared from the chest, and she began to show signs of returning sensibility, the pupils contracted and dilated naturally, and about five p.m. she recognised those around her, said she felt thirsty, and asked for a glass of cold water. The stimulants were still continued, and the head symptoms gradually disappeared, except that the mouth remained drawn towards the right side, pulse 100, skin cool and moist. When she was requested, she put out her tongue, which was much loaded, dry, and brown. At this time, the urine was again tested, and the same results were obtained as in the morning. About seven in the evening, the pulse increased in frequency and diminished in strength, and soon became intermittent, so the brandy was increased to a table spoonful every quarter of an hour. At half-past nine, she became very restless and irritable, and complained of being sore all over, on which forty-five drops of the solution of muriate of morphia were given in a dose of the stimulant. About half an hour after the administration of the opiate, she was excited and talked for a few minutes incoherently; she then fell asleep and slept quietly till half-past four in the morning of the 13th, the respirations being twenty-six in the minute, and the pulse calm and regular. About five the breathing again became stertorous, the respirations fifteen in the minute, pupils fixed and contracted and insensible to light, pulse 130, feeble, compressible, and intermitting, skin cold, lips livid. On applying the stethoscope, a mucous râle was heard over the greater part of the chest. The fauces were irritated with a feather, in the hope of exciting vomit-

ing, but without success; and an emetic of ipecacuanha was given. The emetic produced great nausea, but caused no vomiting, and after waiting half an hour, a scruple of sulphate of zinc was administered, which caused almost immediately copious evacuation of the contents of the stomach, and seemed greatly to relieve the symptoms. The brandy was resumed, and given in tea spoonfuls every five minutes, with beneficial effect, the pulse falling to 112, the pupils becoming contractile, and the stertorous breathing ceasing. About 11 o'clock a.m., the respiration became very feeble, and the pulse almost imperceptible, and she appeared to be fast sinking, but rallied under larger doses of stimulants.

From this time she continued to improve a little, the respiration being sixteen in the minute, and without any unnatural sound; the pulse became more firm than it had been since the previous evening, and at two o'clock she asked for a drink, and put out her tongue on being requested to do so. The tongue was moist and white. A considerable quantity of urine was now drawn from the bladder, and being tested, was found to give no precipitate with nitric acid or heat, separately or combined; it was of the specific gravity of 1020, and of a deep brownish colour. As the lochial discharge had become fœtid and scanty, the vagina was syringed with tepid water. The symptoms continued much the same until four o'clock in the afternoon, when the skin became hot and dry, the face flushed, pulse 130, firm and rather full, respirations twenty-six in the minute. The stimulants were ordered to be omitted while the flushing continued, and the attendant was directed to keep the head cool, and to give the patient plenty of soda water. In about an hour, the skin cooled and the pulse became feeble, the stimulants were accordingly recommenced, directions being given to omit them on the return of an attack of feverishness.

After several remissions and exacerbations, the febrile disturbance became constant, and she was ordered one drachm of the spirit of nitric æther in cold water every other hour.

Four o'clock, morning of June 14th.—Since last report the patient has been in a dozing state. She remained flushed for some hours, during which time the hands and face were frequently sponged with cold water, and the medicine above mentioned exhibited; she has repeatedly recognised those around her, and asked for what she wished. She is now in a state of collapse, with the respirations quick and feeble, pulse 120, soft and thready; tongue moist; mucous râle heard over the lower part of the chest; bowels moved slightly. The attendant ordered to have her shifted from side to side, and to recommence the stimulants, continuing the diuretic medicine. Urine drawn off.

Two o'clock afternoon.—The patient is gradually sinking; pulse 140, respirations thirty-six in the minute, feeble and irregular; face contracted, skin cold, mouth not twisted, deglutition difficult; on the left side of the chest there is resonance on percussion, and puerile respiration; the right is very dull on percussion, with bronchial respiration. A blister now directed to be applied over the whole of the back of the right side of the chest, and half a grain of tartar emetic to be given every hour; the brandy and water to be continued.

Four p.m.—Face flushed, skin hot, and bedewed with warm sweat; respirations forty in the minute, pulse 140, rather stronger; mucus of thick viscid quality pouring from the mouth. Stimulants omitted, and the tartar emetic continued.

Half-past nine p.m.—The patient is now unable to swallow; respirations forty-five in the minute, pulse 140 and feeble; pupils dilated, eyelids closed; mucus bubbling from mouth and nostrils; dulness on percussion on the right side of the chest; bronchial respiration distinctly audible both on the right and left sides; respiration chiefly abdominal. She has bowel complaint. To have a starch enema containing a drachm of tincture of opium, ten grains of gallic acid and two table spoonfuls of brandy.

Four a.m. of 15th.—The enema passed off shortly after being administered, and was repeated in about two hours; patient lies on the back with the mouth open, eyes half closed, face pale, respirations fifty in the minute, pulse 164, consciousness entire. To have the enema every third hour.

Two o'clock afternoon.—Since last report the patient gradually sunk, and died at one o'clock.

No post-mortem examination was allowed.

CASES OF PARTIAL PARALYSIS IN THE LATTER MONTHS OF PREGNANCY. BY DR GRAHAM WEIR.

Dr Weir shortly detailed the history of two cases of paralysis of the sensitive nerves of the fingers, coming on in the latter months of pregnancy. In the first, the disease came on gradually at the ninth month; the fore-finger and thumb of the right hand were the parts affected. So complete was the palsy, that the patient was unable to feel a needle between the fingers. Immediately after delivery, sensation returned, and has continued perfect ever since.

In the second case, the disease came on about the eighth month of pregnancy; the parts affected were the thumb and two first fingers. The peculiarity in this case was that sensation was not absent, but perverted. A needle placed between the fingers felt by the patient as if it were (to use her own words) of the thickness of a common fire-poker. In this case, recovery was not so immediate as in the former. After delivery, she suffered intense intermittent pain in the middle finger, the paroxysms being synchronous with the after-pains. This state lasted for four days; it then gradually subsided, and she is now able to use the hand as formerly.

Several similar cases were mentioned by other members of the Society.

NIPPLE SUCTION GLASS. BY DR PEDDIE.

Dr Peddie exhibited a "Nipple Suction Glass," which he had employed sufficiently often, during the last half-year, to satisfy him of its utility in the cases for which it was designed. For the purposes of drawing out the nipple when flat, and drawing off the milk when the infant was unable to do so, Dr Peddie had made trial of the various instruments and means most generally recommended. The "breast pump" he had tried again and again without success. It is an expensive instrument; and in the hands even of the most intelligent nurse, is constantly getting out of repair, or its application is mismanaged. The old glass trumpet-shaped tube with ball receiver, is an utterly worthless instrument. Attendants also are very rarely met with who will employ the lips simply, or who can do so gently or properly even when willing; and a pup, when procurable—which is not often the case—is a most merciless and injurious tractor. Previous to the use of the instrument now recommended to the notice of the Society, Dr Peddie had employed with most success, a soda or Carrara water bottle, from which the air had been exhausted with hot water.

The "nipple suction glass," is of very simple construction, and can be had at a very trifling cost. The woodcut represents its full size, and will convey the best idea of its shape and peculiarities.

Dr Peddie had also employed another form of the instrument, with a lateral nipple piece.[1] The opening to receive the nipple is about seven-eighths of an inch in diameter, the edges being well rounded off. The floor of the glass is applied close to the mamma, whilst the patient assumes a sitting posture. The nurse then holds it firmly in place by two fingers spread out on its back, and applying the lips to the nipple-like projection, makes gentle and slow suction—placing the point of the tongue on the small orifice (about one-twentieth of an inch in diameter) and resting for a few seconds between each draught. By this latter precaution she will avoid giving pain, or drawing blood, while time is given for the enlargement of the nipple and the flow of the milk.

1. In cases of flattened nipples, when the child is dull and weak, and unable to draw them out, the application of this instrument for a few minutes, as directed, will make the nipple large and prominent enough for the child to take hold of it

[1] From the difficulty which the glass-blower has experienced in making this form, the instrument with the centre nipplel-ike projection only is recommended.

easily; and will, in instances of first suckling, open up the mammillary orifices, so as to enable them to part readily with the secretion.

2. In cases where the milk is found in great abundance, and the infant is from any cause unable to afford the needful relief, by means of the glass, and slow, steady, tender suction, aided by a slight expressing movement of the hand on the surrounding gland, the milk will flow freely into the lower part of the glass, which will contain about ʒi. before it rises to the level of the nipple.

3. The milk thus drawn off may, if occasion requires, be conveniently given to the infant by placing the thumb on the opening in the floor of the glass, and its nipple fixed in the child's mouth. By covering this orifice with the thumb more or less completely, the milk may be given to the infant in drops, or in a stream, or it may be suspended altogether; and if desired, a teat may be tied to the nipple-piece, and thus the habit of sucking the better kept up in the child.

4. The remaining use of the nipple-glass is its application to one breast, while the mother is in the act of suckling from the other. This is desirable in those instances in which the secretion is very abundant, and flows copiously from the one breast while it is given from the other, thus making the mother's dress most uncomfortable, and exposing her consequently to chills.

Meeting IV.—*March* 26, 1851.—Dr C. Bell in the Chair.

Notice of a "Pulvis ad Partum" in the First Edinburgh Pharmacopœia. By Professor Simpson.

By the older authors, many different medicines are described as being supposed to have an oxytocic effect upon the uterus; or, in other words, as capable of either originating or increasing the parturient contractions of that organ. From the time of Dioscorides and Galen downwards, various and diversified plants, &c., have been described in medical works, as possessing these alleged virtues. The only two believed at the present day to have a decided oxytocic effect, are ergot of rye and Indian hemp; and both of these plants when used medicinally, seem to possess also in common, a depressing or sedative effect upon the spinal nervous system. Is their oxytocic power dependant upon this action on the centre of the

reflex system? Various mineral substances used to be reputed also as oxytocic in their properties, as eagle-stone, &c. Borax has long enjoyed such a reputation. Homberg and La Motte long ago spoke of its oxytocic properties. In the first edition of the Edinburgh Pharmacopœia, published in 1699, there is given a recipe for a "Pulvis ad Partum," containing borax as its first ingredient. The powder contains, besides, crocus and amber, substances to which oxytocic powers were ascribed in olden times; and two animal ingredients that were endowed with the same properties. Its exact composition consists of Venetian borax, British crocus, the livers of eels dried with their gall, white amber, of each two drachms, and of horses' testicles dried in an oven one ounce.

"℞. Boracis Venetæ.
Croci Britannici.
Jecorum cum felle Anguillarum exiccatorum.
Succini albissimi ana drachmas duas.
Testiculorum Caballinorum in clibano siccatorum unciam unam.
M. F. Pulvis s. a."

This last ingredient is almost a forestalling of the absurd modern therapeutic doctrine of isopathy. And the whole is perhaps not uninteresting as shewing one of the resources officially recommended in tedious labour in Edinburgh practice some hundred and fifty years ago.

VITAL CONTRACTIONS IN THE UMBILICAL ARTERIES AND VEINS. BY PROFESSOR SIMPSON.

It is well known that up to a late period, some physiologists doubted entirely whether the larger arteries of the human body, could be made to contract at individual points by stimulants applied to those points. Few or no satisfactory experiments have yet been published, shewing that the veins possess the property of contracting locally under local stimuli. But in the umbilical cord, the medical practitioner may have daily an opportunity of repeating experiments, which prove both of these important physiological facts: viz. the contraction both of the tubes of arteries, and of the tubes of veins, under local stimuli applied to them.

For some time past, Dr Simpson has pointed out to his pupils the following facts, regarding the contractility of the umbilical veins and arteries.

First. After the child is born, the cord (whether tied or not), is generally seen to become contracted at different points, in consequence of the local contraction at those points of the vessels included within it. In fact, its vessels are thrown, by the irritation of the external atmosphere, and in consequence of their partially empty condition, into tubes consisting of a series of dilatations and contractions, like the peristaltic contractions of the intestine; but with this difference, that the contractions of the vessels are permanent.

Secondly. By pinching a portion of the cord, immediately after the birth of the child, between the nails of the thumb and finger, contractions of the vessels of the cord will be seen to occur at the point irritated. And if this mechanical irritation is confined to individual vessels, as to one of the arteries or to the vein, the effect is equally marked.

Thirdly. If the sheath of the cord be slit up by a pair of scissors or a knife, with any of its vessels or arteries exposed, the same experiment may be repeated upon the exposed individual vessels with similar effect: viz., that the mechanical pinching of them will be followed in the course of a short time, by a gradual but very marked contraction in the irritated part.

Fourthly. Similar local contractions of these vessels occur, under the local application to them of irritating chemical substances, or of electricity.

Fifthly. These contractions do not instantly follow the application of the stimulant, but a short time intervenes before the effect is seen, and the contraction is permanent.

Sixthly. When the experiment is made upon the human umbilical cord, or upon that of the lower animals, when the circulation is still going on in the ves-

sels of the cord, the irritated vessel will sometimes almost entirely close its tube under such local irritation.

Seventhly. These simple experiments, illustrative of the contractility and irritability of the coats of the veins and arteries, are the more remarkable as seen in the umbilical veins and arteries, in consequence of anatomists not being able to detect any nerves in the umbilical cord; although probably elementary nervous tissue may exist in some form in it. For if it did not exist, then we would have irritability in these vessels existing without nervous influence. And Dr S. proposed the investigation of the existence of nerve in any form in the umbilical cord and its vessels, as an interesting microscopical study, in reference to the physiological question of the dependence or independence of the contractility of vessels on the presence of nerves.

VARIETIES.

THE MEDICAL PROFESSION AND LIFE ASSURANCE COMPANIES.—We have always regarded the replying to questions respecting the health and constitutions of patients for the information of Insurance Companies, as a troublesome and responsible duty, which ought not to be discharged gratuitously. We have also been of opinion, that the remuneration for this service, whencesoever derived, should proceed directly from the office to the medical man employed. But, while entertaining these sentiments, we have not the less regretted and deprecated the unseemly conflict which has been carried on through the refusal of individual practitioners, to give the information required without a fee. Nothing, in our opinion, could be more calculated to lower the character of the profession than such a course of proceeding; and while sympathising with the feeling of injustice which led to it, so far as to withhold any blame from our brethren who have waged this warfare, we have long deplored the occasion of its origin and continuance.

It consequently gives us great pleasure to learn that the Colleges of Physicians and Surgeons of Edinburgh have taken up the matter, and brought their influence to bear on the refractory Insurance Companies. There can be no more legitimate and praiseworthy exercise of the power which such bodies possess on the public mind, than their interference under the present existing circumstances, and we entertain not the slightest doubt that before twelve months have expired, every office in Scotland will have complied with the reasonable remonstrances of our profession. The Standard Insurance Company, which now transacts about the largest business in Scotland, and is second in this respect to only one in England, has lately made the concession, which we trust will soon be general. The Scottish Equitable had previously led the way.

In connection with this subject, a Life Assurance Company, under the title of the "New Equitable," which has lately been instituted in London, appears to require notice, since, under the insidious pretext of recognising the claims of medical men, it appears singularly calculated to lower the tone of feeling which should exist in our profession. A circular issued by the directors and addressed to all legally qualified medical practitioners, which now lies before us, holds out a series of pecuniary temptations for connecting themselves, directly or through the medium of their patients, with the office. In the first place, they are to have *two* guineas instead of the one, hitherto considered sufficient for a medical report; then they are to have 10 per cent. down, and 5 per cent. annually, on the amount of policies obtained through their recommendation; and lastly, they have it in their power to take shares in the concern, so as to derive a further profit from their exertions in its support. It is well-known that medical practitioners are apt to acquire, in a remarkable degree, the confidence of their patients, in regard to other matters than those strictly belonging to the field of practice, and it is much to the credit of our brethren, that the opportunities so acquired have so seldom been used for their own advantage. But it seems to us that the various baits held out by the so-called "New Equitable" Company are cunningly devised to

excite the feeling of cupidity, which has happily been dormant, and may have the effect of suggesting counsel not directed solely with a view to the patients' advantage. Under these impressions, we shall watch the proceedings of this Company with extreme suspicion, and in the meantime beg to raise a warning voice against too ready confidence in its plausible schemes.

STATISTICS OF INFLAMMATION.—HINTS FOR VISITORS TO THE GREAT EXHIBITION.—During seventeen years, ending with 1849, there have occurred in London 11,305 fires, great and small; 451 houses have been totally destroyed, 3,335 have been considerably damaged, and 342 lives have been lost. Of the total number of fires, 3,584 have happened in private houses, 967 in lodging-houses, 579 in victualling-houses, 83 in hotels and club-houses, 74 in eating-houses, and 34 in churches. A stranger is certain to be burnt out once every three years who may be quartered with a lucifer match-maker, and once every six years if he inhabits a lodging-house. Even an unoccupied tenement is not so snug as it might seem, 4 or 5 being burnt annually. Nay, a man is not quite secure at the water-works, one fire having happened there, "which did serious damage." He is as safe indeed at a milliner's or dressmaker's, among whom there has been only one fire in seventeen years. But the safest berth of all is a china-shop,—no such establishment appearing in Mr Braidwood's list.

Among the causes of fires in private houses, the most prolific are candles, curtains, bad flues, and tobacco smoking; the cat accounts for 9, the dog for 1, children for 61, old age for 1, drunkards for 19, smokers for 18, carelessness for only 10; 2 fires are traced to "airing beds," 5 to "reading in bed," 2 to "bug-hunting." Whence it follows, that in London, at a time when people should be sleeping, for every five persons who read in bed, there are two hunting bugs.—*Abstract of Statistics of London Fires in the Assurance Magazine*, No. 2.

PUBLICATIONS RECEIVED.

Pharmacopœia Collegii Regalis Medicorum Londinensis. Londini: Apud Churchill. 8vo. 1851.

On the Medicinal Springs of Harrowgate. By George Kennion, M.D.

Notice of the Professional Life of the late John Walker, F.R.C.S.E. By William Brown, F.R.C.S. (From Edin. Med. and Surg. Journal, April 1851.)

Urinary Deposits, their Diagnosis, Pathology, and Therapeutical Indications. By Golding Bird, A.M., M.D., F.R.S., &c. Third edition, revised and enlarged. London: Churchill. 1851.

A Non-Medical Essay on Spasmodic Cholera. By Philo-Medicus. London: Houlston and Stoneman. 1851.

On the Threatenings of Apoplexy and Paralysis, &c. By Marshall Hall, M.D., F.R.SS., L. and E. (Croonian Lectures, 1851.) London: Longman. 1851.

Practical Treatise on the Diseases of the Urinary and Generative Organs. By William Acton. Second Edit. London: Churchill. 1851.

The Prescriber's Pharmacopœia, containing all the Medicines in the New London Pharmacopœia of 1851. By a Practising Physician. Fourth Edition. London: Churchill. 1851.

Rendiconto Delle Adunanze e Dei Lavori Dell'Academia Medico-Chirurgica di Napoli. Anno Quatro. Fascicolo II. Aprile, Maggio, e Guigno 1850.

Remarks on the Comparative Size of the Hand of the Accoucheur and of the Female Pelvis. By George King, Esq. (Reprinted from the Institute, March 1851.)

Beiträge zur Mikroscopischen Anatomie der Menschlichen Zähne. Inauguraldisserta-tion von Dr Johann Czermak.

On Syphilitic Eruptions, with Especial Reference to the Use of Mercury. By Thomas Hunt, M.R.C.S., Eng., &c. London: Churchill. 1851.

Giornale Oftalmologico Egiziano del Dr O. Abbate. Anno Primo. 1o Aprile 1851. No. I. Alessandria d'Egitto.

On the Cure of Popliteal Aneurism by Pressure at the Groin. By J. M. O'Ferrall, M.D., M.R.I.A. (From Dublin Quarterly Journal of Medical Science, November 1846.)

Practical Analysis of Seventy Cases of Inflammatory, Functional, and Structural Disease of the Heart, with Observations on the Treatment and Prevention. By S. Scott Alison, M.D. (Reprinted from London Journal of Medicine.) London: Churchill. 1851.

We beg to inform our readers, that the paper by Mr Syme at p. 401 of our present No. was *printed* before we received the statement by Mr Miller, which will be found at p. 456.

Part First.

ORIGINAL COMMUNICATIONS.

ARTICLE I.—*The Treatment of Erysipelas by the Muriated Tincture of Iron.* By G. HAMILTON BELL, Surgeon, F.R.C.S.E. With Additional Remarks and Cases, by CHARLES BELL, M.D., F.R.C.P.E.

(*Read before the Medico-Chirurgical Society of Edinburgh, April 16th* 1851.)

IT will be generally admitted that practical observations on the treatment of diseases of daily occurrence are more valuable to the medical practitioner than the most interesting descriptions of anomalous cases, however extraordinary in their character, or successful in their treatment. I am convinced, indeed, that the publication of the journals of well employed medical men, giving in detail the treatment of every case occurring in their private practice, would prove most useful to the young practitioner, and be a valuable contribution to our medical literature.

In furtherance of such views, I am anxious to bring to the knowledge of my professional brethren a mode of treating erysipelas, differing from that usually resorted to, but which I have found invariably successful. I have no intention, however, of writing a treatise on a disease so well known, and on which so much has been recently published, because I think my object will be fully attained by reporting a few cases from my own journals, illustrative of my experience, to which will be added some cases of my brother's, Dr Charles Bell, who has adopted the same treatment in this disease.

My purpose then being purely practical, it would be out of place to premise with a disquisition on the nature and causes of inflammation; but in order to explain in some measure the principle by which I have been actuated in employing a powerful tonic in a disease generally occasioning so much fever and cerebral excitement as erysipelas, I consider it necessary to repeat the opinion I have elsewhere expressed,—viz., that "in inflammation, the capillary vessels having apparently lost the power of separating or electing the component parts of the blood which are necessary for functional purposes, and become to a certain extent inert tubes, a stream of blood is admitted,

for the circulation of which they are not calculated."[1] In other words, I consider that in erysipelas the capillary vessels are in an atonic state.

This hypothesis appears to me to be supported by the effect of the treatment I have adopted in erysipelas,—the cases demonstrating that when an extensive portion of the surface of the body is violently inflamed, producing a high degree of fever and cerebral excitement; on the system being rapidly surcharged with, or brought under the influence of, the muriated tincture of iron, while the cerebral affection and other symptoms of fever subside, the local pain is relieved, and the redness and swelling gradually disappear; and, so far as the tonic medicine appears to be concerned, all this is effected without any appreciable evacuation from the emunctories of the system.

But refraining from theory or speculation, and in the hope that I shall lead to the general adoption of the practice I have suggested, and which I shall illustrate by a few cases, I shall proceed at once to state shortly the mode of treatment I have resorted to, in every case of erysipelas I have attended for upwards of a quarter of a century, without having in a single instance failed of success. I have found that my remedy is not only effectual in removing erysipelas, but that it generally renders the patient more healthy and robust than before the attack of the disease; and in no instance in which I have had the charge from the commencement of the disease has suppuration taken place. I have for a series of years pressed privately on the attention of the senior members of the profession the value of this remedy in the treatment of this always troublesome and often fatal disease; and I have taught many of my junior brethren successfully to combat it. But my prolonged experience of the invariable success of my practice justifies me, I think, in thus bringing it to the notice of the profession generally, trusting that, in the hands of others, it may prove as great a blessing as it has in mine.

Mode of administering the remedy.—Of course the first object is to have the bowels freely acted on. If the erysipelas be mild, fifteen drops of the muriated tincture of iron are administered in water every two hours until the disease is completely removed. When the attack threatens to be more severe, the dose of the tincture is increased to twenty-five drops every two hours, and persevered in night and day, however high the fever and delirium. The only local applications I ever find necessary, are hair powder and cotton wadding. While I depend for the removal of the disease on the chalybeate, it is necessary that the bowels should be attended to throughout the treatment.

Cases.

I. *25th December* 1832.—Mrs Naughton, a poor woman, about 60 years of age, of broken constitution, and having what is vulgarly called "shaking palsy"—a state of continuous chorea—had been suffering from erysipelas some days, and I was called to see her late at night. I found her head entirely covered with erysipelas; she was

[1] Treatise on Diseases of the Liver, p. 5.

in a state of great exhaustion, and delirious. Ten drops of the muriated tincture of iron were ordered to be taken every two hours. On the 26*th* she was calm and appeared easier; cont. tinct. 27*th*.—Quite collected, inflammation and swelling decreasing; cont. tinct.; to have a dose of oil. 28*th*.—Inflammation rapidly disappearing; nourishing diet; continue drops. 30*th*.—Recovering fast; *purge*. 2*d January*.—Erysipelas gone, and the poor woman wonderfully well.

II. 20*th May* 1835.—I was called to the Waterloo Hotel, at 1 o'clock P.M., to see R. R., Esq., a stout gentleman, of full habit, about 30 years of age. I found him suffering from excruciating pain in the soles of the feet, which he ascribed to cold he had caught the previous day in coming from Glasgow. The pain was so acute that he could not bear the weight of the bed-clothes, nor his feet to be touched in the gentlest manner. Tongue loaded; pulse irregular, but not accelerated; skin of natural temperature, except on the feet. Eighteen leeches were applied, and he had six grains of calomel, followed in three hours by a dose of the compound powder of jalap.

Vespere.—Pain of feet relieved, but there is excessive prostration of strength. Tongue still loaded; thirst; pulse irregular and unsatisfactory. The medicines had acted freely, and brought away a quantity of dark coloured offensive scybalous matter. Slight erysipelatous blush on the right ankle. The following medicines to be given immediately:—Calomel, five grains; henbane, six grains; opium, one grain; James's powder, three grains, in the form of pills.

21*st*.—Slept well; pain of feet much diminished; right ankle swelled and red; complains of pain behind the knee; much thirst; skin natural; pulse 120, irregular; the evacuations from the bowels black, watery, and offensive. A tablespoonful of a mixture containing one part of the spirit of Mindererus and two of camphor mixture, to be given every two hours. Hair powder and cotton wadding to be applied to the feet and ankles. 2 P.M. No improvement; spots of erysipelas at the roots of the toes and over the instep, extending to the ankles. In consultation with Dr Abercrombie, the following medicine was ordered:—Half a drachm of the compound powder of jalap and ten grains of rhubarb. The camphor and sp. Mindereri to be continued. 5 P.M.—No improvement; pulse 130. 8 P.M.—Dr Abercrombie again saw him. Erysipelas rising on the leg; vesicles on the instep; tongue much loaded; stools black and unwholesome; prostration continuous. I explained to Dr Abercrombie my mode of treating erysipelas, and obtained his assent to the exhibition of the muriated tincture of iron,—twenty drops were ordered to be given every two hours. A colocynth pill to be taken at bed time. *Midnight*.—Bowels freely moved; two doses of the tincture of iron had been taken; pulse 100, fuller, and more regular; skin hot. The colocynth pills not to be taken until the morning. Cont. Tinct.

22*d*, 8 A.M.—A sleepless night; suffering much from rheumatism in right arm; less pain in the soles of the feet; erysipelas not extending; pulse 100, full and soft; bowels not open; to have the colocynth pills. *Noon*.—With Dr Abercrombie. Tongue cleaner; skin soft; erysipelas diminishing; rheumatism very severe in right arm. To continue the iron, and to have forty drops of colchicum wine three times a-day. 7 P.M.—With Dr Abercrombie. The symptoms more satisfactory; perspiring; *purge*. The improvement continued, and on the 25th the erysipelas had entirely disappeared. The rheumatism was more obstinate, but Mr R. returned home in perfect health on the 6th June.

I have seldom seen more alarming exhaustion than appeared in this case for the first six-and-thirty hours after the attack.

III. 6*th March* 1841.—Mrs W., æt. about 40, of full habit, complains of having a painful boil above the ear; there is also a patch of erysipelatous inflammation extending over the cheek; the ear much swollen. To have fifteen drops of the muriated tincture of iron every three hours; hair powder to be applied to the inflamed surface.

7*th*.—Boil better, but erysipelas extending all over the side of the face. To continue the steel drops.

8*th*.—Erysipelas subsiding. To have an aperient; cont. tinct.

12*th*.—Patient well.

IV. 7*th July* 1843.—Mr A. W., æt. 16, a stout youth, of florid complexion, attacked with fever and headach. Ordered a dose of calomel and antimonial powder, to be followed by a black draught.

8*th*.—Erysipelas appeared over side of the face, with much fever; bowels free. To have ten drops of the muriated tincture of iron every two hours.

9*th*.—Less fever; erysipelas arrested; cont. med.

10*th*.—Disease stationary. The tincture to be increased to twenty drops every two hours.

12*th*.—Considerable improvement.

15*th*.—Erysipelas gone.

V. 10*th December* 1845.—R. S. W., Esq., æt. 50, a delicate man, having had his constitutional strength injured by an accident in early life. I was called to the country to see him, and found him suffering a severe attack of erysipelas on the right side of the head; ear much swollen; pulse 100 and throbbing; great thirst; tongue furred; slight confusion of head; bowels open by medicine. Fifteen drops of the muriated tincture of iron to be taken every three hours.

11*th*.—Very ill; erysipelas extending; severe headach; troublesome dry cough. The chalybeate tincture to be continued; to have a cough mixture and effervescing draughts.

12*th*.—Less confusion of ideas in the morning, but erysipelas extending all over the face, which was much swollen.

Vespere.—Very restless. To have half a drachm of the solution of morphia in an ounce of camphor mixture at bed-time; the tincture continued.

13*th*.—Erysipelas extending, covering nearly the whole head and face. Had not taken the drops regularly; bowels free; the drops resumed.

14*th*.—No improvement; the head an inflamed mass, and much swollen; takes nothing but water; cough troublesome; mind wandering. To continue the tincture, and to have an aperient.

Vespere.—A diminution of the inflammation, and swelling of the side of the head first affected.

16*th*.—No amendment; left side of the head tremendously swollen; very incoherent all day. I remained with my patient during the night, and induced him to take the drops regularly.

17*th*.—A most satisfactory change; quite coherent; pain and swelling of face diminished, although quite a mask of dried exudation. To have an aperient, and continue the tincture.

18*th*.—Watched and administered the drops during the night, and to-day there is a wonderful improvement in all his symptoms. To continue the medicine.

21*st*.—Doing well.

1*st January*.—Has had no interruption to his convalescence, and feels in better health than before his illness.

There was, probably from the state of the patient's mind, much irregularity in the administering the steel drops until I personally watched the case; and, on the 16th, the disease had assumed a very alarming appearance; but immediately upon the medicine being regularly given, so as to bring the system thoroughly under its influence, a most satisfactory change was produced in the symptoms; the fever, and delirium, and pain subsided, calm sleep was induced, and the inflammation gradually disappeared.

VI. 16*th January* 1850.—Miss C. M. seized with pain, inflammation, and swelling, extending over both cheeks and nose. Pulse 100; tongue loaded. A dose of calomel and James's powder ordered to be given at bed-time, to be followed in the morning by hourly doses of the solution of Epsom salts and tart. emetic, until the bowels should be freely moved.

17*th*.—Face more swelled and inflamed; pulse 80; tongue cleaner. To have fifteen drops of the muriated tincture of iron every three hours; hair powder to be applied to the inflamed surface.

18*th*.—Face less painful. To continue the drops regularly, and to have an aperient.

19*th*.—Face entirely encrusted with exudation, having very much the appearance of the advanced stage of confluent small pox; erysipelas not extending; no fever; tongue clean, but much annoyance from cough. To have a cough mixture, and continue the drops. 20*th*.—Improving. 21*st*.—Doing well.

22*d*.—So much better as to be in the drawing-room. The disease happily left no sort of mark on a face of much beauty.

VII. 1*st November* 1850.—W. C., Esq., æt. 73, had been suffering from a severe catarrh and cough for some days. Felt yesterday as if his shoe were too tight for the left foot, and on going to bed the ball of great toe became agonisingly painful; no sleep nor ease in any posture. Ball of toe is much swelled and tender to the touch, having all the character of gout, but the inflammation is erysipelatous, extending over the front of the foot. Pulse 60, irregular; bowels open from medicine; tongue clean. Fifteen drops of the muriated tincture of iron to be taken every three hours.

Vespere.—Mr C. declares that the drops acted like a charm, and at once relieved him from the pain. Redness and swelling continue. To have an aperient, and continue the drops.

2*d*.—Scarcely any pain, although the inflammation is extending. A troublesome cough. The dose of the tincture to be increased to twenty-five drops. To have paregoric for the cough.

3*d*.—Inflammation spreading towards instep. To have an aperient pill, and morphia lozenges for the cough. Continue the drops.

4*th*.—Inflammation fading.

5*th*.—Improving.

8*th*.—Able to go out of doors.

I have in my journals several cases in which erysipelas was combined with gout. Two of these occurred in gouty subjects, and in both the health was restored by the chalybeate, as in the above case of Mr C. This gentleman had, two years ago, an attack of erysipelas in his thigh, and was cured by the steel drops. He never had gout before, but I understand his father had suffered from it.

With regard to the diet of the patient under erysipelas, unless when there is much fever, I always recommend it to be generous, and of easy digestion. In a case at present convalescent, in which the attack was very similar to Mr C.'s, only that while the great toe and foot were violently inflamed, the knee of the other leg was also affected. From the habits of the patient—a constitution broken from intemperance—I was obliged to allow, along with the drops, nearly a bottle of port a-day. In this case the podagral erysipelas has disappeared, and my patient is in better health than he has had for many months.

G. H. B.

ADDITIONAL REMARKS, WITH CASES. BY C. BELL, M.D.

My attention having been early attracted by the success of my brother's treatment of erysipelas, by the tincture of the muriate of iron, I now, after many years' experience of its use, unhesitatingly express my opinion in its favour. I have prescribed it both in the

idiopathic form of the disease and in that consequent on external injury, with the most satisfactory results; and I have found it equally efficacious at every period of life, from early infancy to advanced age. It not only removes erysipelas in a remarkably short time without weakening the patient, but it effects such an improvement in the system, that those who are subject to periodical attacks of the disease are rendered much less liable to have a return.

The beneficial effects of this medicine are so immediate and invariable in the common forms of erysipelas, that I feel convinced were it given with boldness and perseverance in puerperal fever, which is now generally admitted to be analogous in its nature, and frequently accompanied by erysipelatous inflammation on the surface of the body, many valuable lives might be preserved.

In pursuing the chalybeate treatment of erysipelas, I consider it of much importance to bring the system rapidly under its influence, in order to effect a speedy removal of the disease. I have therefore been in the habit recently of giving much larger doses of the tincture than I ventured to prescribe at first. It is a remarkable circumstance in the exhibition of this valuable remedy in the erysipelatous diatheses, that although given in much larger and more frequently-repeated doses than have been recommended in our dispensatories, it never produces headach, and when this symptom is present it quickly relieves it; at the same time, it reduces and regulates the pulse: Thus showing that in this state of the system, it has a soothing and sedative, as well as alterative effect.

The cases I have just read, prove so clearly the remarkable influence which this medicine has in overcoming the idiopathic form of erysipelas in the adult, that I consider it almost unnecessary to increase the length of this paper by producing additional evidence on the subject, although I have had many cases illustrating the fact in my own practice. I am more anxious to direct attention to the advantages to be derived from this mode of treatment in infantile erysipelas, in which the field for curative measures is so very limited; and also to its effects in that form of the disease consequent on external injury, in which the best treatment has hitherto been too often unavailing, especially in our hospitals, and after operations. With this view, I shall now briefly relate a few cases illustrative of the treatment in each of these forms of erysipelas.

On the 27th March 1849, I was requested to see Mrs Munro's baby, of a few weeks old, which was suffering from a severe attack of erysipelas, extending from the wrist to the elbow. I ordered it to have a dose of castor oil immediately, and two drops of the tincture of the muriate of iron, to be given every two hours in a teaspoonful of water, with a little sugar. In three days, the erysipelas had entirely disappeared, and the infant seemed lively and in good health.

Mrs M.'s baby, an extremely emaciated infant, born on the 24th January 1850, was seized a few days after birth with erysipelas,

which occupied the vulva and nates, and was accompanied by severe catarrh and cough. There were much fulness and tension of the lower part of the abdomen, the urine was retained, and the bladder could be felt like a small orange immediately above the pubis. Being naturally weak, it soon became so excessively feeble that no hopes were entertained of its life being preserved, and it was deemed advisable to have it christened without delay.

Small doses of calomel and James' powder were given at short intervals, along with a cough mixture composed of ipecacuan wine, carbonate of soda, water, and syrup. Very little benefit seemed to arise from this treatment; I therefore stopped the calomel and antimony, and ordered two drops of the tincture of the muriate of iron to be given every two hours in a little sugar and water. The infant had become so exhausted, that I was obliged for some time to order, in addition to the tincture, a drop of brandy to be given every half hour.

Under this treatment, the erysipelas and dysuria rapidly disappeared, and the infant slowly gained strength, and is now a lively and healthy child.

Mrs C.'s baby, a fine healthy looking infant, was born on the 27th September 1850, with six fingers on each hand. A few days after birth, erysipelas appeared on the vulva and nates, accompanied with a disordered state of the bowels and frequent green evacuations. Having endeavoured for some time to correct this state of the bowels with small doses of grey powder, rhubarb, and the carbonate of soda, and occasional doses of castor oil, without effect, I ordered two drops of the tincture of the muriate of iron to be given every two hours in a little water and sugar. For some time little progress, however, was made in removing the erysipelas. One day it seemed to subside, and on the subsequent day it was as bad as ever. I was induced to believe that the medicine was not given regularly, and that some days it was not given at all. There was reason also to suspect that the nurse's milk was bad, as I was informed that her own child had been affected in the same manner. I recommended that another nurse should be got, and insisted on the muriated tincture being given regularly. The result was that the erysipelas was removed, and the child's health so much improved, that Mr Syme was enabled, within ten weeks after its birth, to remove the supernumerary fingers with success.

I shall now give a few cases of sympathetic erysipelas, or that consequent on external injury. Mrs D., of broken constitution, received an injury on her nose, which slightly abraded the skin. Erysipelas of the upper part of the face soon followed. She was put on the tincture of the muriate of iron, and in three days the disease was entirely removed.

3d September 1850.—I was requested to see Mr Thomas Ewart, railway guard, who was said to have met with a serious accident. I found him suffering under phlegmonous erysipelas, extending from

the ankle to the knee,—the consequence of a slight bruise he received on his shin about a fortnight previously, and which he had neglected. The usual treatment was adopted, viz., evaporating lotions, poultices, and incisions, along with internal medicines, such as calomel and antimony, with saline draughts; but with no improvement in the leg,—there being much swelling and inflammation, and the wounds had a dry and unhealthy look. For some days I had great fears that the patient would sink under the disease; and my apprehensions of his ultimate recovery were much increased when erysipelatous inflammation appeared above the knee and extended to the hip, occupying the whole of the posterior part of the thigh. There was a considerable degree of fever, pulse 110, flushed face, headach, and severe cough.

Twenty-five drops of the tincture of the muriate of iron were ordered to be given in water every two hours, along with a diaphoretic mixture, containing the solution of the acetate of ammonia and antimonial wine. The bowels were kept open by saline medicines.

The headach and fever soon subsided, and the erysipelas disappeared from the thigh. The leg healed slowly, in consequence of the extensive sloughs which formed round the incisions, and the luxuriant granulations which afterwards formed requiring the frequent application of escharotics. The man ultimately made a good recovery, and returned to his duty in better health than he had enjoyed for a length of time before his accident. I have no doubt had the chalybeate tincture been given at an earlier period in this case, the disease would have been checked in its commencement, and no sloughing would have taken place. I am the more convinced of this from the result of the following case.

On the 4th December 1850, I was called a few miles into the country to see Mr T.'s son, aged about four years. He had always been paralytic in his right leg, in consequence of an accident it was supposed he had met with when an infant. For some time previous to my seeing him, he had suffered from a painful swelling on the right side of the anus, which suppurated, and was opened by the family surgeon, and a large quantity of pus discharged; but the abscess showed no tendency to heal. While playing on the carpet, he slightly bruised the skin of his left leg under the knee, which was immediately followed by erysipelas extending from the knee to the ankle, and accompanied by high fever and rapid pulse.

Small doses of calomel and antimonial powder were ordered to be given every three hours during the day, and a dose of the essence of senna in the morning. In addition to these medicines, to have three drops of the tincture of the muriate of iron every two hours in thin syrup. The leg was dressed with flour and cotton wadding.

5th.—The erysipelas more generally diffused over the leg. To

stop the calomel and antimony, but continue the chalybeate tincture, and keep the bowels open with the senna.

8th.—The erysipelas quite gone, and the original wound under the knee healed. No fever; tongue clean; and the little patient more cheerful than he had been for a length of time.

In conclusion, I shall shortly refer to two cases of idiopathic erysipelas, in which I was consulted some years ago. The one case occurred in a gentleman nearly eighty years of age, who had long been subject to periodical attacks of erysipelas in his legs. I recommended him to take the tincture of the muriate of iron; which removed the attack he was then suffering from, and he remained longer free from the disease than he had done for a number of years. He lived until he was eighty-six years of age.

The second case was that of a gentleman of high talents and acquirements, rector of an academy in the south of Scotland. He had an alarming attack of erysipelas, and was attended by two intelligent medical men, who had adopted all the usual remedies with very little advantage. They, therefore, considered his case hopeless. The erysipelas was extensively diffused over his head and shoulders; there was much incoherence and great debility. Immediately on his system being brought under the influence of the tincture of the muriate of iron, the disease subsided, and he was gradually restored to health. C. B.

ARTICLE II.—*Evidence of Puerperal Fever depending upon the Contagious Inoculation of Morbid Matter.* By Dr F. H. ARNETH, of Vienna.

(*Read before Edinburgh Medico-Chirurgical Society on April 16th,* 1851.)

THERE is, perhaps, no subject in medicine that has excited so much of the attention both of the medical profession and of the public at large, as the epidemic puerperal fever, sometimes prevailing to a fearful degree in lying-in hospitals. So strongly, indeed, has the apprehension of the public been excited, that the entire closure of lying-in hospitals has been repeatedly urged, even by professional men; and some of them have really been shut up for a certain time.

Whilst I am fully convinced that the facts I shall have the honour to communicate to the Society, illustrative of one of the most frequent causes of so dreadful a disease, and of a means preventive of its spreading so fearfully, will prove interesting, I have serious doubts as to my being competent to do justice to the subject in a language with which I am but imperfectly acquainted.

This difficulty becomes still greater, as the outlines of what I have to say to-night are well known in this country, through a very able paper of Dr Routh's.

Having, however, been connected with the Vienna Lying-in Hospital, and thus been enabled to watch the disease, and the manner of its being spread and limited, I hold it a duty to give the subsequent observations all possible publicity.

Before entering on the subject in question, I must apologise for alluding somewhat to the history and the management of the Vienna Lying-in Hospital.

The Lying-in Hospital was opened, together with the "General Hospital," of which it forms a part, in 1784. Its wards being always excessively crowded, the government, by which it is supported, gave orders to build some new wards, and to use them as a second obstetric clinic. Thus, in 1833, the first midwifery clinic took possession of the new wards, leaving the old buildings to the more recently established clinic. I think it proper to observe, that these two clinics form only one large building, their wards being, in fact, so close together, that they are merely separated by a wooden door.

The other arrangements are entirely similar in both establishments. The linen of both is washed together. The two clinics admit pregnant females on alternate days.

In 1839, however, from different reasons not connected with our subject, it was thought advisable that the new wards should be exclusively devoted for the instruction of the male pupils; whilst, in the other establishment, the female pupils should be trained. As, of late, a far greater mortality had prevailed in the Lying-in Hospital than is generally to be met with in private practice, councils of leading men of the profession were held at different times, with a view to ascertain the cause of this fearful amount of mortality.

These gentlemen were struck with the fact that the amount of mortality was, without any exception, far greater in the wards where the medical students pursued their studies, than in those where the midwives were taught. But this was the case only since the year 1839; that is to say, from the time that the medical students and the midwives occupied different wards; the mortality having been nearly equal in former years, when each clinic included among its pupils both medical students and midwives.

This will be exactly shown by the following tables:—

Died in the whole lying-in hospital.	In the second clinic alone.
In 1834, 8·06 per cent.	8· per cent.
„ 1835, 5·33 „	4·98 „
„ 1836, 7·61 „	7·8 „
„ 1837, 8·28 „	6·9 „
„ 1838, 3·75 „	4·94 „

Whereas, since the separation of the pupils took place, there died in the

Clinic of the medical students.	Clinic of midwives.
In 1839, 5·4 per cent.	4·5 per cent.
„ 1840, 9·5 „	2·6 „
„ 1841, 7·7 „	3·5 „
„ 1842, 15·8 „	7·5 „
„ 1843, 8·9 „	5·9 „
„ 1844, 8·2 „	2·3 „
„ 1845, 6·8 „	2·03 „
„ 1846, 11·4 „	2·7 „

The commission generally adverted to the linen being washed without sufficient care, and to the unfavourable situation of the wards, as explanatory of the great amount of mortality; but did not suggest any explanation of the great difference in the relative number of cases ending fatally in the two clinics. At length, in November 1846, it was decided to diminish very considerably the number of young medical men attending the lectures of midwifery, as the opinion prevailed, that the too frequent examinations of the patients, by great numbers of students eager to make the best of their time while in the lying-in hospital, had brought on epidemics of puerperal fever. And, in fact, in the subsequent months of December, January, February, and March, the mortality was much less; but re-appeared, unfortunately, in April, when, in this clinic, 37 died out of somewhat more than 300; and in May, when 36 cases, out of about the same number of confinements, ended fatally. This failure proved, better than the best concocted theory, that the real cause of the fact occupying our attention had not then been discovered.

In March 1847, Dr Semmelweiss was nominated assistant to the professor of midwifery, in whose wards this frightful mortality had prevailed. From the first moment, he was occupied in finding out the cause of the epidemic which it became his lot to watch. As "Time," the best judge of opinions, had so loudly spoken against the views that had hitherto prevailed, it was certainly out of the question to inquire into *them* a second time. His attention was very soon drawn to the management, both as to dietetics and medical treatment, in the neighbouring clinic, which had to boast of results comparatively so favourable. But, to his amazement, he found that there neither was the ventilation better carried on than in his own wards, nor had any particular plan of treatment been resorted to. At length, however, he was struck with what was very likely the sole difference to be found in the management of the two clinics, namely, the pupils of the one being midwives, those of the other medical students. The latter were, almost without exception, in the daily practice of assisting at autopsies, of which eight or ten took place in our large hospital almost every day. The dissections were sometimes executed by the students, or they handled at least

the pathological preparations, and examined them carefully. Moreover, the assistant used to lecture on the obstetrical operations. These were performed on dead bodies, and, of course, sometimes repeatedly. Now, after such investigations and such practice, it was not rare to see the students going immediately to the wards of the lying-in hospital, and examining the pregnant and parturient women.

It is scarcely necessary to state, that the pupils of the other clinic, being midwives, did not take any share in the occupations just alluded to; nay, even the assistant of that clinic had comparatively seldom to deal with post-mortem examinations, as it was not a part of his duty to give instructions to the midwives in pathology or operative midwifery.

Having convinced himself that the great prevalence of the disease in his wards was caused by the inoculation of the female genitals, Dr Semmelweiss began to entertain a hope of being able to diminish this frightful mortality.

Every one who has been engaged in post-mortem examinations must be aware of the very disagreeable smell, which it is impossible to get rid of, even by the most careful washing. Of course this smell which remains about the hands is more penetrating and more lasting the more dissections have been made at a time. The existence of any smell can only be dependent on a substance detained on the epidermis, and is still sensible as long as there remains the least atom of that substance. It is impossible not to recollect how actively the process of absorption, generally speaking, is carried on in the pregnant female.

The opinion of Dr Semmelweiss on this important matter is as follows: *Any fluid matter in a state of putrefaction*[1]—communicated by linen, by a catheter, by a sponge, by small particles of the placenta, even by the ambient atmosphere impregnated with the foul substances—*may produce puerperal fever.*

If these ideas are admitted, it appears very likely that washing the hands with some powerfully cleansing fluid must destroy the smell, and remove the morbific particles from the hand.

After having tried various substances, he was latterly in the habit of prescribing for that purpose solution of chloride of lime as the cheapest.

In the course of the month of May 1847, it was arranged that no one should examine any woman in the clinic without previously having washed his hands with solution of chloride of lime, and having made use of a nail brush.

Even in June, it was impossible not to be sensible of the influence of this precaution. Out of more than 300 women that were confined in that month, only six died; in July, three out of about

---

[1] I think every morbid matter undergoes a degree of putrefaction, after it has been discharged from the body for any length of time.

the same number; in August, three; in September, twelve; October, eleven; November, eleven; December, eight: whereas, in April, fifty-seven; and in May, thirty-six, cases had ended fatally. In the year 1848, the mortality amongst the puerperal women delivered by male pupils was 1 in 84; whilst, in the second clinic, amongst the women delivered by midwives, it was 1 in 76.

Since the year 1827, the rate of mortality in the hospital had never been so low. I need not say more than that, from the time when this precaution was introduced into the practice, up to the time of my leaving Vienna in November last—that is to say, for more than three years—the mortality in the two clinics has always been nearly the same, which had never been the case since 1839—that is to say, since the time when the male and female pupils were exclusively confined to separate clinics.

A very similar observation was made at Kiel. Dr Michaelis, professor of midwifery in the university of that town, wrote a letter to Dr Semmelweiss, bearing the date March 18th, 1848, in which he informed Dr Semmelweiss that they had been obliged, in 1847 (July 1), to shut the lying-in hospital because of the puerperal fever spreading fearfully. In November they opened the doors again, but a very short time afterwards the disease began again to make its appearance—so that they had already come to a determination to close the house for a second time, when the news of Dr Semmelweiss' opinions, and the measures adopted in our lying-in hospital, reached Kiel (December 21st, 1847). From that time, up to the 18th of March, they had no case of puerperal fever excepting one, that was caused, as Dr Michaelis believes, by a dirty catheter.

I must call your attention to some more facts which, as it appears, speak strongly in favour of the opinions of Dr Semmelweiss.

In the course of those years when the post-mortem examinations were less frequent with us in Vienna, owing to the little importance attached to pathological anatomy, the puerperal fever was comparatively seldom met with; and this was certainly not explicable by the wards being less crowded than at present, as the one clinic of those former times contained—in comparison with its room—nearly as many women as the *two* clinics do now. You may recollect that I have stated before that when first the second clinic was established, its pupils included both midwives and medical students, and that in these days the mortality in its wards was not less than in the other clinic. It was only after the separation of pupils already mentioned, that the reports of the second clinic became much more favourable than those of the first clinic. They remained constantly so; with one striking exception, however, occupying the period between 15th October 1841, and 15th October 1843, when the mortality was much higher than usual, the then assistant being very busy in anatomical pursuits.

According to the reports of the lying-in houses in the whole empire of Austria, in none of those institutions where midwives have

been the only pupils has a puerperal epidemic made its appearance; but it reigned obstinately in Pavia, where they used to dissect—in one of the rooms of the lying-in-hospital—the bodies of the children who died in the hospital.

There existed, within the last few years, two clinics in Strassburg, as closely adjacent as those in Vienna, and divided on the same plan in reference to the pupils. The professors of Strassburg told me that they had always less mortality in the clinic to which the midwives were admitted, and that there was no instance of any epidemic in those wards.

While in search of the true cause of the prevalence of puerperal fever, the impression on Dr Semmelweiss was unfortunately at first exclusively directed to the influence of post-mortem examinations, and the precautions that were already in practice were not resorted to in the case of a pregnant woman who was admitted into the lying-in hospital labouring under cancer of the uterus. As several days elapsed before her confinement was over, and as the case was highly interesting, of course every one wished to examine her. The consequence of this was most deplorable. Fourteen mothers who had been confined at the same time with this woman, and who had been examined by the same students, exhibited puerperal symptoms, and three of them died, although puerperal fever had not been prevailing immediately before, nor did any other case occur except these fourteen.

I think there can scarcely be any doubt as to the mode in which the matters of putrefaction act on the system. All the symptoms that are so often observed in puerperal fever, and the more so the greater the number of patients at a time is, show clearly that it is phlebitis, ending in purulent infection, with which the women are seized. The rigors, the shivering, that prevail as the first symptoms, the dirty yellow discoloration of the skin, the metastatic deposits that are collected in the cellular tissue and in the articulations, are ascribed by most of the very eminent writers on the subject to the purulent infection; and every accoucheur is aware, that those are also the symptoms of the most hopeless cases of epidemic puerperal fever. But allow me to observe that, as far at least as I am aware, purulent infection is never met with except in cases where the purulent matter comes in immediate contact with the blood, as is observed in punctures inflicted in dissections, that present quite similar symptoms, and end unfortunately too often in a similar way.

If the discharges of the uterus and the vagina were the true causes of puerperal fever in most cases, it would be decidedly very difficult to say why they did not infect the women in both clinics of the Vienna hospital with equal frequency.

Before concluding, I have only to express a hope that most of the great variations of mortality may be avoided in future by the greatest care after dissections, and by attention to cleanliness, the air of the wards, the instruments, linen, &c.

It is, however, my firm conviction that any individual who may not choose to submit to what we have stated to be necessary after the contact with matters in putrefaction, may artificially produce the puerperal fever on many persons whom he may successively examine, and who, in all probability, would never have become ill but for this inhuman carelessness.

Allow me, in conclusion, to state, that I know of no case of the puerperal fever having been communicated through the clothes of a medical man to a patient. As far as I know, there is no such case on record, neither in hospital nor in private practice in Vienna, where it had been possible to trace a puerperal fever to such a source. As to erysipelas, which I am aware is very often admitted in this country to produce puerperal fever, I feel myself bound to aver that erysipelas has by no means been most frequently observed in the Vienna hospital when we had epidemics of puerperal fever, nor did we find the children of the puerperal women seized with erysipelas. Scarcely one of our pupils, and none of the nurses, were taken with erysipelas during my stay in the Vienna hospital.

I have perused a good many of the histories of puerperal fever detailed in English periodicals and standard works, and I think nearly all the cases brought forward to maintain the possibility of bringing on puerperal fever by touching erysipelas, relate to cases of gangrenous erysipelas; the mode of propagation of which it would be very easy to explain by the views spoken of at length in this paper.

I hope the high importance of the question will serve as my excuse for asking you whether the members of this distinguished Society are aware of any cases of puerperal fever brought on by touching common erysipelas, or erysipelas without discharge. Concluding, however, from the different appearances shown by some diseases, such as typhus fever, in different countries, I think it quite possible that such an affinity between erysipelas and puerperal fever may exist in this country, without our having occasion to make the same observation on the Continent.

ARTICLE III.—*Electro-Biological Phenomena Physiologically and Psychologically Considered.* By JAMES BRAID, M.R.C.S. Edinburgh, &c. &c.

(*Lecture delivered at the Royal Institution, Manchester, March* 26, 1851.)

IN the following essay, I shall endeavour to submit to your consideration, in the most simple and comprehensible manner, the results of my experience, whilst investigating one of the most recondite and interesting departments of philosophy and science,—namely, the reciprocal actions upon each other of mind and matter.

Most of you have no doubt seen or heard of the experiments in

what has been designated "Electro-Biology, or the Electrical Science of Life," which were lately exhibited in Manchester, Liverpool, and the surrounding towns, and which are now being exhibited in London, by an American gentleman named Mr Stone. The following announcement is contained in one of his hand-bills, of the 12th inst., as to what he professes to exhibit in London, at the Marylebone Literary and Scientific Institution :—

"Persons in a perfectly wakeful state, of well known character and standing in society, who come forward voluntarily from among the audience, will be experimented upon. They will be deprived of the power of speech, hearing, sight. Their voluntary motions will be completely controlled, so that they can neither rise up nor sit down, except at the will of the operator; their memory will be taken away, so that they will forget their own name and that of their most intimate friends; they will be made to stammer, and to feel pain in any part of their body at the option of the operator—a walking stick will be made to appear a snake, the taste of water will be changed to vinegar, honey, coffee, milk, brandy, wormwood, lemonade, &c. &c. &c. These extraordinary experiments are really and truly performed without the aid of trick, collusion, or deception, in the slightest possible degree."

These may no doubt be considered as startling announcements; nevertheless, I know as a fact, that such phenomena, and the very reverse effects, may be realised in certain individuals; but I account for them in a very different manner from the so-styled Electro-Biologists.

A similar class of phenomena have also lately been exhibited in Edinburgh, Glasgow, and various parts of Scotland, by another American named Dr Darling. These exhibitions have excited great interest at Edinburgh and Glasgow, and have enlisted in the discussion the pens of Sir David Brewster, Professor Gregory, Professor J. Hughes Bennett, Mr Robert Chambers, and other gentlemen of the highest character and scientific attainments. Amongst the contributions which have been published on the subject, I have been quite charmed by an article which appeared in the Number of "Chambers' Edinburgh Journal" for the 8th of last month, entitled "Experiments on Electro-Biology." The analysis therein given of the nature, causes, and tendencies of scepticism in mankind generally, and in certain coteries in particular, in reference to probationary sciences, is an admirable picture by a first-rate artist, which merits the candid study of the whole human family. The writer of the article alluded to had been a decided sceptic, and had accompanied a gentleman of his acquaintance, who was equally sceptical as himself, to a *seance* in a private family, where most of the company assembled consisted of persons in the upper ranks of society. The said friend offered to submit to the trial on himself, for the purpose of proving the fallacy of such pretensions; however, the result was very different from his anticipations, for the proud sceptic and derider was converted into

a complete victim. Two ladies of the party, also well known to the writer of the article, were likewise proved to be equally susceptible. Collusion and fraud being out of the question in all these cases, the writer was constrained, through the force of such unexceptionable evidence, to renounce his scepticism. From what has been stated by Professor J. Hughes Bennett, it appears that several members of the Medical Society of Edinburgh, who were most sturdy sceptics, were also victimised upon submitting to such trials. In short, there is now abundance of evidence of the fact that such phenomena may be produced in perfectly good faith in some subjects, in the waking condition, and consequently the chief point remaining to be determined is the *cause* of these remarkable phenomena.

The ingenuous and manly confession of the learned author regarding the scepticism of himself and his friend, and the interesting narrative of the immediate cause of their conviction as to the reality of certain phenomena manifested at the *seance* depicted by him, is just a transcript of my own feelings and experience in this department of science. I beg, therefore, to submit the following remarks for your consideration, with the hope that they may tend to the elucidation of truth, and the overthrow of that unreasoning, uninquiring, dogmatic scepticism which Chambers' correspondent has so ably combated.

Nine years ago I entered upon the experimental investigation of mesmerism, believing, from what I had read and heard of it, that the whole was a system of collusion or illusion. I very soon discovered, however, that there was a reality in some of the phenomena, notwithstanding I had reason to differ from the mesmerists regarding the cause. My experiments proved that similar phenomena, of abnormal sleep and peculiar condition of mind and body, might be self-induced, by the patient's maintaining a steady fixed gaze at any inanimate object, the mental attention being concentrated on the act. This at once proved the *subjective* or *personal* nature of the influence, and that it did not arise from the transmission of any magnetic or occult influence passing from the operator into the patient, which the mesmerists contended for. The potency of this method was proved by me at a public lecture in Manchester, before an audience of about eight hundred individuals. After explaining my views or theory, and exhibiting the phenomena on patients who had previously been subjected to such operations, I requested strangers, who had never been operated upon, to come forward and try the effect of my process. Fourteen male adults came forward, all of whom were entirely strangers to me. Some of them were desired to keep a steady fixed gaze upon the end of a cork, bound on the head, so as to project from the middle of the forehead, each to look at the cork bound upon his own head, and to fix his undivided attention on the act. Some of the others were desired to fix their sight and thoughts upon a part of the gas apparatus in the room. All commenced the process at the same time, and ten of the four-

teen went into the sleep, and that whilst I never touched any one of them, until after their eyelids closed involuntarily. None of them seemed able to open their eyes, although they remained conscious; some became cataleptic; others were insensible to the prick of a pin, and one or two forgot all which had occurred. One, who remembered nothing of what had happened during his sleep, was a powerful mechanic, who was sent down, bribed by a medical man, to resist me, and this he tried by not complying with my conditions; but, nevertheless, when I at length signified that I observed he was acting falsely, he set about it with a look of grim defiance, when he was speedily caught, and became one of the best examples of the power of my process that evening. Another, a most intelligent gentleman, was so sceptical, that before coming down he said, "*seeing* was *not* believing" *with him*, but that he must *feel* it before he would believe it,—he became a beautiful example, and after being released, described his feelings to the audience. During these proceedings three more of the company sent themselves into the condition, by fixing their gaze and thoughts upon points in the room, according to what I had indicated in my lecture as sufficient to produce the sleep. I knew nothing of their acts or intentions until called upon by their friends to unlock them from the profound sleep into which they had fallen, and from which they could not succeed in arousing them.

I was equally successful in operating upon a number of strangers together at a private conversazione, given to the profession in London, in March 1842, sixteen out of eighteen having passed into the sleep, simply by maintaining a steady fixed stare and fixed act of attention, whilst gazing at the root of a chandelier. Most of these had never been so tried before. I never touched any one of them until their eyelids closed. Mr Herbert Mayo, the eminent physiologist and surgeon, tested them, and ran a needle from the back to the palm of the hand of one patient without his (the patient) evincing the slightest consciousness of pain, or remembrance of it after awaking. Entrancing a fowl, by holding its bill to the floor or a table, so as to cause it to gaze at a chalk line or stripe of coloured paper placed before it, is also a familiar example in point. From these facts, and innumerable others which have occurred to myself and to those who have adopted my views and mode of operating; and also from the practices of the Fakirs of India, who, for religious purposes, have been accustomed for the last 2,400 years to throw themselves into the state of ecstatic trance by similar processes,—viz., by each looking stedfastly at the point of his nose, or some other part of his own body, or at some inanimate or ideal object, as one of their gods, and fixing their undivided attention on the act,—I say, inasmuch as similar results arise from *all* these *personal acts*, there can be no reason to doubt that the influence is essentially *subjective* or *personal*, and that the subsequent results are merely modified or intensified by others, by suggestions or impressions conveyed to the subjects through words spoken within their hearing, and other physical

impressions acting on their organs of special sense. The condition is essentially one of mental abstraction or concentration of attention, in which the powers of the mind are engrossed, if not entirely absorbed, with a single idea or train of thought, and concurrently rendered unconscious of, or indifferently conscious to, all other ideas or impressions. In this condition the imagination becomes so vivid as to invest with present reality whatever idea arises spontaneously in the mind, or is suggested to it by another person on whom their attention has become specially fixed; and the oftener these phenomena are excited, the more easily may they be reproduced through the laws of association and habit. Moreover, patients who are naturally highly susceptible of such impressions may at length become so impressible as to be liable to lapse into the condition entirely through the force of imagination and fixed belief that some process is going forward elsewhere, which is competent to throw them into the state; and this may happen without any process whatever being in operation at the time. This is, no doubt, a grand source of fallacy with those who allege, through occasional coincidences of trials and results, that they possess the power of influencing patients at a distance, through mere volition or secret passes.

It is an undoubted fact, that there is great difference in the susceptibility to the mesmeric impression, some becoming rapidly and intensely affected, others slowly and feebly so. This want of uniformity in the results has been seized upon by sceptics as a warranty for their suspecting the whole phenomena manifested to be a tissue of collusion or illusion,—whereas, when viewed in the light in which I advocate the science, as the result of a *subjective* or *personal* influence, we ought naturally to expect as great difference in the results as there are differences in the mental and physical constitutions of the patients subjected to the processes. Upon this principle, therefore, I contend that the *variety* manifested by *different individuals* is the strongest proof of the *genuineness* of the phenomena.

In the ordinary mesmeric state, where the eyes are closed and the patient passes into that condition called the double-conscious state,—that is, a state in which the patient forgets, when aroused from the sleep, all which had occurred during that condition, but of which he recovers a perfect recollection when he passes into the sleep again,—it is generally found that the mind of the subject is very much in the same condition as in ordinary dreaming, every idea arising in the mind spontaneously, or suggested to it by words spoken in his hearing, or by other impressions made upon his organs of special sense, being instantly accepted by him as truths, and invested by him with all the force of present reality. In this state the imagination seems to assume a supremacy over the reason, will, and natural functions of the organs of special sense, and the concentrated state of mind enables them readily to be imposed upon, and induced to act out their dreams, excited by such suggestions, precisely in the manner referred to by Dr Abercrombie, in the case of the officer

whom his companions could converse with in his sleep, and convince of anything; and whom they even conducted through the whole process of a duel, till the firing of the pistol awakened him by its report. On another occasion, being asleep on the locker of the cabin, he was made to believe that he had fallen overboard, and was told to save himself by swimming. He imitated the act of swimming, when they told him to dive for his life, as a shark was pursuing him, which he attempted so energetically that he threw himself from the locker, by which he bruised himself severely.

The sense in action may through suggestion be rendered inordinately active, or its natural function may be temporarily suspended, by mental impressions strongly imprinted on the minds of the subjects by repeated and appropriate suggestions. In short, in this condition the mind seems ready to accept as truth the first suggestion; and, from its abstractive tendency, becomes so absorbed with that as to be dead, or comparatively indifferent, to all else during the dominance of that idea or train of thought. We see an analogous condition in individuals noted for absence of mind; indeed, in a less degree, it must have been observed by most people in their own persons, as they may have been so engaged in a train of thought as not to observe a passing friend, or have had any distinct apprehension of conversation going on around them.

With most patients, so long as they remain in that condition in which they can remember on awaking what had been said and done during the sleep, there remains a degree of reason and volition sufficient to enable them to distinguish true from false and merely suggested ideas, and to enable them, moreover, to resist complying with whatever they do not approve of. There are some subjects, however, who, *even in this stage*, are so imaginative and abstractive, possessed of such easy credulity and passive obedience to the will of others, that they can be subjugated and controlled as those last referred to when in the *deeper* state; and indeed not a few of them can be so influenced and controlled by suggestions of others, whilst wide awake and remaining perfectly conscious. These were the subjects which used to manifest what were called by the mesmerists the "vigilant or waking phenomena," and were precisely similar to what have been exhibited by Dr Darling in Edinburgh, and lately in this quarter by his countryman and friend, Mr Stone, under the new designation of "Electro-Biology."

It will be observed, that the processes resorted to by these Americans is merely a variety of my old process for inducing what I called the hypnotic or mesmeric state. The zinc and copper are merely visible and tangible objects for aiding the patients in fixing their attention, and inducing that state of mental abstraction which is the real origin and essence of all that follows. I know, from experience, that any other object would be equally efficient, provided the patients were impressed with that conviction. All in which their processes differ, therefore, from what I had been accustomed to do for

more than nine years past is, in beginning, to make mental and physical impressions on the patients *before* the subjects close their eyes and pass into a state of sleep; and it will be shown presently that I had investigated the subject in *this* mode also, and published narratives of cases, in July 1846, precisely similar to those of Dr Darling and Mr Stone. I explained the phenomena differently, however, and when my *mental* theory has been propounded, shall leave it to others to decide which theory is preferable, the *mental* or the *electric*. The auricular or audible suggestions and other impressions resorted to by these experimentalists (for they speak aloud, within the hearing of the patient, the idea or act which they wish and intend to be manifested, or give some visible signal, or a tangible impression, to excite ideas or actions in their subjects) are merely modes of influencing the minds of their patients, and of reflecting the habit of concentrated attention, superinduced by the first process, from one idea or purpose and fixing it on another; and the expression, by the experimentalist, of "all right," or a nod of his head, or other manœuvre for dissipating the existing delusion, are merely different modes of breaking down the previously existing ideas, just as a tap upon the shoulder will restore to consciousness of all around the individual who was so wrapt in mental contemplation as to have passed his friend without recognition, or been insensible to what was being said or done in his presence previously. Now, the repetition of these audible, visible, and tangible suggestions being requisite to insure the result, is a clear proof that the influence is *psychical* or *mental* in its nature, and that it is not a *physical* or *electric* influence, as they allege. Were it *electric*, there ought to be no more need for their auricular suggestions and manœuvres to produce the results on their patients than there would be for such to insure the one side of a Leyden jar being charged negatively with electricity when the other is being charged positively, or for the attendant to speak aloud to the electric telegraph the message which he wishes it to convey. The few *occasional* examples adduced by them in which they allege that they can convey their behests to subjects *silently and unseen*, through *pure sympathy* and power of their will, I consider inadequate to sustain their hypothesis. At best they are but exceptional cases; and where I have had opportunities of testing such patients, I have soon been able to demonstrate the sources of fallacy which had misled the operators.

It is a well established physiological fact, that the moment the trunk of a nerve is divided, all sense and voluntary motion is abolished in parts supplied by such nerve beyond the point of section. Now, are we to believe that it could be possible that such a degree of sympathy could exist between different individuals as to enable one, by the exercise of his volition, or other silent and unseen manœuvres, to force his own nervo-vital influence beyond his own organism, so as to control the acts of the other at a distance, even when miles apart, when he could not propel it a fraction of a line beyond

the section of the nerve in his own limb (the divided ends of the nerve being in close apposition), so as to produce a voluntary movement of his own member?

In July 1846, I published a little brochure, entitled "The Power of the Mind over the Body." The object of that work was to point out what appeared to me an important source of fallacy, which had been overlooked, or not sufficiently attended to, by Baron Reichenbach when instituting experiments to prove the existence of a new imponderable, which he has designated the Od, or Odyle force. I most readily admit that Baron Reichenbach's experiments were carefully conducted, and well contrived for determining *merely physical* facts; but there seemed to have been a want of due consideration given to the very important part which the *mind* of the *patient* plays in such experiments, in producing or modifying results, quite irrespectively of external influences. The only test or proof of this alleged new force was certain effects produced on the human nerves of some highly nervous subjects. Now, my experiments went to prove that precisely similar phenomena might be realised in such highly sensitive subjects as the Baron succeeded with as the mere result of sustained mental attention of the patient alone, changing the physical action of the part so regarded, especially when done with the expectation of something being about to happen. These effects might be increased in intensity by the subject seeing or feeling anything drawn over a part, from the visible or tangible object aiding the subject in concentrating his mental attention, without the transmission of any occult or external influence, proceeding from the object or operator to the subject. Of this there can be no doubt, as the results of my experiments, which I shall now quote from the above work, will prove.

"With nearly all the patients I have tried, many of whom had never been hypnotised or mesmerised, when drawing the magnet or other object slowly from the wrist to the points of the fingers, various effects were realised, such as a change of temperature, tingling, creeping, pricking, spasmodic twitching of muscles, catalepsy of the fingers, or arm, or both; and reversing the motion was generally followed by a change of symptoms, from the altered current of ideas thereby suggested. Moreover, if any idea of what might be expected existed in the mind previously, or was suggested orally, during the process, it was generally very speedily realised. The above patients being now requested to look aside, or a screen having been interposed, so as to prevent their seeing what was being done, and they were requested to describe their sensations during the repetition of the processes, similar phenomena were stated to be realised, even when there was nothing whatever done, beyond watching them, and noting their responses. They believed the processes were being repeated, and had their minds directed to the part, and thus the physical action was excited, so as actually to lead them to believe and describe their feelings as arising from *external* impressions."

Immediately after my little work on "The Power of the Mind over the Body" was published, Dr Henry Holland,—a very competent authority in such matters,—wrote to me to say, that my experiments and comments were so satisfactory to his mind, that he considered no farther refutation of Baron Reichenbach's speculations about the Od force was necessary. I certainly had been enabled clearly to prove that the *mind* of the patient *alone* was adequate to produce the effects attributed to odyle; and I had also been enabled to prove that, with such subjects, ideas *audibly suggested* by a second party could speedily produce the like results; but I have never yet seen any specific influence from *silent* willing, either when near or at a distance, *when all sources of fallacy were duly guarded against.* It is now nearly five years since that little work was published, and I am not aware that my objections to the validity of Reichenbach's alleged discovery have ever yet been satisfactorily answered.

Again I quote from the above work:—"A lady, upwards of fifty-six years of age, in youth a somnambulist, but now in perfect health, and wide awake, having been taken into a dark closet, and desired to look at the poles of the powerful horse-shoe magnet of nine elements, and describe what she saw, declared, after looking a considerable time, that she saw nothing. However, after I told her to look attentively, and she would see fire come out of it, she speedily saw sparks, and presently it seemed to her to burst forth, as she had witnessed an artificial representation of the volcano of Mount Vesuvius at some public gardens. Without her knowledge, I closed down the lid of the trunk which contained the magnet, *but still the same appearances were described as visible.* By puting leading questions, and asking her to describe what she saw from *another* part of the closet (where there was nothing but bare walls), she went on describing various shades of most brilliant corruscations and flame, according to the leading questions I had put for the purpose of changing the fundamental ideas. On repeating the experiments, similar results were repeatedly realised by this patient. On taking this lady into the said closet, after the magnet had been removed to another part of the house, she still perceived the same visible appearances of light and flame, when there was nothing but the bare walls to produce them; and, two weeks after the magnet was removed, when she went into the closet by herself, the mere association of ideas was sufficient to cause her to realise a visible representation of the same light and flames. Indeed, such had been the case with her on entering the closet ever since the few first times she saw the light and flames. In like manner, when she was made to touch the poles of the magnet when wide awake, no manifestations of attraction took place between her hand and the magnet; but the moment the idea was suggested that she would be held fast by its powerful attraction, so that she would be utterly unable to separate her hands from it, such result was realised; and on separating it, by

the suggestion of a new idea, and causing her to touch the *other* pole in like manner, predicating that *it* would *exert no attractive power* for the fingers or hand, such negative effects were at once manifested. I know this lady was incapable of trying to deceive myself, or others present; but she was self-deceived and spell-bound by the predominance of a pre-conceived idea, and was not less surprised at the varying powers of the instrument than others who witnessed the results."—Pp. 19-23.

After detailing a number of experiments on patients in the waking condition, I adduced the following:—"In like manner, several other patients whom I took into the dark closet could see nothing until told to look steadily at a certain point, and they would see flame and light of various colours proceeding from it, which predictions were speedily realised, whilst they were wide awake, and nothing but bare walls towards which to direct their eyes. Not only so, but I have moreover ascertained that, even in broad daylight, a strong mental impression is adequate to produce such delusions with certain individuals of a highly imaginative and concentrative turn of mind. This fact was beautifully illustrated in the case of a gentleman, twenty-four years of age, who had suffered severely from epilepsy for eleven years. When taken into the above closet, and tested as the latter, he likewise saw nothing till I suggested that he would see flame and light, after which prediction he very speedily saw it accordingly, not merely where the magnet was, but also from other parts of the room. Now this patient, and the last two referred to, when taken into the closet *after* the magnet had been a long time removed to a distant part of the house, still saw the flames and changing colours as before,—a clear proof that the whole was a mental delusion, arising from an excited imagination on the point under consideration, changing physical action. The same gentleman, being made to look at the point of a piece of brass wire, could be made to imagine that he saw any sort of flame or colour indicated issuing from it, *even in broad daylight;* and when made to touch it with a finger, and being then told that he would find it impossible to draw it away, the mere idea was sufficient to paralyse his volition, the whole muscles became rigid, and he looked with astonishment at his condition; but the moment I said, *Now the attraction is gone, and his hand will separate,* such results followed. Moreover, now that his finger was a little withdrawn, by simply saying confidently that it would *now* be found that he could not touch the wire, as it would repel him, the idea once more paralysed his volition, and he again manifested his incapacity, and in spite of his anxious but misdirected efforts, there he remained fixed as a statue. On hinting that *now* the influence was suspended, the hand and arm became limber, when I told another person watching the experiment, that *now* he would find the hand irresistibly drawn to the wire, and such result was presently manifested. No one had touched this wire for hours. It was merely a piece of bent brass wire, which was lying loosely and projecting from

the chimney piece. This power seems to have been understood by Virgil, when he said,—

'Possunt, quia posse videntur.'

In like manner, having intimated to a friend the remarkable vividness of this patient's imagination, implicit belief, and credulity, which rendered him liable to believe that he had an ocular perception of an external change, according to whatever idea might be suggested to him by others, I requested this friend, when he went into the room, to look at the end of the above wire at the same time with the patient, and that the former should pretend to me, when asked what coloured flame he saw emanating from it, to give a new idea at each inquiry. By this mode the patient caught the ideas suggested, having no notion that he was deluded in the manner indicated. He left with the full conviction of the physical reality of all he had seen and described; and he has manifested like phenomena as frequently as he has been so tested.

"I have detailed the above case so much at length, because it is a very good type of a class of patients to be met with, who readily become the dupes of suggested ideas, in the manner presently to be explained, without the least desire to deceive others, or the most distant idea that they are themselves deceived. I have proved all I have advanced by so many concurrent examples, with individuals of the utmost probity and competency to describe their feelings, that there can be no doubt of the facts.

"But not only may patients in the waking state be made to believe that they see various forms and colours, and perceive variable and sensible impressions, and irresistible powers, drawing, repelling, or paralysing them, from a strong mental impression changing the physical action of the organ, or part usually engaged in the normal manifestation of such functions; but I have, moreover, ascertained that the same influence may be realised in respect to sound, smell, taste, heat, and cold,—so that suggested ideas and concentration of inward consciousness, are competent, with some individuals, to excite ideas not merely of hearing vague sounds, but particular tunes, the smell of particular odours, and to discriminate particular tastes, and feel heat or cold. All this, I have proved, may be realised with some excitable subjects when they were wide awake, and when there was neither actual sound, nor odour, nor taste in the situations or substances to which they referred; and by merely asking what tune, what odour, what animal, or what substance they perceived *now* (a mode of interrogation which naturally suggests the idea of change), I clearly proved that ideas may be thus excited in the minds of subjects totally different from those existing in my own at the time. The subjects with whom I made these experiments were worthy of implicit credit as to their integrity in describing their feelings and belief; and the whole results, therefore, are attributable to the re-

markable reciprocal action of the mind and body upon each other, to which I have so often referred. Indeed, one of the most beautiful examples I have had of these 'vigilant phenomena,' in respect to *all* the senses, occurred in the case of a gentleman of high classical and mathematical attainments, as well as in general science. He had seen no experiments of the sort before I tested him. On finishing my round of experiments with him, he begged of me to explain the *rationale* of what had occurred. I requested him to read what I had written on the subject, which he perused with great attention, after which he expressed himself perfectly satisfied that I had hit upon the true solution of the problem. Indeed, he was so kind as to authorise me to refer any one to him for a confirmation of the *rationale* I had given of the phenomena, as experienced by him in his own person, when wide awake, and in the bright light of day."—Pp. 21–24.

I may here observe, that when he was in Manchester, Mr Stone requested a mutual friend to introduce him to me. At our interview I read to him the foregoing extract from my work, when he expressed his surprise that he had not before been aware of experiments so entirely similar to his own having been made and published in this country. He admitted that my explanation was quite satisfactory where audible suggestions were required, but still wished to hold to the electric theory, as the only mode of explaining their alleged power of conveying their behests and feelings to patients silently and unseen, through pure sympathy or silent willing at a distance,—phenomena, the actual existence of which I very much doubt.

Again,—"The true cause of these 'vigilant phenomena' is not a physical influence from without, but a mental delusion from within, which paralyses their reason and independent volition; so that, for the time being, they are mere puppets in the hands of another person, by whom they are irresistibly controlled, so that they can only see, or hear, or taste, or feel, or act, in accordance with his will and direction. They have their whole attention fixed on what may be said or signified by this alleged superior power, and consequently perceive impressions through the excited state of the organs of sense called into operation, which they could not perceive in their ordinary condition. It may have been interesting enough to have demonstrated that the human mind could be so subjugated and controlled; but I do not consider the *continual repetition* of such experiments in the waking condition as at all proper, or free from the danger of throwing the faculties of the minds of such patients into a permanently morbid condition."—*Power of the Mind over the Body*, pp. 33, 34.[1]

---

[1] Since this lecture was delivered, which was on the 26th March 1851, I have been much gratified by the perusal of two admirable lectures on the subject, by Professor J. Hughes Bennett of Edinburgh. In these lectures, that acute physiologist and accomplished physician advocates essentially similar views to those

The processes, however, when prudently and skilfully conducted, are perfectly harmless, and are, moreover, capable of being turned to the happiest results in the relief and cure of several affections which are most intractable, or altogether incurable, by the ordinary modes of treatment, by the exhibition of medicine. In this manner we can excite or depress the state of sensation and circulation locally or generally, and consequently the function of any organ or part, in a most remarkable degree, according to the mode of procedure and mental suggestion resorted to in each individual case,—so that, in this way, morbid delusions and diseases may not only be excited but cured in many instances, not merely as regards the faculties of the mind, but also as regards the functions of the body. Whatever function can be paralysed or perverted by a given mode of procedure, and suggestion, and manipulation, may be invigorated and corrected by an opposite mode of management. This is still more remarkably verified in patients who pass into the sleep.

I am as well aware as any sceptic can be, that many of the phenomena manifested by such subjects, both when asleep and in the waking condition, are capable of being feigned. Indeed, like the correspondent in "Chambers' Journal," I set about the experimental investigation of the subject with the full conviction that there was imposture, and with the determination, moreover, if possible, to discover and expose the *trick*. With such evidence as presented itself before me, however, of the genuineness of the phenomena, as exhibited in parties known to me to be possessed of the strictest probity, and honour, and christian principles, it became impossible for me, with a proper regard for candour and honour, to persist longer in a course of dogmatic scepticism. I therefore confessed that my former notions had been erroneous, and set about the further inves-

---

which I have so long contended for. Like a true philosopher, Dr B. interrogated nature by experimental observation; and, in reference to the doctrine of *external influence* as the *cause* of the phenomena, he says,—"I know of no series of well-ascertained facts capable of supporting such a doctrine." Regarding the ascertained facts, the Professor adds, that they "are highly important, and demand the careful consideration of the physiologist and medical practitioner." To the same effect are the following observations of Dr Henry Holland of London, in a letter, bearing date April 26, 1851, addressed to me by that highly accomplished physician and able writer. After acknowledging receipt of a copy of a synopsis of this lecture, he says,—"You take the true course in regard to both mesmerists and biologists in keeping ever before you the main question, Is there any unknown influence, *communicated from A to B*, by which the latter attains a state and powers not otherwise attainable?

"This is the gist of the whole question; and I doubt not you are right in your conclusion, that there is no mesmeric phenomenon (well verified, and free from all charge of *accident*, *mistake*, or *imposture*) which is not explicable by causes, normal or abnormal, within the mesmerised individual himself.

"Your experiments," Dr H. adds, "have done much, and by new methods, to elucidate and justify this conclusion; and they apply very explicitly also to this *biological branch of mesmerism*."

tigation of the subject with the honest desire to arrive at truth. It is important to remark, however, that these vigilant phenomena prove that a strong fixed idea of determined scepticism may so possess the mind of an inquirer as to render him a totally unfit person to be a valid witness of what are matters of plain fact and observation to those who are not so enslaved by prejudice. His imagination and fixed conceit may be so powerful and engrossing that unconsciously he is forced to interpret the *ideas* of his own *mind* as the *facts*, instead of what is actually presented before him. This view of the subject throws important light upon the nature and value of evidence in cases where parties point-blank contradict each other when describing occurrences of which they have both been eye-witnesses. A strong mental impression or prejudice may have deceived one or more witnesses, without the slightest intention to misrepresent facts.

Chambers' correspondent has, to a certain extent, hit upon the true solution of the cause of these vigilant phenomena,—viz., that it is a play upon the *will* of the patient, and the influence of sympathy and imitation upon certain individuals. This theory readily accounts for all the phenomena in which muscular motion is concerned; but it does not at all account for a much larger and more surprising class of phenomena,—viz., suspending, intensifying, or perverting impressions addressed to any or all of the organs of special sense. These results are to be accounted for only by an appeal to the power of an over-excited and vivid imagination, and fixed idea changing physical action. Those who suppose that the power of the imagination and sustained mental attention directed to any part of the body is a mere *mental* condition, without being accompanied by any change in the physical organism, labour under a grave mistake. Sustained mental attention directed to any part changes both the sensation and circulation of the part so regarded, and in a more marked degree, according to the imagination and belief of the patient. From this cause the most grave and fatal diseases may be engendered, and by withdrawing the attention, and fixing the train of thought on some new object or pursuit, relief and cures may be effected in cases which would utterly fail without such management of the mind. In support of this doctrine, I beg to refer to Dr Holland's "Medical Notes and Reflections," where he treats of "the effects of mental attention on the bodily organs" with his usual ability.

Voluntary motion arises from a mandate of the *will* giving a special direction to the nervo-vital influence, so as to excite into action the class of muscles which naturally produce such motion, whilst their antagonists remain passive.

In these electro-biological experiments or vigilant phenomena, however, the suggestion of the operator excites a greater flow of nervous force into the class of muscles which produce the action *suggested*, than the patient is able to throw into the opposite class. *Both* classes of muscles, therefore, are called strongly into action, but the *suggested* idea being the *stronger*, there is no efficient volun-

tary motion, notwithstanding a great amount of nervous influence may be expended by the patient, as evinced by his rapid exhaustion. In this manner I lately caused a patient's hands to become involuntarily closed on a walking-stick, so that he could not relax his hold of the stick; and then, by saying aloud in his hearing, "I will now make it so heavy that it will be impossible for him to support its weight," the idea excited by this simple auricular suggestion was more than a match for his energetic but misdirected muscular efforts, for there he struggled, until at last he fell upon the floor, quite exhausted by his efforts to support his imaginary load. The patient assured us, after the experiment was over, that he firmly believed he saw a fifty-six pound weight put upon each end of the stick at points indicated, and that he felt the weight increase at each addition, and that its weight at last became quite overpowering. It was obvious, from the condition of the patient, that he was quite as much exhausted by his efforts to support the imaginary weight as he could have been had it been real. Most people must have experienced a similar feeling and result from an attack of nightmare, during common sleep. The struggle and exhaustion are in both cases analogous. On the other hand, if the suggested idea and strongly fixed belief go along with the will of the patient, his volition may thereby be so much intensified as to enable him to lift a weight which he would be quite incapable of doing in his ordinary state. The history of cases of panic in an army from slight causes, and deeds of daring and achievements almost superhuman, during fits of enthusiasm, are examples in point on a large scale.

Neither a play upon the will, nor sympathy, nor imitation, can account for patients feeling insufferable heat or cold, or for having the organs of sight, hearing, smell, and taste, paralysed or perverted so that the patient should be made to perceive impressions at variance with the ordinary effects of the exciting cause, according to the suggestions of the experimentalist. A strongly fixed idea and vivid imagination, however, are well known to be quite adequate to do so, as witnessed in monomania, delirium tremens, intoxication, and narcotism from opium, the hachisch, and other drugs, as well as in various other morbid conditions. In all these states, it is a well ascertained fact, that ideas may become so vivid as to assume all the force of reality, and that whilst the individual so affected retains a certain amount of consciousness of all around, still he cannot emancipate himself from the erroneous impression, or that he may be so engrossed with the one idea as to be dead or indifferent for the nonce to all other impressions.

Before concluding this paper, I shall state my views regarding the nature and effects of what have been called mesmeric passes. The passes have been divided into *contact* passes, in which the fingers of the operator are drawn gently over the part intended to be affected, and into *non*-contact passes, which consist in passing the hand over the part, near to, but not touching it, the fingers being extended

and held apart, with a tremulous motion, so as to cause a slight agitation of the air in contact with the part operated upon. The mesmerists alleged, that the effects realised arose from some occult or magnetic influence, or the odyle force of Baron Reichenbach, affecting the part in a particular manner. My researches, however, have led me to attribute it to the power which the mind of the patient possesses to change the physical action of the part to which it is strongly drawn and fixed by sensible impressions of an external nature, or by a steady fixed state of mental attention, by an effort of the patient's own will, especially if done with the expectation and confident belief of some change being about to happen. If, at the proper stage of the sleep, the impressions are directed to the organs of special sense, ideas will be excited in the mind of the patient in accordance with the special function of the organ to which the mind has been so directed; if to a part where there are muscles subjacent, it will excite the muscles into action, and probably such ideas also as usually occasion or precede such physical actions.

One of the most puzzling classes of phenomena which I had occasion to observe was the *reverse* results which seemed to arise from the *same sensible* impressions. Thus, contact passes, or agitating the air along the course of an arm or leg, would call the muscles into action and elevate the limb. This the mesmerists called "mesmerising passes"; and wafting the air across the extremity caused it to descend, and this they called "demesmerising passes"; or agitating the air over one side of the head would cause the head to follow the hand of the operator first to the one side and then to the other; or darting a hand over the hand of a patient and suddenly withdrawing it, and repeating the operation, would cause the hand to rise and become cataleptic. This was set down by the mesmerist as indubitable proof that there was an attraction between the hand of the operator and the patient's hand, which attracted it as the magnet does iron. I very soon perceived that, on first going into the sleep, patients had sufficient mobility to be affected by certain impressions, and that they would approach to or recede from impressions, according as they were agreeable or disagreeable in quality or intensity. Thus, soft music they would be delighted with, and approach to, whilst they would be painfully affected, and run from, loud or harsh music; and the same of odours, and impressions of heat and cold. I ascertained that titillating the skin, or agitating the air over the skin, would call into action the subjacent muscles, and thus you might flex the hand, and raise the arm; and by acting in a similar manner with the *opposite* class of muscles, the hand and fingers would become extended, and the arm fall. This seemed simple and comprehensible enough. But at length I ascertained that a class of muscles having been called into action by such impressions, and allowed to remain a short time in the position assumed, the repetition of the like sensible impression on the same points through which it had produced the action, would now reduce it, thus producing, from the

same apparently exciting cause, the very *reverse effects*, whether that had been a contact pass or a simple agitation of the air. I found, moreover, that my *will* had nothing to do with these results, as precisely the usual result would be realised whilst I was willing the reverse. These opposite effects from the same exciting cause puzzled and perplexed me vastly; but at last I arrived at a very simple solution of the apparent mystery. When the patient got into the proper stage of the sleep for manifesting these phenomena, I recalled to mind that consciousness and the will are so much adumbrated that the movements are instinctive or automatic, and hence the impression merely gives a tendency to motion, the direction and character of the motion being the *most natural motion under the circumstances which exist at the time*. Hence, if a muscle is passive, it will become active, and if active, it will become passive, from the same exciting cause. Thus, when an impression is made on the hand or arm when reposing on the lap, as it cannot descend it will rise, and become rigid; but by making a like impression on it after remaining in that condition for a little while, it will give a tendency to perform the most natural motion, which, in *this* instance, is to descend. If any obstacle be interposed to its rising or descending, and the impression be repeated, it will move laterally. This same mode of exciting action by sensible impressions, and reversing results in the way described, may be confined to individual muscles, or classes of muscles, and in this way we may act on the muscles of expression, so as to awaken any passion or emotion in the mind; the action of the muscles, which constitute the "Anatomy of Expression," suggesting the feeling to the mind of the entranced subject, as the idea, in the waking state, naturally calls such anatomy of expression into play. It is, therefore, a mere inversion of the sequence which ordinarily obtains between mental emotion and physical manifestations of such emotions. That there is nothing occult or specific in the pass with the hand, is manifest from this, that a similar agitation of the air by the blast from a pair of bellows will produce precisely similar results as the like current of air from the wafting of the human hand, as I have proved to the entire satisfaction of hundreds of intelligent individuals.

A pass, therefore, as a visible or sensible impression, aids the patient in concentrating his mental attention to a given organ or part, and thus influences the function, through giving a special direction to a power residing within the patient's own frame; but it no more imparts a virtue of an occult nature from the operator to the patient, than the lens *produces* the light and heat which it makes visible and perceptible to the senses, through concentrating the luminous and calorific rays of the sun, and drawing them to a focus. Both the pass and the lens aid in concentrating and manifesting the respective influences; but neither the operator nor the lens is the source or origin of the power or influence so manifested.

The above is an explanation of what may be realised naturally in

subjects without any previous training or auricular suggestions whatever. It is quite possible, however, to subvert the whole of these natural phenomena by a system of training, as follows:—Supposing that, with each touch or agitation of the air, the operator speaks aloud and predicts what should happen, the auricular suggestion may be so strong as to cause the *predicted* manifestation to be realised instead of what otherwise would have been the case; and thus, from this time forward, through the double conscious memory, the like impression on that part or organ of sense, will recall the previously associated idea and manifestation. We may, therefore, have an *artificial* as well as *natural* set of phenomena manifested, according to the mode of operating and intention of the operator.

It also merits notice that there are some subjects who, during the sleep, see through their partially closed eyelids. In the case of such subjects, if the operator gazes stedfastly at any part of their bodies, such as a leg or arm, the patient will immediately catch the suggestion, and imagine that a movement of the member looked at is wished to be made, and his docility will instantly incline him to obey the suggestion, as if it had been excited by a contact-pass, or any other stimulus applied directly to the part—*i. e.*, if the member is down it will rise, or if up it will descend, or will move laterally if a mechanical obstacle is interposed to prevent it moving upwards or downwards. In like manner the remarkable tendency to sympathy and imitation in such patients, will induce them to observe and imitate every physical act of the operator, or other person to whom he has specially directed the patient's attention. Interpose a screen, however, and then make movements of the body or extremities, and they will rely upon *hearing*, instead of sight, under the circumstances, and will only be *occasionally* correct now in their imitation of physical acts; which is a clear proof that, in the other instances, they were directed by sight—viz., seeing through the partially closed eyelids.

All these phenomena may be realised without the patient intending to play off any deception on others, or having any remembrance of the facts on coming out of the sleep.

I have never yet seen any phenomena during either the hypnotic or mesmeric sleep, or during the state for manifesting vigilant phenomena, which were not in accordance with generally admitted physiological and psychological principles. The senses and mental powers may be torpified or quickened in an extraordinary degree; but I have never seen anything to warrant a belief that individuals could thereby become gifted with the power of reading through decidedly opaque bodies; acquire the faculty of knowing the meaning of language which they had never learned; and other transcendental phenomena, called by the mesmerists the *higher* phenomena. The power of a strongly fixed attention, vivid imagination, and self-confidence, however, enables them to perform some extraordinary feats of phonic imitation, and writing and drawing by

touch, without the use of their eyes; discovering parties who own certain articles worn by them, through the quickened sense of smell; overhearing conversation in a distant apartment, which they could not do in the waking condition; of recalling to mind things long forgotten when awake; and also of deducing conclusions, manifesting uncommon shrewdness, from premises suggested to them, or arising in their minds spontaneously from recollection of past events, to which they have directed their concentrated attention. These latter feats are precisely analogous to those manifested by the Celtic Seers, or Second-sight men, in the Highlands of Scotland. Their attention was so fixed, and their spirits so rapt in the subject of their deep contemplation, that they stood with staring eyes,—"the eyes being open, but the sense shut,"—similar to some somnambulists, and the shrewdness of some of their deductions was looked upon as the result of a sort of inspiration peculiar to that class of men.

Several important inferences may be drawn from what has been adduced regarding the power of the imagination, belief, and fixed act of attention, in changing and controlling physical action, and of the state of the body thus superinduced re-acting on the mind. It not only enables us to comprehend, upon scientific principles, the cause and cure of many diseases, where there has been no specific and adequate external physical agency to account for the results; but it also explains many phenomena which used to be attributed to demonology, witchcraft, ghost-seeing, being spell-bound, the power of the Obi women, who could cause their credulous victims to wither and die under their assumed malign power and maledictions; the power of charms, spells, and amulets, of Perkins' metallic tractors, the efficacy of galvanic rings, bread pills, and such like. It also explains the alleged clairvoyance of the Egyptian boys after looking stedfastly at an inky globule held in their hands, narratives of which are published by Lord Prudhoe and Mr Lane. The revelations of Kelly, recorded daily for Dr Dee, Warden of Manchester in the reign of Queen Elizabeth, and which were alleged to be revealed to him by a spirit whilst gazing into Dr Dee's celebrated show-stone; the fortune-tellers, who exercise a similar mode of looking into futurity, by gazing into a glass egg; and the late revelations of the same sort, published at the end of Zadkiel's Almanac for this year, as to the revelations of the Angel in Lady Blessington's magic crystal, all come under the same category, being merely figments of fancy, excited by questions or otherwise, set down for visions seen, and answers audibly uttered, or written in visible characters before them by the said Angels. Whatever greatly excites and changes the existing train of thought and feeling, especially if done with faith and expectation, and fixed mental attention, will assuredly be followed by a change in the previously existing mental and physical condition of the subject.

We also thus acquire this salutary lesson, that, in order to have

a healthy state of mind and body, it is requisite that *all* our faculties should be duly cultivated, and that the attention should not be devoted too long and exclusively to any *one* object or pursuit, as the latter course has a tendency to engender a morbid state of mind as well as of body. Hence the advantage of relaxation and amusements of various kinds, so long as they are pursued in reason and with moderation. We are also taught this important fact, that dogmatic scepticism, or prejudice, not only beclouds the reason, but also destroys or perverts the perceptive faculties. This truth seems to have been clearly perceived by Dugald Stewart, when he penned the following notable paragraph:—"Unlimited scepticism is equally the child of imbecility, as implicit credulity."

I shall conclude this essay by a very simple mode of illustration, as respects the different points of view in which the mesmerists, the electro-biologists, and myself, stand toward each other in *theory*, by referring to the two theories of light contended for at the present time. Some believe in a positive emission from the sun of a subtile material, or imponderable influence, as the cause of light; whilst others deny this emission theory, and contend that light is produced by simple vibration excited by the sun, without any positive emission from that luminary. I may, therefore, be said to have adopted the *vibratory* theory, whilst the mesmerists and electro-biologists contend for the *emission* theory. But my experiments have proved that the ordinary phenomena of mesmerism may be realised through the subjective or personal mental and physical acts of the *patient alone*; whereas the proximity, acts, or influence of a *second* party, would be *indispensably* requisite for their production, if the theory of the mesmerists were true. Moreover, my experiments have proved that audible, visible, or tangible suggestions of another person, whom the subject believes to possess such power over him, is requisite for the production of the *waking* phenomena; whereas no audible, visible, or tangible suggestion from a *second* party ought to be required to produce these phenomena, if the theory of the electro-biologists were true.

There is, therefore, both positive and negative proof in favour of my *mental* and *suggestive* theory, and in opposition to the magnetic, occult, or electric theories of the mesmerists and electro-biologists. My theory, moreover, has this additional recommendation, that it is level to our comprehension, and adequate to account for all which is demonstrably true, without offering any violence to reason and common sense, or being at variance with generally admitted physiological and psychological principles. Under these circumstances, therefore, I trust that you will consider me entitled to your verdict in favour of my MENTAL THEORY.

ARLINGTON HOUSE, Oxford Street, Manchester.

ARTICLE IV.—*Case of Deficiency of Cranial Bones.* By JAMES H. HOWIE, M.D., Shandon, Gareloch, late House-Surgeon, Glasgow Royal Infirmary.

ABOUT 4 A.M. on the morning of the 17th April 1851, I was called to attend Mrs ———, æt. 32, the mother of two children, who had been seized with labour pains several hours before. On my arrival, I found, on examination, the membranes entire, the os uteri dilated to about the size of a crown-piece, and the vagina cool, relaxed, and copiously lubricated with mucus. A soft body, which had exactly the feeling of the placenta, both in smoothness and consistency, and seemed to be nearly of the same size, was felt presenting. As the os became more dilated, it was found to be divided by a fissure somewhat resembling a nates presentation; the finger could now be passed beyond this tumour, and the child's right ear was easily felt. The tumour was found to be firmly attached to the upper surface of the head, but no connection could be discovered between it and the uterine parietes. At 8 A.M., as the pains were very feeble and inefficient, and as the os uteri was well dilated, I resolved to rupture the membranes; on accomplishing this, an unusually large quantity of liquor amnii was evacuated. I was now satisfied, on examining the tumour, that it was either an enormous hernia cerebri, or the brain itself unprotected except by the dura mater. I therefore allowed the labour to proceed in the natural manner, the pains gradually became more effective, and a very large male child was born about 11 A.M. The whole of the upper surface of the cerebral mass was perfectly uncovered, except by the usual membranes, and had, as seen through the dura mater, a dark purple colour. The child did not cry, but after one or two short spasmodic attacks, in which the whole body and head were thrown convulsively backwards, as in opisthotonos, for a second or two, it began to respire irregularly and stertorously, the number of respirations per minute being about fourteen. The cord was now divided, and the greater part of the body, with the exception of the head and chest, placed in a warm bath; the breathing became in a short time rather easier, and the child was washed and dressed in the usual manner. The brain was covered with a piece of soft linen and a light cap, and the head slightly elevated, but so placed that no pressure was made on the unprotected organ.

From this period up to that of the child's death, the breathing was always more or less laborious, the pupils remained insensible to light, and no reflex motions of the extremities whatever could be induced. On being applied to the breast about six hours after birth, the child made no attempt to lay hold of the nipple, and the change of position necessary in making this trial suspended the respiration for a few seconds; and, at the same time, the countenance, from being a little darker than natural, became perfectly livid; it was afterwards

observed, that any considerable change of position was attended with the same result. The mouth of a nursing bottle, prepared in the usual manner, and containing milk diluted and slightly sweetened, was introduced between the lips in a short time afterwards; after a little, the child commenced to suck greedily, but ceased entirely in about a minute, nor could it be induced to suck again. On several occasions a very small quantity of fluid was introduced into the mouth, but it was never swallowed, and invariably produced more or less lividity of the face, and, for a short time, suspension of the respiration. The child died on the 20th of April, about 1 A.M., upwards of sixty hours after birth. Meconium and urine were twice voided during this period.

On an *external* inspection of the head, the frontal and parietal bones were found to be entirely awanting; the face was livid; the eyes and eyelids were perfectly natural in appearance, but the roof of the orbit was formed by the integument, which extended backwards as far as the crista galli, to which it was attached; it also was prolonged inwards beneath the brain, around the whole circumference of the head, forming a circular band about three-fourths of an inch in breadth, on which both hemispheres rested, and which seemed, posteriorly, to supply the place of, or at least to cover, the tentorium cerebelli. Both hemispheres of the brain, as seen and felt through the dura mater, were of a deep purple colour, as if from effusion of blood on their surface, and had a semi-fluid consistency.

Further inspection of the brain was not permitted. The rest of the body was perfectly natural in appearance, and fully developed.

The above case is not only interesting on account of its rarity, but as illustrating the possible difficulty of diagnosis, if I may use the term, between a case more or less resembling that described, and one in which the presentation is placental. The distinguishing of the presentation of the unprotected cerebrum from that of the placenta, was at first in this instance a little difficult; but in a primipara, from the contracted state of the passage, or in a case of rigid and undilated os uteri, it would have been much more so.

I have only to add, that the opinion of MM. Guitard and Joly,—that mental impressions have generally something to do with the production of monstrosities, or in causing arrests of development, does not receive corroboration in this instance. The mother, an intelligent woman, is utterly unconscious of having experienced any mental impression whatsoever which could at all bear upon the subject, except a little anxiety at the period of her gestation being, as she thought, unusually prolonged. It may be proper to mention, that for about two months before her delivery she suffered from occasional short paroxysms of pain in the abdomen. This, however, in all probability had no connection with the production (to speak paradoxically) of the monstrous deficiency, for though an unusual occurrence with her, it is with many women, when pregnant, very common.

SHANDON, May 7th 1851.

ARTICLE V.—*Case of Stricture of the Urethra.* By JAMES F. CRUIKSHANK, M.D., Dalmellington, Ayrshire.

THE recent introduction, by Mr Syme, of a method of treating stricture of the urethra by incision, has brought within the range of surgical relief a certain class of cases, which hitherto have been regarded as irremediable by the usual method of dilatation, and which in some cases even demanded immediate relief by puncture of the bladder. But as new methods of operating are generally slow in being adopted, more especially when such surgical interference finds a place among the minor operations of surgery, I think that medical men, and chiefly those practising in large country districts, are bound to record their experience, when they have had any opportunities of putting new methods of cure to the test. Upon the principle, also, that every case ought to be recorded in which any new operation has had a fair trial, whether the result shall have been favourable or not, I beg to publish the following case. I have withheld its history till now, for this reason, "That strictures which have been cured in the ordinary way, by dilatation, are often apt to return in some degree upon exposure to cold and wet, even when gonorrhœa has not been again contracted."

About the beginning of November 1849, James Macrae, a miner, 45 years of age, and of rather intemperate habits, consulted me regarding a stricture of the urethra, from which he had suffered with more or less severity for about three years, the result of an acute gonorrhœa, which had lasted some weeks, and had given way under the ordinary treatment, without injections.

The stricture was a very tight one (admitting with difficulty No. 2 bougie), and situated at the bulb of the urethra. During a period of about two months I continued to dilate it regularly, in the usual way, by the introduction of metallic bougies, till No. 8 of the scale could be introduced. Gradually, however, he began to suffer much pain from the passage of the instrument through the canal, and latterly had rigors, succeeded by severe constitutional irritation, which confined him to bed for some days. I did not venture to introduce the bougie for more than a week after his recovery from this attack, and on doing so, I found that the stricture had contracted to its original size, so that I could with difficulty introduce bougie No. 2. A small hard swelling, with indistinct fluctuation, now appeared at the seat of stricture. Being under the necessity of working constantly, and, from the nature of his employment, to sit on the wet ground (circumstances which tended to aggravate his complaint), he was naturally anxious for some effectual and speedy cure. At this time I had occasion to be in Edinburgh for a day or two, and availed myself of the opportunity of seeing Mr Syme perform his operation for the relief of stricture, upon two patients. I determined accordingly to try it; and, on the 30th January 1850, with the

ready consent of the patient, and the help of my assistant, the late Dr M'Curdy, I performed the operation as directed by Mr S. I introduced a grooved bougie, of very small dimensions (about No. 2), and made an incision in the raphe of the perineum, over the seat of stricture, which I completely divided. On making the incision, a small quantity of matter escaped, which had formed in the tumour I have already noticed as existing. I then introduced No. 9 catheter, which was retained during 48 hours, without much uneasiness. The urine continued to come by the wound for five days, when it began to flow by the natural channel; and in three weeks after the operation, he was able to resume his work, the wound having healed slowly and well. I introduced catheters Nos. 11, 12, and 13, once a-month for three months, with ease, and without pain or rigors. He then left this part of the country, and I saw no more of him till about the end of March 1851, when he appeared quite well, and informed me that he had been constantly at work since his recovery from the operation; and, in order to test the efficacy of the cure, I introduced No. 13 bougie without the least difficulty.

DALMELLINGTON, 19th April 1851.

ARTICLE VI.—*On the Treatment of Stricture by External Incision.* By JAMES SYME, Esq., Professor of Clinical Surgery in the University of Edinburgh.

STRICTURES of the urethra may be divided into three classes. First, those in which there is no real organic contraction of the canal; second, those in which the contraction admits of dilatation, and may be prevented from returning by the use of bougies; and thirdly, those which either resist attempts to effect dilatation, or return so quickly after its completion as to prevent any permanent advantage from being derived by the patient.

The first class is very numerous, in consequence of the various and frequent circumstances which, independently of contraction, tend to impede the flow of urine. Hemorrhoidal affections, and fissure of the anus, enlargement of the prostate, paralytic states of the bladder, disorder of the digestive organs, sexual excesses, and an irritable state of the urethra, with many other derangements of a functional and organic kind that might be mentioned, are apt to occasion, more or less, uneasiness in micturition, and excite the suspicion of stricture. The introduction of a full-sized bougie would of course correct any such erroneous impression, but, unfortunately, cannot always be accomplished, even though there is no obstruction, through want of practice in the use of instruments, and morbid sensibility on the part of the patient. Both he and the practitioner are thus apt to be confirmed in their mistake, and led to enter upon

a course of treatment not only altogether unnecessary, but frequently very injurious. It is in these imaginary strictures that various means of remedy have acquired any credit they possess, such as, of course, all those employed externally, or through the medium of the system; and also many others of a local kind, which could not be mentioned on the present occasion without giving offence to their respective authors or advocates, and therefore may be passed over in silence, since they never can be of any practical utility *to the patient*. At the same time, I think it right to protest openly and distinctly against any evidence drawn from this spurious source, being admitted into the discussion of what may be done with most advantage for the treatment of real organic stricture.

The dilatable form of this disease may be remedied in many different ways, through the use of bougies and catheters, flexible or rigid, and introduced occasionally, or retained permanently during the period of treatment. While it cannot be denied that the object in view may be attained by any of these modes, there is as little doubt that they cannot be all equally beneficial, or free from inconvenience; and when they are compared together, with regard to their qualities in these respects, I think there should be no hesitation in giving a decided preference to the metallic bougie, employed at intervals of from two to four days, and withdrawn each time immediately after being introduced. This method, which has been characterised by one of the London writers as "*frivolous*," affords the certain means of dilating any dilatable stricture in the course of a few weeks, not only more easily and safely than any of the other forms in which pressure is employed to widen contractions of the urethra, but also with a greater degree of lasting effect, since it appears that the more slowly the process of dilatation is accomplished, the less rapid is subsequent relapse.

The third sort of stricture fortunately bears a small proportion in frequency to the one last mentioned, but does not do so to the same extent at all parts of the urethra. It is between two and three inches from the orifice, and just before the bulb, that organic contractions are chiefly met with. Tight strictures are much more frequent in the latter than the former of these situations, but when they do occur in the anterior part of the canal, are more apt to prove obstinate than those of the posterior position. Thus, of twelve tight strictures, the proportion may be about three in the anterior, and nine in the posterior situation, and in each of them *one* of the obstinate kind. I happen to have in my possession a letter from the late Mr John Pearson of London, to the late Dr John Thomson of Edinburgh, which affords a remarkable illustration of this pathological fact in the following extract:—"London, September 26, 1804,—I have lately had under my care a case of impermeable stricture, about two inches from the meatus urinae. Being foiled in every attempt to open it, I dissected down to it, cut through the contracted part, which was as hard as cartilage, and the wound is now very nearly

healed. The patient makes water in a perfectly good stream. I yesterday saw a similar case, and intend to operate on it on Thursday."

In this obstinate form of stricture, there is not only contraction of the canal, but also a remarkable thickening of the part affected, which consequently is in general distinctly perceptible through the integuments, being felt like a ring of firm consistence, and flattened form. The bougie when introduced is firmly grasped, so as to be withdrawn with difficulty, and the patient, so far from experiencing relief afterwards, usually suffers an aggravation of his distress. It is difficult to account for these differences from the cases of ordinary occurrence, or to explain the origin of a disease so peculiar in its characters. I have not been able to trace any connection between the effect in question, and the cause giving rise to it, or to distinguish any specialty of constitution favouring its production. But that there may be some predisposition depending upon the temperament, or other circumstances of individual limitation, would appear from the hereditary and collateral connection occasionally observed between patients thus affected. A gentleman who was lately under my care for a stricture of twenty-four years' standing, told me that both his father and grandfather had died of the disease. And some time ago, I divided the stricture of a patient who was one of five brothers, three of whom had died of the disease; a fourth, who is now well, had been under my care; and this, the fifth, had run an extraordinary gauntlet of treatment, of which it may be sufficient to mention a year or two passed under the care of Mr Guthrie, who employed a succession of catheters, and afterwards caustic; a prolonged residence at Gräffenberg, under the water-cure; and a trial of Parisian homœopathy, which nearly proved fatal in a negative way, by disregarding an extravasation of urine that took place, all through the scrotum, and over the lower part of the abdomen, and but for the skilful care of M. Cloquet would doubtless have proved fatal. It is a fact of much practical importance that there is seldom, if ever, more than one contraction of the unyielding or resilient kind in the same urethra.

Having frequently had occasion to witness the distress resulting from this form of stricture, and to regret the insufficiency of any known means of remedy, I had great pleasure in communicating to the profession a safe, easy, and effectual method of overcoming the difficulty, which consisted in dividing the contracted part of the canal upon a grooved director passed through it. The speedy and complete relief thus afforded, even in cases of the most protracted and aggravated suffering, is one of the most gratifying services that can be rendered by the art of surgery. The favourable anticipation originally entertained of it has been more than realised; and any patient who continues to suffer from obstinate stricture, can no longer attribute his wretched state to the imperfection of surgery. In the present and following papers I propose to illustrate and confirm the

statements which have hitherto been offered in favour of this treatment.

I have represented it as secure against the ordinary dangers which attend operations on the urinary organs,—namely, hemorrhage, inflammation, and infiltration of urine; and may now appeal to the evidence of forty-four cases which I have thus treated without encountering any of these effects, even in a single instance. It is quite true that through awkwardness, or defective acquaintance with the parts concerned, the artery of the bulb may be cut, and other errors of a serious nature be committed, so that I am not prepared to deny the allegation of this procedure being attended with danger in "inexperienced hands." But any man of ordinary skill, who chooses to follow the directions which I have given for conducting the process, will find little difficulty in accomplishing it. And here I may remark, that my commentators who speak of "long incisions" in the perineum, and laying open the urethra to the extent of several inches, have no warrant for such statements in anything that I have said or written on the subject. I never open the urethra beyond the extent of an inch, and seldom beyond that of half, or two-thirds of an inch.

The bleeding at the time of the operation hardly ever exceeds one or two teaspoonfuls; but occasionally takes place some hours afterwards, to the amount of an ounce or two, being apparently proportioned to the hemorrhagic disposition of the patient, and therefore to be deemed rather salutary than otherwise. If any circumstance should render it desirable to prevent the chance of this discharge, a piece of lint put into the wound at the time of its infliction will effectually do so.

As to the question of "impermeability," I simply maintain, that if the urine passes out, instruments may always, through care and perseverance, be got in beyond the contraction. It should be observed that the case here is quite different from that of a distended bladder requiring *immediate* relief. I have never maintained that in such circumstances the introduction of a catheter was always practicable; and although in the course of two and twenty years of hospital practice I have not happened to meet with a stricture that resisted this instrument, I neither profess security of being equally fortunate for the future, nor teach such confidence to others, especially if they are not to be daily practised in overcoming obstructions of the urethra. In my "Principles of Surgery" it is said:—"If the surgeon possesses the requisite tact for introducing instruments into the bladder through the urethra, and has the treatment of the case from its commencement, he will very rarely, perhaps never, be under the necessity of resorting to this puncture. But should he not be able to draw off the water by the catheter, either from his own want of dexterity, or from the existence of obstacles arising from mismanagement or previous organic alteration of the passage, as stricture or

enlargement of the prostate, complicated with a lacerated, softened, swelled, and bleeding state of the lining membrane, caused by forcible attempts to pass an instrument, there can be no hesitation in having recourse to the operation. Puncture of the bladder, however performed, is always attended with more or less danger of urinous infiltration; but a doubtful remedy is better than none; and there are few states of disease more hopeless than complete retention of urine, permitted to follow its own course."—P. 359.

But if sufficient time and opportunity be afforded, I firmly believe that every stricture may be rendered permeable. I feel deeply impressed with the importance of this principle, and, with the view of establishing it,—not, certainly, to obtain an advantage over my professional brethren,—I have again and again offered to undertake the treatment of any stricture, however impermeable it might be deemed, that was sent to the hospital. All those hitherto received have been afforded complete relief, either by simple dilatation, or by incision after being rendered permeable; and I venture to hope that the results in future will be no less satisfactory.

With regard to the permanence of effect obtained through incision, in my earlier communications I could only express the expectations which seemed to have a reasonable foundation; but now, with the advantage of seven years' experience, I am able to speak more decidedly on the subject. It is well known that cuts, lacerations, and even slight bruises of the urethra from external violence, are almost sure to occasion stricture of the canal, unless a full-sized instrument be passed occasionally for a few weeks after the injury has been sustained; and I have, therefore, always considered attention to this circumstance essential for success. The first three cases of stricture at the bulb in which division was effected, after being treated in this way, seemed so completely divested of their contractile tendency that I expected it would never be necessary to use any precautionary measures by introducing bougies beyond the period just mentioned, immediately subsequent to the operation. In many patients this expectation has been fully realised, but in others it has been found necessary to introduce bougies for a longer time. How far these exceptions may depend upon some imperfection in the performance of the operation, the mode of healing in the wound, irritability of the system, or impropriety in the patient's mode of life, it is not very easy to determine at present, although more definite ideas may probably be acquired from more extended observation. In the meanwhile I feel warranted to state, that if the stricture be thoroughly divided, and a full-sized bougie be passed occasionally for a month or six weeks afterwards, the patient will certainly obtain complete relief from all the distress which attends the obstinate form of the disease, and require at most merely the precautionary treatment which is found sufficient to prevent the ordinary form of stricture from being troublesome. In illustration of what has been stated, I will now commence a relation of facts, which shall be con-

tinued so long as such information seems to be required by the profession.

IMMEDIATE EFFECTS OF THE OPERATION.

As it has been alleged, and perhaps believed, though I should hope by few, that, in order to recommend the treatment of stricture by external incision, I have glossed over the unpleasant consequences of this procedure, and made it appear less formidable than the reality, I hope the following evidence will satisfy every reader who entertains any doubt upon this part of the subject, that I have not underrated the effects of the operation. To show that I do not resort to division of strictures unnecessarily, as had also been insinuated, or until dilatation is found altogether unavailing for the patient's relief, I stated in the last Number of this Journal the leading particulars of all the cases which had been subjected by me to operation in the Royal Infirmary, during the winter session just concluded. They amount to seven in number, and, together with an eighth which has since occurred, were treated publicly in the hospital. All the operations were performed in the theatre before the most numerous class of surgical clinical students in her Majesty's dominions, who can testify that the object in view was always attained without delay or difficulty, and that in no instance did the bleeding exceed in amount a teaspoonful. In regard to the hemorrhage, or rather the absence of hemorrhage, subsequently to the operation, as I cannot speak from my own personal observation, it may be satisfactory to give the house-surgeon's account of what fell within his observation, which was constantly fixed on the patients:—

"Royal Infirmary, Edinburgh, May 16, 1851.

MY DEAR SIR,—During the last seven months there have been eight cases of stricture of the urethra in the hospital, in which you have divided the stricture by external incision upon a grooved staff, previously passed through it. Every one of these eight patients I saw repeatedly in the course of the afternoon and evening after the operation, and I always took especial care to ascertain whether or not there was any bleeding from the wound. In every case the amount of hemorrhage after the operation was very trifling—certainly never to such an extent as to make me think it at all necessary to use means for arresting it. In two or three of the cases there was no bleeding, or at all events not more than a single teaspoonful of blood; while the average amount of blood lost after the operation by all the eight patients was, I can confidently state, not more than *one fluid ounce* in each case.—I remain, my dear Sir, your obedient servant,

"CHARLES MURCHISON, Surgeon,
"Resident Clerk, Clinical Surgical Wards.

"James Syme, Esq., Professor of Clinical Surgery."

The gentlemen attending the hospital, who had daily opportunity of watching the progress of these cases, will not, I dare say, have forgotten the smiling aspect, grateful expression, and confident tone, with which my questions were answered. "How do you feel to-day?" "Quite well." "Have you any pain or uneasiness?" "None whatever." All these patients obtained complete relief, which will, I fully expect, prove permanent; but as sufficient length of time has not yet elapsed to test the permanency of their recovery, I produce them at present merely as evidence in regard to the immediate effects of the operation.

NON-EXISTENCE OF "IMPERMEABILITY."

In December 1849, I was applied to by a gentleman, who had come over from Ireland, in that state of extreme distress and suffering which results from stricture when the water can neither be discharged nor retained. He was about thirty years of age, during twelve of which he had been afflicted with the disease, and been under the care of many practitioners, not one of whom had ever succeeded in passing an instrument through the stricture, which lay between the scrotum and bulb. In a note of his case the patient gave me, he says:—"I was under Liston's care for nearly two months, and after his trying every other day to pass an instrument, I got tired of the expense, as well as the pain I suffered, and discontinued my visits to him. At ——— [a large provincial town in England], I was attended for three weeks by one of the first doctors there, and nothing could be done for me by him. Also in America, many doctors have tried to pass the bougie, but always without success, and army doctors [the patient was a military officer] without end."

In a letter from this gentleman, dated the 10th of March 1851—that is, more than a year after I had divided the stricture upon a director passed through it—the following account is given of his condition:—"On my return, last Saturday, from England, I found your kind note, inquiring after my health. In answer, I am delighted to be able to tell you, that I have never felt the slightest return of my old malady. I sometimes pass No. 7, but it is only to satisfy myself, as I think now it is of no use doing so. My health [which had been much broken when he was under my care] also never was so good; and I can undergo more fatigue, which I do, than I ever could in my whole life, and for this I may thank you, which I heartily do."

PERMANENCE OF EFFECT PRODUCED BY THE DIVISION OF STRICTURES.

The patient whose case is the fourth related in my treatise on stricture, lately presented himself at the hospital, and afforded an opportunity of ascertaining the result of his treatment. As the case is not long, it may be here reprinted:—

"J. T., aged thirty-six, was admitted into the Royal Infirmary,

on the 29th of November 1847, suffering very severely from stricture of the urethra. He stated that, about three years before, he had lain intoxicated in the street, during the whole of a winter night,—in the morning finding himself (to use his own expression) "frozen to the ground;" and immediately afterwards had begun to suffer from the symptoms of stricture. In the course of the preceding winter, he had been eleven weeks in the hospital under treatment by dilatation, with considerable but only temporary relief, as the symptoms returned very soon after his dismissal. In these circumstances, it appeared necessary to divide the stricture, which was extremely tight, and seated at the bulb. I performed the operation on the 2d of December; the catheter was removed on the 4th; and the patient was dismissed, with complete relief from his complaint, on the 3d of January."

For three years afterwards he had continued (to use his own expression) as well as any man in the world, but then caught a gonorrhœa, and suffered from a partial return of the symptoms; for which he had applied to the Perth Infirmary, and had bougies passed, with the effect of relieving him. Five months having subsequently elapsed, I found the urethra perfectly free, and introduced instruments of the full size without any difficulty.

(*To be continued.*)

Part Second.

REVIEWS.

Manual of Botany, &c. By JOHN HUTTON BALFOUR, M.D. Second edition. Griffin & Co., London. 1851.

Encyclopædia Metropolitana, &c.; Second Division—Applied Sciences —Botany. Griffin & Co., London. 1851.

THESE two titles belong to one and the same book, a second edition of Dr Balfour's well-known and deservedly popular manual being made to serve as the volume on Botany in the Encyclopædia. To such an arrangement there can be no objection, provided that this so-called second edition is such in reality, and that it has been duly corrected and brought up to the actual state of our knowledge. But such is not the case in the present instance. This may be called a second edition; but it is in fact little else than a reprint, with this remarkable peculiarity, that most of the alterations made in it have only served to introduce errors. It almost seems as if this reprint had developed itself by a kind of spontaneous generation. It has lots of title-pages, and bears the preface of

the first edition, with the date omitted; but it has no preface of its own to explain in what respect it differs from the first, and might be a thing of spontaneous, we may certainly say of equivocal, generation, but that at page 613, a mysterious personage, subscribing himself "Editor," makes his appearance, in connection with a parenthetic sentence, containing an advertisement of certain microscopes manufactured and sold by the publishers of the volume.

We leave it to those journals, whose province it is more particularly to attend to the interests of natural science, to expose, if they think fit, the errors which this book contains. All that we purpose to do is to protest, as an Edinburgh journal, against the supposition that this book indicates either the actual state of botany among us, or the way in which science is cultivated here. We hope it will not be thought that our ideas of a second edition are, that it is good enough, though it may not contain a syllable regarding all that has been done for two years back. It may be all very well for the subscribers to the Encyclopædia Metropolitana to be supplied with such musty information, and if they are content, we, not being of their number, have no objections to offer,—*chacun à son gout*; we like our information on science to be fresh, even though it should make the reading a little tough; theirs is well kept, and ought to be tender, and so we wish them a good digestion of it.

It behoves us, however, to explain to our readers that in making these remarks we have no blame to lay at the door of the Professor whose name the book bears. He has himself, publicly in his class-room, and also at a meeting of the Botanical Society, where he presided, disclaimed all connection with, and responsibility for, this edition of his Manual; he is content to bear the onus of whatever errors the original edition contained, but he is not chargeable with the faults, either of omission or commission, which pertain to the second; and in justice to him we must shortly explain how this comes to be.

When Dr Balfour published his first edition, he was, though an old botanist, only a young author; however well acquainted with the scientific arrangements of the vegetable kingdom, he knew but little of the mysterious commercial arrangements of the republic of letters which refer to the sale of copyright. With respect to these, he was in what we shall, since writing on a botanical subject, venture to call a state of chlorophylle—in short, "green"—and he speedily found that, when he had sold his first edition to his publishers, the terms which he had made were such that he had unwittingly parted with the entire copyright of his work. When a second edition was called for, by the rapid sale of the first, he found himself in the predicament of being called upon to superintend its preparation on terms with which he could not comply. Under these circumstances, and acting under the advice of his friends, he had no option but to abandon his Manual altogether, and leave it to those who had in this way acquired it, to make of it what they liked.

What they have made of it may be gathered from our opening sentences. Being unable to compel Dr Balfour himself to edit it, they have employed somebody to do the work in his place; and the qualification of this unknown "Editor" to superintend a botanical publication may be judged of by one instance, which is too startling not to have attracted, as it has already done, the notice of botanical reviewers elsewhere.

There is a remarkable set of parasitical plants called Rhizantheæ, which agree in many points of their general structure with fungi, but which are, in respect of their reproductive organs, closely allied with endogens. They have been a sad puzzle to systematic botanists; and in order to find a resting-place for them, Lindley has made of them a separate class, Rhizogens, between Thallogens and Endogens, in which, however, he has not been followed by some other botanists of authority, as Robert Brown and Griffiths. The "Editor" of the present book, apparently attracted by a sentence in the original edition mentioning this arrangement of Lindley, but totally neglecting all the rest that was said regarding them, seems to have been inspired with a hidden wish to do something remarkable with them, and he certainly has succeeded to his heart's content. They were to be placed, according to Lindley, as a class, between Endogens and Thallogens, and there he certainly does put them, not as a class, however, but as an order, and under the last tribe of the Endogens, the Glumaceæ,—the Sedges, and Grasses. In short, he does not know the difference between a class and an order, and puts "parasitical plants, destitute of true leaves," whose "stem is either an amorphous fungous mass, or a ramified mycelium," which are in "no instance of green colour," and the "true nature of whose seeds is in most species quite unknown," with the long-stemmed, long-leaved, fresh-green, large-seeded Carices and Grasses. We do not know whether the "Editor" has ever tried his hand on any thing zoological, but if he has or does, we need not, after this, be surprised if we should find in his classification the cameleopard keeping company with a generation of tortoises, or the hippopotamus agreeably disporting himself with a family of lobsters.

Of omissions the catalogue is endless; little that originally was or has become erroneous is corrected, and everything new is omitted in every department. To take one example, which is of interest to us in a medical point of view, the yellow Cinchona still stands in the list of barks as being furnished by an unknown tree, whilst nearly about the time that the first edition of Balfour's Manual was published, Weddel's researches were given to the world, by which it was made known that this valuable drug was yielded by a new species of Cinchona, which that gentleman named C. Calisaya. The carelessness with which even the mere mechanical work of superintending the reprint has been done is conspicuous. Thus in noticing the Narthex Assafœtida, which is growing

in the Edinburgh Botanic Garden, the plant is stated to be two years old. That was true in 1848, when the earlier part of this Manual was published. It of course is not so now; so that this interesting plant stands still in point of years, however progressive in point of growth, somewhat like a lady of a certain age of whom we have heard, who has returned herself as 30 in three successive decennial censuses.

But we need not pursue this farther. Enough that we protest that this edition is not to be taken as a sample of Edinburgh scientific literature, being, as it is, neither as a Manual nor as an encyclopædic treatise, a correct representation of the present state of botanical science.

Die geburtshülfliche Praxis, erläutert durch Ergebnisse der II. Gebärklinik zu Wien, und deren stete Vergleichung mit den statistischen Ausweisen der Anstalten zu Paris, Dublin, u. s. w. Von Dr F. H. ARNETH, Assistent an der II. Gebärklinik zu Wien. 8vo. Pp. 254. Wien. 1851.—[*Practical Midwifery, illustrated by the Experience of the Lying-in Hospital at Vienna, and by a Comparison of it with the statistical Results of the Institutions at Paris, Dublin, &c.* By Dr F. H. ARNETH, Assistant in the Lying-in Hospital at Vienna.]

THE Vienna Lying-in Hospital is, we believe, the largest obstetric hospital in the world. On an average, above 7000 women are now delivered in its wards annually. Since its first establishment in 1784, upwards of 200,000 mothers have been delivered within its walls. Consequently the field for observation which such an institution affords is immense. The midwifery hospital is divided into two departments; one for the instruction of female pupils, and the other for the instruction of male medical pupils. Dr Arneth's Report consists principally of an account of the practice in the wards devoted to the instruction of the female pupils from October 1847 to October 1849. During these two years, when Dr Arneth was resident physician in the hospital, 6527 mothers were delivered in the division of the institution under his charge. His volume contains a clear and interesting summary of the results of his observation and practice in these 6527 cases. And the work is executed with such talent and zeal as reflect the highest honour on the medical school of Vienna, and lead us fondly to augur many more rich contributions to midwifery, from the very able and practical pen of the same author.

In the proceedings of the Obstetric Society[1] will be found an

---

[1] Want of space compels us to postpone this paper of Dr Arneth's, together with the Obstetric Society's Proceedings, till our July Number.

excellent digest, furnished by Dr Arneth, of the more interesting results contained in his work. It is consequently unnecessary to insert here any part of that analysis. And in his article in our present Number, he has stated so fully his views regarding the contagious propagation of puerperal fever, that we abstain also from appealing to the pages of his work on that important topic.

To add, however, to the completeness of Dr Arneth's analysis of the practice of the Vienna Hospital, we shall insert here, from the Vienna "Zeitschrift der Gesellschaft der Aerzte," for September and October 1850, a summary which is there given of the obstetric practice and operations in the whole hospital for the year 1849, the year subsequent to the date of Dr Arneth's Report; the wards under the male and female pupils both included.

During the currency of the year 1849, 7430 mothers were delivered in the Vienna Lying-in Hospital; 3731 male children were born alive, and 3534 female children. There were 250 children still-born,—viz., 140 boys, and 110 girls. Of twin cases there were 85.

In the delivery of these women, the following operations were had recourse to:—

The forceps were used in 59 cases, in consequence of the inertia of the uterus. Of the 59 mothers, 2 died; and 54 children were born living.

The forceps were used for pelvic contraction in 22 cases: 8 of the mothers died, and 13 of the children were born living.

The forceps were used in 1 case of placenta prævia. Both mother and child did well.

In 10 *cases of eclampsia* the mothers did well: 3 of the children were still-born.

The forceps were used in three cases of funis presentation. The 3 mothers recovered: 2 of the children were still-born.

The forceps were applied in 11 cases of pelvic presentation, in order to extract the head after the body was born: 4 of the mothers died, and 2 of the children were still-born.

Craniotomy was performed with Smellie's scissors in 7 cases: 4 of the mothers died. It was performed with Joerg's trepan and the cephalotribe in 2 cases. Both the mothers did well. We may state in passing that the supposed advantage of the trepan, which is now used by various continental practitioners, is, that it makes an aperture into the cranium, which is not so broken and apt to throw out spicula of bone, injuring the maternal passages, as when a small, irregular, broken, opening is made by the common craniotomy scissors.

Cephalic version was had recourse to in 11 cases. All the mothers did well, and all the children were born alive.

Turning was resorted to in 1 case of contracted pelvis. The mother did well. The child was still-born.

Turning for presentation of the arm was employed in 34 cases: 2 of the mothers died: 29 of the children were born alive.

Turning was resorted to in 10 cases of placental presentation: 2 of the mothers died: 5 of the children were born living.

Turning was employed in 1 case of eclampsia. The mother died, and the child was still-born.

Turning was used in 1 case of rupture of the uterus. Both mother and child died.

Turning was employed in 2 cases of funis presentation. Both the mother and children did well.

Artificial Reposition of the funis was employed in 14 cases. All the mothers did well, and all the children were born alive.

Artificial extraction of the child was employed in 139 pelvic presentations. All the mothers did well: 7 children were still-born.

The Cæsarean section was resorted to in 1 case of greatly deformed pelvis. The mother died. The child survived.

The os uteri was incised in 1 case of morbid degeneration. The mother died. The child was still-born.

One of the most interesting chapters in Dr Arneth's volume, and one which has evidently cost Dr Arneth no small degree of research and trouble, is an elaborate section on the maternal and infantile mortality attendant upon those obstetric operations which are had recourse to for the extraction of the infant in morbid and difficult labours. Setting aside three or four cases of operation by the Cæsarean section, gastrotomy, and symphesotomy, given in the Parisian reports, we find that, according to Dr Arneth's researches, the following are the results to the mothers and children, from the other modes of artificial delivery adopted, viz., by the forceps, vectis, craniotomy, and turning, in the three largest lying-in hospitals of Europe, as respectively reported by Boer and Arneth relatively to the Vienna hospital, by Lachapelle and Boivin relatively to the Paris hospital, and by Drs Collins, M'Clintock, and Hardy, relatively to the Dublin hospital.

1. The proportion of cases of operative or artificial delivery of the child is very nearly the same in all these three great hospitals. In the Vienna Hospital under Boer, 1 out of every 55 women was delivered by one of the operations we have named, namely, by the forceps, vectis, craniotomy, or version; and during the time of Dr Arneth's report, operative delivery was resorted to in 1 out of every 69 cases. In the Paris Hospital, Madame Boivin reports 1 out of every 61 labours as requiring delivery by operation. Madame Lachapelle found that in the 10 years preceding 1810, 1 in 57 mothers was delivered artificially, and during the subsequent 10 years 1 in 82 required such a procedure. In the Dublin Hospital, Dr Collins reports 1 out of every 86 women as having

been delivered artificially; and Drs M'Clintock and Hardy describe 1 out of every 52 of their cases as having been similarly assisted.

2. Though the total proportion of operations is thus not very different in these three large hospitals, yet the results to the children are very diverse. We shall give these results in round numbers, and without copying the minute fractions that Dr Arneth has appended. In the Vienna Hospital, in his operative deliveries, Boer lost nearly 1 in 2 of all the children; Arneth, 1 in 3 (or 33 out of 95 cases); in the Paris Hospital, in the same kind of cases, Lachapelle reports less than 1 in 2 of all the children as having been still-born (or 207 were still-born out of 541 deliveries); while Madame Boivin reports about 1 in 4 of the children as having been lost (95 having been still-born in 334 artificial deliveries). In the Dublin Hospital, in his cases of artificial or operative delivery, Dr Collins reports about 3 in every 4 children as having been lost (150 of the children out of 193 cases having been still-born); and Drs M'Clintock and Hardy return, under the same circumstances, a nearly similar amount of infantile mortality, out of 128 operative cases reported by them, the infants being still-born in 98 instances. Or perhaps we may state these results, as regards the infantile mortality in operative cases, more intelligibly in per centage proportions. Taking this method, the results are as follows:—Out of every 100 operative cases, Boer lost about 47 of the children; Arneth, 34; Lachapelle, 36; Boivin, 28; Collins, 77; and M'Clintock and Hardy, 76.

3. The maternal mortality in operative cases is a matter of even still greater moment and greater diversity. Unfortunately we have no records on this point from the Parisian hospital; as Lachapelle and Boivin have not published any general statistics relative to the fate of the mothers. The maternal results, however, in these cases, are given in sufficient fulness in both the Vienna and Dublin reports. Boer lost 1 out of every 17 mothers in his cases of operative delivery; Arneth 1 out of every 9. In his cases of operative delivery, Dr Collins lost 1 out of every 4 mothers; and Drs M'Clintock and Hardy nearly 1 out of every 5. Or, to state the result in per centage proportions, out of every 100 operative or artificial deliveries, Boer lost 6 mothers; Arneth 12; Collins 25; and M'Clintock and Hardy 22.

As some explanation of these differences in cases of morbid labour, in the results to the mothers and children, we may state one fact, without entering further into the discussion of the subject. In almost all, or indeed all, operative cases in the Dublin Hospital, in which the head of the child has not completely passed down into the pelvic cavity, craniotomy is employed,—an operation always, of course, fatal to the child, and extremely dangerous to the

mother. In the Vienna Hospital, on the other hand, they deliver the child by turning, and not by craniotomy, when the head is above the brim; and they apply the forceps in other cases, where the head has *partially* descended through the brim, but which in Dublin would still be regarded and treated as crotchet cases.

To all who are interested in the progress of midwifery, and more particularly in the statistics of obstetrics, we beg strongly to recommend Dr Arneth's excellent volume. And, in conclusion, we cannot but express our regret, first, that such statistical returns from our larger hospitals should not appear more frequently, and, secondly, that some general and uniform system of statistical observation has not yet been agreed upon among the accoucheurs of the different lying-in hospitals of Europe and America,—an arrangement that would add greatly to the practical value and importance of such reports.

The Anatomy of the Human Gravid Uterus exhibited in Figures. By WILLIAM HUNTER, Physician Extraordinary to the Queen, Professor of Anatomy in the Royal Academy, &c. Printed for the Sydenham Society. 1851.

DR HUNTER'S magnificent plates, illustrative of the anatomy of the gravid uterus and its contents, were first published in 1775. The original work, however, was very costly in its execution, and expensive in its purchase. And we do deem the medical profession greatly beholden to the Sydenham Society, for reproducing this beautiful work in its present faithful and available form.

"Anatomical figures," says Dr Hunter, in his preface, "are made in two different ways: One is a simple portrait in which the object is represented exactly as it was seen; the other is a representation of the object under such circumstances as were not actually seen, but were conceived in the imagination."

Most of the plates of the gravid uterus and its contents, that had been published before the time of Hunter, belonged to this second class. The plates of Albinus, for example, which were long greatly in vogue, are mere imaginative plans and schemes, full of grave errors. On the other hand, all the figures in the thirty-four folio plates of this volume of Hunter's, with the exception of the three figures in the last plate, are genuine and correct copies taken from nature, by some of the best artists of the time, and engraved by the best burins of that day. Two of the plates were engraved by that great master of his art, Sir Richard Strange; and one of these plates, the sixth of the present volume, representing the position of the fœtus in utero, is, as a mere work of art, perhaps the most beautiful anatomical plate that has ever been given to the world. We have heard it related, that Strange worked upon this plate with his own hands for six months, and ultimately refused any remuneration for his labour, presenting it as an offering to the friendship of

Dr Hunter, and as an *artist's* contribution to the advancement of anatomical science.

We have just stated that Hunter's plates are genuine and faithful copies from nature. But one curious and qualifying remark must be appended. The celebrated sixth plate, to which we have just alluded, "representing," to quote Dr Hunter's own words, "the child in the womb in its natural situation," does certainly not represent the child in the position which it commonly and naturally occupies. Anatomists and accoucheurs are now well aware that the most common position of the infant in utero is, with its occiput turned towards the left acetabulum, and its forehead or face turned towards the right sacro-iliac synchondrosis of the mother, and that this is the position of the infant in about 70 out of every 100 cranial presentations. In the remaining 30 per cent. of the presentations of the head, the direction of the occiput and forehead is reversed, the forehead being directed towards the left acetabulum, and the occiput towards the right sacro-iliac synchondrosis. The late lamented Professor Naegle, of Heidelberg, to whom obstetric science is so deeply indebted for a thorough knowledge of the mechanism of labour, averred that he had not met with the forehead or face looking towards the left sacro-iliac synchondrosis of the maternal pelvis in more than in about 1 in a 1000 labours. Yet it is this rare and exceptional position of the fœtal head that Dr Hunter's plate represents as "the natural situation" of the child in utero. In copying this plate, Professor Kilian, of Bonn, has, by an accident on the part of the engraver, reversed the position of the child, and thus made it correct. And we long fancied that probably the rare and unusual position of the infant represented in Hunter's plates, resulted originally from an accident of the same kind on the part of the engraver. But some years ago, we had an opportunity of inspecting, in the Hunterian Museum of Glasgow, the masterly original drawing that Rymsdyk had made of this subject for Hunter, and from which Strange's engraving was taken. The unusual situation of the fœtus exists in the original drawing of Rymsdyk; and is seen also in a cast in the museum taken from this patient. And hence it is evident, that Dr Hunter has, in the engraving, represented accurately the position of the fœtus, such as he found it in the patient whom he was dissecting. But the situation of the fœtus in that patient was, instead of the natural situation of the fœtus, a position of unusual rarity. Speaking of this plate, Dr Hunter observes, "every part of the child is represented just as it was found, not so much as one joint in a finger having been moved to show any part more distinctly, or to give a more picturesque effect." The mother from whose body this plate was taken, died suddenly at the end of the ninth month of pregnancy. The left arm of the fœtus is displaced from its usual situation, and the head is thrown more backwards upon the neck than usual,—changes which probably were the result of the movements of *agonie* in the infant at the time of

its death. Is the unusual position of the head of the fœtus, in relation to the maternal pelvis, owing to the same cause?

Every figure in these plates is worthy of careful study; but we have no time to allude to more of them in such a brief notice as the present. Let us merely repeat, that we greatly commend the Sydenham Society for placing, in such a cheap form, in the hands of its members, a work of such indubitable value as the present.

In a prefatory notice, the council of the Society tells us that they have obtained possession of Dr Hunter's original copper-plates, and that the present work is cast off from them. We would fain ask, from whom did they obtain these copper-plates? Dr Hunter was obliged to defer for a week or two the commencement of his third course of lectures in London, in consequence of not being possessed of the small sum of money requisite for the expenses of the usual advertisements. At his death, however, he was rich enough to leave a museum to the University of Glasgow which had cost him in its collection upwards of L.120,000; and he bequeathed to that University L.8000 for the support and augmentation of it. Amongst other articles, his books, drawings, and plates, were left also to the University of Glasgow. We know that the Sydenham Society did not obtain the present copper-plates from the University of Glasgow; and we repeat, from whom, then, did they receive them? We would advise the authorities of the University of Glasgow to look to this point, as the copper-plates should undoubtedly be in their possession.

We cannot but think that, in re-editing Dr Hunter's great work, the Society have fallen into two errors. Dr Hunter published, in his original edition, the explanation of his plates both in English and in Latin, because the work was expected to circulate abroad as well as at home, and English was not then understood to the same extent by continental anatomists and physicians as it is at the present day. But most assuredly there was no such reason as this for the Sydenham Society publishing, as they have done, a Latin description of his plates, in addition to their English description, seeing the present edition is intended for circulation and use among the members of a Society composed of English medical practitioners. Such a proceeding in the middle of the nineteenth century savours too much, we opine, of pedantry. It lacks, even in support of it, some of those ridiculous reasons that are still held up by old medical protectionists for publishing the London Pharmacopœia in Latin. And we cannot but fancy that it was so far an unnecessary expenditure of the funds of the Society. Besides, we do think the funds thus expended in printing the Latin translation of the English context to these plates, would have been far more profitably employed in reprinting, as an introduction to the volume, Dr Hunter's most excellent work, entitled "Anatomical Description of the Human Gravid Uterus, and its Contents." This "Description" was first published as a small quarto volume by Dr Baillie some

years after Dr Hunter's death. In editing that posthumous work, Dr Baillie observes,—

"No regular description of the anatomy of the gravid uterus had accompanied these plates, but the plates themselves were merely explained. Dr Hunter had intended, however, to make up this *deficiency, so as to render the whole work complete;* he had made a promise to this purpose in the preface to the large volume of his engravings, and he has left behind him a manuscript containing a description of the gravid uterus and its contents, which he has not quite finished; what appeared to me to be wanted, I have attempted with much diffidence to add, and this amounts only to a few pages."

Seeing that Dr Hunter himself conceived this "Description" to be necessary to render his work complete, in re-editing it, the Sydenham Society would have followed out only Dr Hunter's own wish in printing this short but most masterly work along with, and as a preface to, the plates. It would have added greatly to the value and importance of the work, without adding much to the expense of its production.

Part Third.

CLINICAL REPORTS, LECTURES, ETC.

CLINICAL MEDICINE.—PROFESSOR CHRISTISON.

BRIGHT'S DISEASE OF THE KIDNEYS.

(*Delivered March 11th and 18th*, 1851.)

As my colleagues, to whom I lately succeeded in the usual order of clinical duty, have not been able to bring the subject of Bright's Disease of the Kidneys under your notice, and an interesting case of it has just made its appearance in the person of the patient Flint, long in the hospital for lupus, I have requested the admitting physician to send us, for your instruction, any applicants who may present themselves in the waiting-room, with this disease; and in the meantime, I shall now give you a general sketch of what is known on the subject, but dwelling principally on the microscopic anatomy of the disease and its treatment, because in these two departments a good deal has been recently said and done, of which you cannot find a methodical account in any publication.

Let us, in the first place, understand one another as to what is meant by the term Bright's Disease of the Kidneys. You probably know that neither practitioners nor pathologists are as yet agreed about a scientific name for the disease. It seems very probable that pathological anatomy will presently supply that desideratum, and so reconcile all differences of opinion as to nomenclature. But these differences will be all the more easily reconciled by-and-bye, if we do not rashly affix a pathological name, or pathological names, for the present. In the meantime, therefore, let us use the vague, and because vague, the convenient, name of Bright's Disease, but with such definition as will leave no doubt how much we are to understand it to embrace. For some writers, such as Professor Gluge, attach the phrase merely to the congeries of outward symptoms, essential

and secondary; others, such as Dr Simon, to the morbid anatomical changes in the kidneys producing these symptoms; others, such as Dr George Johnson, to one special morbid change only, the fatty degeneration of the kidneys. For my part, I shall always understand to be comprehended under the expression "Bright's Disease," not only every pathological change in the kidneys, which may give rise to the concatenation of symptoms first methodised, and traced to diseased kidneys as their cause, by Dr Bright, but likewise every variety of combination in which these symptoms may present themselves in practice. In no other acceptation can the term be used with justice to him from whom it is derived. Nor is there any inconvenience in employing it in so wide a signification; for, on the contrary, we shall thus be more able to supply, without ambiguity, a scientific nomenclature for those branches of this evidently comprehensive subject of inquiry, which may be successively cleared up by the labours of the pathologist.

In what is to follow, I shall first sketch briefly the history and bibliography of the subject; then I shall describe the symptoms, more or less essential, of the fundamental renal disorder, together with the various secondary diseases which are apt to be developed during its course; I shall next treat of its causes, so far as this obscure department of the subject is known; the prognosis, derived both from the fundamental symptoms and from the secondary affections, will next engage our attention. You will then be prepared to follow what is to be said of the morbid anatomy and pathological nature of the disorder, and I shall conclude with a summary of the treatment, and some evidence of its curability in at least some of its forms.

I. HISTORY AND BIBLIOGRAPHY.—Almost every great discovery in medicine, as well as in science at large, has been ushered in by certain precursory indications; and so it was with the discovery of Bright's disease of the kidneys. But no other pathological discovery of equal importance has been referred with such universal consent, and so entirely without deduction, to its original author. It was first announced by him in his Hospital Reports in 1827. For some time medical men were slow to believe that the multitude of pathologists who, for many years before, had been zealously scrutinising, in all parts of Europe, the morbid changes of structure in every important organ of the body, could have missed a disease described by Dr Bright as so discoverable by its symptoms, so obvious in its anatomical characters, and of such frequent occurrence in medical practice. Two entire years indeed elapsed before his statements were confirmed by any other observer; and the first public confirmation proceeded from this Infirmary. The subject, however, was then speedily taken up at every considerable hospital, whether in this country or abroad, and everywhere with the same result. A supposition next arose, and in some quarters became prevalent, that the malady was one peculiar to the poor and working-classes alone, because it seemed to be met with only in hospital practice. But this was found ere long to be a great mistake,—and now no physician entertains any doubt that Bright's disease occurs alike in all ranks of life, and at all ages, or that it is one of the most common of organic diseases, and also one of the most important, both on its own account, and likewise because it is very apt to be mixed up intercurrently with other diseases.

Many have publicly reported their experience since Dr Bright's first work appeared. * * * * But, passing from the authors of these writings, I have to mention more particularly, that within a few years various microscopic observers have studied with great diligence the intimate changes of organic structure which constitute the diseased condition of the kidneys, and have by their researches thrown a clear light on several branches of the pathology of the subject, which were previously involved in great obscurity. Among these the most conspicuous has been Professor Gluge of Brussels, whose researches have appeared in his "Atlas der Pathologischen Anatomie," at different periods between 1837 and 1844. Next in importance to these are the inquiries of Dr George Johnson of London, who has confirmed in all important particu-

lars the observations of Gluge, and extended them in some quarters; and this confirmation is all the more satisfactory, that his inquiries seem to have been carried on in ignorance of the prior researches of the professor of Brussels. These observations being contained in the "London Medico-Chirurgical Transactions for 1846," are easily accessible to you all, and I would therefore recommend them to your attentive study. In the same volume of that work, and in the volume for 1847, you will also find some interesting confirmations and additions in two papers by Mr Toynbee and Dr Simon. And lastly, for an account of some valuable microscopic researches, conducted in this hospital by Dr Wm. Gairdner, I beg to refer you to the volume of the "Monthly Medical Journal for 1848." Under the head of the pathology of the disease, I shall give you a summary of all these microscopic investigations.

II. Symptoms and Secondary Affections.—On this head I shall not enter into many details, because you will find full information on the subject in the work I published in 1839, "On Granular Degeneration of the Kidneys."

The *Primary Disease* occurs both in the acute and in the chronic form,—the latter being of most frequent occurrence. In the acute form there is sometimes pain in the region of the kidneys, seldom however severe, and often wanting; more generally pain or tenderness across the region of the stomach; disturbed digestion, thirst, and not unfrequently vomiting; general re-action of the circulation; leucophlegmatia; anasarca, usually general, but worst in the limbs; frequent micturition; and a change in the condition of the urine, which is at times bloody, often turbid even when just passed, always more or less coagulable by heat and nitric acid, and generally shows after some hours' rest a fleecy sediment, in which the microscope recognises blood-corpuscles, degenerated epithelial cells from the mucous membrane of the uriniferous tubes, and fibrinous casts of these tubes, to be afterwards described more particularly. In this form the density is not low until the disease has subsisted for some time; on the contrary, it is at first, though by no means always, of natural density; and on one occasion I found it to be so high as 1037, and on another 1046, although there was no sugar, but an enormous amount of albumen. The blood is usually buffy, often excessively so; the density of its serum is much reduced if the urine has been for some days highly loaded with albumen; urea may be always discovered in it, if the urine has been for some time greatly reduced in quantity; and when the disease has lasted for a week or two, the proportion of hæmatosin in the blood is found to be diminished. But in the commencement nothing unusual is remarked as to the blood, except buffiness. The symptoms of the acute form are liable to great irregularity, the least variable being frequent micturition, leucophlegmatia, coagulable urine, thinness of the serum, and buffiness of the blood.

The chronic form of Bright's disease is sometimes the concluding stage of the acute form. But more generally it is slow, obscure, and insidious in its approach and progress; so that it is often found to be very far advanced before its existence is suspected. For a long time, in many cases there are literally no symptoms to catch even the patient's attention; or if there be, they are those of some of the secondary affections, such as dyspepsia, diarrhœa, or rheumatism, which rather mislead than direct him to the fundamental disease. The most frequent symptoms, more or less referable to the primary disease, are frequent micturition, sometimes scanty, sometimes superabundant, and disturbing the patient at night; leucophlegmatia, or rather a dingy bloodless hue; progressive emaciation and debility, listlessness and drowsiness, yet without sound sleep; sickness in the morning before breakfast, a failing appetite, and considerable thirst. These symptoms, however, are seldom well marked until the disease has made considerable progress; and its state of progress is proved by the condition of the urine, which is found to be pale in colour, low in density, more or less coagulable by heat and nitric acid, and turbid when passed, owing to a fleecy matter, which remains very long suspended, does not entirely settle, but forms a sediment presenting various combinations of blood-globules, pus-globules, oil-globules, degene-

rated epithelial cells, and fibrinous tube-casts, sometimes also with the ordinary deposits of healthy urine. The blood too is much altered, the hæmatosin being reduced, and often excessively, so that I have known it brought down from 13 or 14 per cent, its natural proportion, to 5·3, and even 4·2 per cent. The serum, however, is not in general much altered, its albumen being as abundant as in health, or even more so, unless the urine be much loaded with that principle, an occurrence which is not very common in the chronic form of the disease. Urea can only be detected in the serum when the urine has been for some time very scanty; but in such circumstances I have never failed to find it.

As the disease advances, whether in its chronic or acute form, the urine becomes more and more scanty, paler and paler, and lower and lower in density. In some remarkable cases I have seen it reduced to the density of 1004, and to a quantity not exceeding four or six ounces daily, for some time before death. When this happens, death generally takes place by slowly-supervening coma; which may therefore be regarded as the natural mode of termination of Bright's disease, when uncomplicated. But cases of this nature seldom present themselves to observation, as it is far more common for the patient to be carried off by one or another of the secondary diseases.

I am sorry that our time will not allow me to enter into a full consideration of all these symptoms. But it is necessary for me to do so in regard to one of them, which is usually considered and employed as the pathognomonic sign of the disease. This is coagulability of the urine. In the first place, you must make sure that the coagulability is owing to albumen, otherwise there may be no Bright's disease, nor any thing more than functional disturbance. Therefore you should always employ both heat and nitric acid as tests; and the best way is to heat the urine first, and then to try whether the coagulum resists nitric acid. Nitric acid alone may cause a precipitate of uric acid, when urate of ammonia is superabundant; heat alone may separate earthy salts when these abound; but a coagulum which resists both heat and nitric acid can be nothing else than albumen. In the second place, you must not take it for granted that Bright's disease is positively present, merely because the urine contains albumen. The urine is albuminous, when the patient labours under pyelitis or cystitis, that is, inflammation of the mucous membrane of the kidneys and bladder; because pus is freely thrown out in these diseases; and whenever pus is secreted by a surface, serum must be secreted along with it.[1] The urine is also apt to be albuminous for some time after an attack of hæmaturia, whatever may be its cause; for the relaxation of vessels, which gives occasion to the escape of blood, is seldom so quickly put an end to, but that serum escapes for some time after blood has ceased to flow. A few days ago I saw a remarkable case of purpura hæmorhagica of the active kind in a gentleman of fifty, whose urine, two days after it had ceased to be bloody, was extremely albuminous. The action of a blister on the skin, if it irritates the kidneys or bladder, generally causes a temporary albuminuria, as I have again and again had occasion to ascertain. Mercurial action also is sometimes attended with functional albuminuria, which disappears permanently as the ordinary symptoms of mercurialism subside. In some individuals the use of certain articles of food, somewhat difficult of digestion, seems to cause a secretion of albumen from the kidneys for a very limited period. It is not uncommon to observe a very slight impregnation of albumen in the urine of confirmed dyspeptics, although no other sign of organic disease be discoverable, and the functional character of the affection is shown in the end by the albumen disappearing, at first from time to time, and at last for good and all. I have at present a lady under my care with dyspepsia, in whose urine there is sometimes oxalate of lime, sometimes a little sugar, sometimes a very little albumen, sometimes all three at once, sometimes none of these adventitious matters; nor does

[1] I have several times, however, known albumen disappear from the urine in the progress of cure of cystitis, while the pus-globules continued for some time abundant.

she present any other indication of organic derangement; and certainly there is no Bright's disease.

There can be no doubt, therefore, that albumen may appear in the urine from other causes besides Bright's disease of the kidneys, and that sometimes it is a very trifling matter. But at the same time I would advise you to look carefully after all cases in which this symptom appears in connection with mercurialism, dyspepsia, or dietetic irregularities. Two of the most decided cases of Bright's disease that I have encountered, commenced during mercurial action, excited in the course of the treatment of pneumonia; dyspepsia is one of the most frequent of the early symptoms of obscure chronic cases of the disease; and, when albumen is readily made to appear in the urine by peculiarities of diet, there may be reason to suspect that the irritation thus excited in the kidneys is near akin to that which leads to organic degeneration, and may subsequently pass into it. A medical friend, who had been long a martyr to dyspepsia and neuralgia, but enjoyed otherwise tolerable health, once told me jeeringly, that we had over-rated the importance of albuminuria as a symptom of organic disease, because his own urine was never free of albumen. This statement, however, did not convince me of error; on the contrary, I thought that his dyspepsia, neuralgia, and dingy complexion might possibly be soon too well accounted for. Within two years he was seized with what was at first mistaken for bilious fever by his medical attendant, but which ended in gradual diminution of urine and fatal coma; and after death, the kidneys were found in a very advanced state of granular degeneration. So, too, a young graduate of this University, who was often made the subject of experiment by the late Dr James C. Gregory of this hospital, in his inquiries into the quantity and density of healthy urine, and who could at any time impregnate his urine with albumen, by eating rather freely of cheese or pastry, fell about two years afterwards into decided Bright's disease, and died of it in no long time.

The *Secondary Diseases*, which are apt to complicate Bright's disease of the kidneys, are of great importance, both on account of their number and seriousness, and likewise because they are the most frequent immediate cause of death, and often constitute the principal subject of treatment. They are apt to occur equally in both the acute and chronic forms of the disease, but some of them chiefly in the one, and some chiefly in the other.—The most frequent is *Dropsy*. Some indeed maintain, that dropsy of one kind or another is an essential symptom of Bright's disease, and never wanting altogether. But this is certainly a mistake, according to what has come repeatedly under my own observation. There is no doubt, however, of the frequency of dropsy, and that œdema of the limbs or face is the most frequent of all symptoms to raise the first suspicions of the existence of renal disease. Dropsy occurs usually in the form of anasarca, more or less general, commonly pitting, sometimes elastic, especially in the acute varieties of the malady; and when serous effusion into any of the serous sacs is a prominent part of the dropsy, it is always observed that there is some additional organic disease, such as disease of the heart if there is much hydropericardium; disease of the heart or lungs, if there is much hydrothorax; and above all, disease of the liver when there is much ascites. The presence of dropsy always exasperates every other secondary affection; its removal invariably gives the patient great general relief; if it obstinately accumulates, there is urgent danger; and when it cannot be removed, it may be the immediate occasion of death, either by gorging of the lungs, or by erysipelas and sloughing of the tense integuments of the limbs.—*Dyspepsia* is so frequent an accompaniment of the middle and final stages of the chronic form of Bright's disease, that I would advise you to make a point of examining the urine in all cases of protracted and frequently-recurring indigestion. It is surprising how often the obstinacy of this affection is thus accounted for. Sometimes it puts on the form of chronic vomiting, so that all articles of food and drink are ejected soon after being taken. In such an event the exhaustion is rapid; and I have once known even death thus arise, apparently from nothing else than inanition.—

Another common accompaniment in Edinburgh, but not so frequent, according to all accounts, elsewhere, is *Chronic Diarrhœa*, generally obstinate, sometimes fatal, occasionally depending on ulceration of the bowels, more generally on congestion and relaxation merely in the intestinal mucous membrane.—*Inflammations of the Serous Sacs*, viz. Pleurisy, Pericarditis, and Peritonitis, are common secondary attendants of the acute form, and are very easily developed during the chronic form also, under exposure to cold. They are apt to commence insidiously, are less easily controlled by treatment than in ordinary circumstances, and are a frequent cause of death.—No secondary disease is more often met with than *Bronchitis*, both acute and chronic; in hospital practice, indeed, few cases are entirely without it on admission. It is always a source of great additional misery to the patient, often most obstinate, always worst when anasarca is also present, and not unfrequently the immediate cause of death.—*Affections of the Head* are not so common as might be expected. Besides the terminating coma of those few cases which are not cut off by other diseases, we sometimes observe congestive and sanguineous apoplexy, more rarely paralysis, more frequently perhaps epileptic convulsions. According to Dr Osborne, meningitis is not unfrequent in Dublin; but it is certainly very rare here. The most remarkable and most important head affection is convulsions, when Bright's disease concurs with the puerperal state; for it has been fully recognised, in this city at least, that a large proportion of cases of puerperal convulsions are connected with one of the pathological forms of Bright's disease.—*Neuralgia* of the extremities is a very common and tormenting attendant of the chronic form; chronic articular rheumatism also occurs, but less frequently; and both are apt to be obstinate. It is singular that neither affection is apt to arise when there is considerable dropsy; but in the cases in which there is no anasarca, neuralgia is seldom wanting. The urine ought always to be examined in old rheumatisms. Often have I found by this precaution, that there was a far more serious disease to deal with, which had been entirely overlooked.—*Diseased Liver* is often observed intercurrently with diseased kidneys; nor is it in general easy to say which is the primary, and which the secondary disorder. The reason of the conjunction may be, that they have both of them a common pathological origin, and likewise a common exciting cause,—as will be explained by-and-bye. For the most part, the concurrence of diseased liver renders ascites a predominant dropsical affection.—*Hypertrophy* and *Valvular Disease of the Heart*, also very often present themselves intercurrently, and in the same circumstances, and probably for the same reasons, as disease of the liver. It is, indeed, not an unusual observation, to find heart, liver, and kidneys all diseased, and in such way as to render it difficult or impossible to trace the succession among them.—I do not think that *Phthisis* can be fairly regarded as a secondary affection. On the contrary, it is much more in accordance with my own observation, that disease of the kidneys is apt to occur secondarily to phthisis. In this way, however, they are often conjoined. During phthisis, there occur few cases of anasarca which I do not find to concur with coagulable urine, and other characters of renal disease; and I have repeatedly found the same state of things, when my attention was turned to the kidneys, not by anasarca, but by dyspepsia, neuralgia, or an unusually leucophlegmatic complexion.

III. CAUSES.—The causes of Bright's disease are obscure. The chief *Predisposing cause* is intemperance; and it is probable that this agent may gradually induce the chronic form without the aid of any occasional cause. The scrofulous diathesis is another predisposition. I stated this in my treatise on granular degeneration of the kidneys in 1839 as a probable proposition; but there can be no doubt now, that the disease is one of the maladies of the scrofulous diathesis. A strong predisposition is also engendered for a short time by scarlatina. The tendency is sometimes indicated even in the primary fever, by the urine becoming coagulable; in many cases it breaks out with unequivocal symptoms about the close of the third week; but if the patient escape for another week, he is generally safe. Until lately, many physicians doubted the connection, insisted on by

myself and a few others, between Bright's disease and the secondary disorders which follow scarlatina; but no accurate or unbiassed observer can well entertain any doubt now. The puerperal state is another predisposition, which has been established mainly by the observations made in this city by my colleague, Dr Simpson, who meets with few cases of œdema in advanced pregnancy, or of convulsions at that period, or during delivery, without finding that the urine indicates serious renal disease. I have had several opportunities of observing the same facts, both in consultation with my colleague, and likewise with other obstetrical practitioners. There can be no doubt of the connection, and the discovery is of great practical importance, because in pregnancy, as in scarlatina, the occasional examination of the urine may enable the practitioner to foresee and provide for approaching formidable dangers. It has been supposed that other febrile poisons besides scarlatina may have the same effect of engendering Bright's disease, or a liability to it; also that it may originate in the sudden recession of extensive chronic cutaneous diseases. But on these two points, the facts hitherto collected are too few to be trusted. Age, sex, and station, are no farther predisposing causes, than in so far as persons of a certain age, station, or sex, may be more exposed than usual to other predisposing, and also to occasional, causes of the disease. I have seen it at the most advanced period of life, as well as at four years, and others have seen it in the new-born; I have seen it often in the most comfortable, as in the most miserable, stations of society; and women are quite as prone to it as men. But without doubt it is proportionally more frequent in persons of middle life among the working classes of the community, than in any other circumstances. Of the *Occasional causes*, which have been indicated by various authors, the most unquestionable and most frequent is exposure to unusual cold. It is remarkable how often the acute form of the disease is distinctly referred to some decided exposure to cold; and I do not know a more frequent variety of such exposure than sitting down, while overheated, on the grass or a stone. I think I have observed some cases to arise during mercurial action, without any other apparent occasional cause; and Professor Gluge found it by experiment to be produced by mercurialising dogs. Martin Solon says he has traced it sometimes to blows on the loins; a very probable cause, but one which has never come under my own notice. Gluge has further ascertained, that one pathological form of the disease may be produced in dogs by confining them to a too fatty diet: which has therefore been inferred to be a possible cause in man also. Dr George Johnson found it to arise in animals kept closely confined in dark apartments, with little opportunity of exercise; which has been supposed to be an analogous connection with what is observed between this disease and the dingy confined habitations of the lowest orders of society. Finally, it is very probable that several of the causes which I have regarded above as merely predisposing agents, more especially intemperance, may develop the more insidious cases of the chronic form, without any other occasional cause; and that both scarlatina and the puerperal state may prove not only the predisposing, but likewise the occasional cause, in exciting the acute form.

IV. Prognosis.—The probable result of an attack of Bright's disease of the kidneys is unfavourable, often extremely so. Some have thought it a desperate malady, always fatal in the end, though often deceitfully persuading the patient and the physician that it has disappeared. At one time I had my fears on this head. But an opportunity of more prolonged observation has convinced me that many cases admit of a radical cure, and many others of being brought to a stand-still for a long period. There can be no doubt that many recover permanently in whom it originates after scarlatina. I think the greater proportion recover of those who are attacked in advanced pregnancy, or about the time of delivery. Complete recovery is rare among the intemperate, or those of strongly strumous habit, and is probably unknown in that pathological form of the disease which will be presently seen to be referable by pathognomonic signs during life to fatty degeneration of the kidneys. The unfavourable primary symptoms are, a scanty, pale, light urine, a very leucophlegmatic or dingy coun-

tenance, a chronic progressive emaciation, and above all a constant drowsy listlessness. A great proneness to secondary affections—especially to severe neuralgia, diarrhœa, chronic vomiting, and frequent dropsy—is also unfavourable; the conjunction of diseased liver or heart is always of evil omen. Dropsy for the first time, is not always an unpropitious sign; neither is bronchitis; and acute serous inflammations are no farther unfavourable, than by reason of their own proper risks, and the comparative difficulty of curing them when the kidneys are at the time diseased. It is particularly observable that all intercurrent diseases are more unmanageable and more apt to prove fatal in consequence of the renal disorder. The most mortal of all signs, are a very scanty urine of very low density, an extremely impoverished condition of the blood-corpuscles, and stupor verging gradually towards coma.

V. MORBID ANATOMY AND PATHOLOGY.—The most important morbid appearances in the kidneys are, in the early stage of acute cases, the signs of bloody congestion, both outwardly and in their intimate structure: in advanced cases of the same nature, enlargement; for the most part a lobulated, botryoidal, or granular, yet sometimes too a uniform surface, with injected reticulations of vessels in the hollows; and internally an invasion of the cortical and medullary structures, and diminution of their usually distinct striated appearance, in consequence of the effusion of a heterologous matter, generally of various tints of yellowish white, into which the material of an injection passes very imperfectly or not at all. In chronic cases, we may have the same appearances as in advanced acute cases. But often we find the kidneys not enlarged, sometimes even less than usual, occasionally atrophied to an extreme degree. Some have thought the atrophied state may be secondary to a state of enlargement from morbid deposition, this being subsequently absorbed; others conceive that atrophy may commence with the beginning of the disease; nor is it an easy matter to settle that question. In very slow cases, the adaptation of the constitution to the gradually degenerating structure of the kidneys may be such, that the patient is not cut off until there remains only a single tubular tuft in the medullary part of one kidney, and two, or perhaps a third, in the other. I have seen one kidney completely atrophied, and without any natural structure visible, and in the other an extensive heterologous deposit, leaving only two medullary tufts visible in a longitudinal section.

These morbid appearances are liable to many variations, which it is very difficult to describe, because it is not easy to define them. I must refer you for details to the original description of Dr Bright, as well as that in my own treatise, and above all to the accurate delineations of Dr Bright or M. Rayer in his work on diseases of the kidneys.

Dr Bright and his immediate successors were content with ascertaining the anatomical characters of the disease, so far as they could be ascertained with the unaided organs of sense. The result was a suspicion, which was strongly expressed by myself as well as others, that "future pathological research will probably show that there is more than one organic derangement" of structure concerned in producing Bright's disease [Preface, p. 15]. The fact is so. But it could scarcely have been proved without the aid of the microscope; which in this, as in other departments of pathology, has lately supplied most valuable information. It would take up too much of our time, were I to give you a detailed statement of the separate results which have been arrived at by the various microscopic observers who have successfully studied the subject. The principal observers have been Gluge, Goodsir, Johnson, Simon, Toynbee, and Gairdner. The most original observations are those of Gluge. The others coincide with him in all very essential points; but there are material differences in the views taken by each of some of the details; nor is this wonderful, considering the fallacies to which the microscopist's eye is subject, when he attempts to pierce deep into the minutiæ of organised structures. There is, however, a satisfactory coincidence of observation on the whole. And upon collating all the microscopic inquiries yet published with one another, and trying them by such opportunities as have occurred to me, I think we may arrive at a consistent doctrine, and one which is

tempting alike in a theoretical point of view and in its practical bearings. I shall now state this doctrine in a series of propositions. You are not to suppose I insist on these as matters of assured medical faith; but I already regard them as probable; and it is therefore well to have them distinctly enunciated as the basis of future investigation in these wards, as well as elsewhere:—

1. The congeries of alterations of the kidneys, described by Dr Bright and his more immediate followers in this line of inquiry, depend on at least two distinct morbid processes.

2. One of these is of the nature of inflammation, chronic or acute. The other is a morbid degeneration, always chronic, not inflammatory, analogous to what occurs in various other important organs, and consisting of the morbid accumulation of oil or fat globules.

3. The inflammatory disease in its acute form, although known to originate under various circumstances, and even independently of any pre-existing disease, is best and most frequently exemplified in the phenomena which occur in many instances subsequently to scarlatina, or in the puerperal state.

4. The chronic form of the inflammatory disease always originates obscurely, insidiously, and so gradually that, except in favourable circumstances, it escapes notice for a long time. But it ought always to be looked for by a prudent practitioner in cases of protracted or frequently-recurring dyspepsia, diarrhœa, neuralgia, chronic rheumatism, catarrh, anasarca, and those chronic cutaneous diseases which exhaust the constitution. And it is apt to present itself intercurrently with most other chronic diseases of the greater viscera, especially phthisis, diseased liver, and hypertrophy of the heart.

5. The inflammatory process, according to most observers, consists in a preternatural formation of epithelial cells in the lining mucous membrane of the minute uriniferous tubes of the kidneys, and the detachment or desquamation of these cells; so that, by accumulating in the interior, they obstruct the tubes and choke them up. There are also found in the obstructing matter, blood-discs, pus-globules, uric acid crystals, and sometimes crystals of oxalate of lime. But the greater part of the mass consists of epithelial cells, entire, or much more commonly degenerated, so as to be irregular in their edge, contracted in size, or sometimes reduced actually to their nuclei. According to Gluge, the morbid formation rather consists of the more ordinary secretions of inflammatory action, such as fibrine, pus-globules, and exudation-corpuscles; which not only accumulate in the uriniferous tubes, but are likewise thrown out in greater abundance throughout the surrounding interstitial texture.

6. The consequence is great obstruction to the secretion of the ordinary solids of the urine, and therefore a diminution of these solids, while the watery part may be long secreted as usual, or even in much more than the usual quantity. And the explanation of this interesting double phenomenon, which all must have often witnessed who are conversant with Bright's disease in practice, is that while the epithelial surface of the uriniferous tubes, considered by most physiologists to be the proper secreting tissue of the kidneys,—is grievously injured and diminished, the malpighian bodies,—supposed by some good authorities to secrete rather the watery part of the urine,—remain for a long time little or not at all affected. This doctrine is a beautiful part of the pathological theory of the disease; but it rests on an insecure foundation, as several microscopic observers maintain they have seen the malpighian bodies oppressed by congestion within and pressure outside from the distended uriniferous tubes, at an early period of the morbid process.

7. As a result of the secretion of the water of the urine, the obstructed urine-tubes are often swept free of the obstructing material. But it does not always follow that the tubes thus regain their secreting function; for they may have been permanently deprived of their epithelial cells, no longer renewed, or only in diminished number and in a degenerated form.

8. But an important consequence of this sweeping-out of the urine-tubes is, that we find in the sediment of the urine their morbid contents; and these are

blood-discs [Fig 2, *a*], degenerated epithelial cells, much resembling pus-globules, and still more mucous corpuscles [*b*], also nuclei of epithelial cells [*c*]; and above all, as characterising this particular form or variety of Bright's disease, entire or broken-down casts of the urine-tubes [Fig. 1, 2, 3, *d*], consisting of blood-discs, nuclei, and especially epithelial cells, all united together in cylinders and portions of cylinders by a transparent agglutinating medium, which can scarce be anything else than fibrine. This is one of the most important practical observations yet made in the course of the whole microscopic inquiry. For if exact, it enables us to discover most readily with the microscope, the nature of any particular case, and to refer it to inflammatory action as its fundamental source.

9. The other morbid affection of the kidneys, constituting also what is comprehensively called Bright's disease, is a peculiar degeneration, unconnected with any form of inflammatory action, and consisting in deposition of oil-globules in preternatural quantity in the epithelial cells of the urine-tubes, distending these cells, detaching them from the epithelium-wall, distending consequently the tubes by their accumulation, bursting the tubes here and there, and so escaping into the interstitial tissue.

10. The pathological consequences to the structure of the kidneys are here much the same as in the inflammatory form of the disease. The urine-tubes, gorged with oleiferous epithelial cells, become useless themselves in the process of secretion, and by pressure around impede the function both of the unaffected urine-tubes and of the blood-vessels. The secretion of urine is therefore much disturbed, in respect of its proper solids, and often too its watery part; but sometimes the watery portion is unaltered, or even superabundant. And whether abundant or scanty, it sweeps out from time to time the morbid contents of the tubes,—without thereby restoring, however, the healthy organisation of their basement membrane, or the healthy discharge of their function.

11. An important consequence, in a practical point of view, is that in the instance of oily degeneration, as in that of inflammatory deposition, the urine contains characteristic microscopic ingredients in its sediment. Detached oil-globules may be found, though they are difficult to detect, because they do not subside. But degenerated epithelial cells, loaded with oil-globules, and distended often to a considerable size, are more easily found [Figs. 4, 5, *e*]. And there are broken, as well as entire, cylindrical tube-casts consisting of oil-globules, and epithelial cells gorged with oil-globules, and all connected by a transparent and probably fibrinous matrix [*f*]. In this affection of the kidneys, the tube-casts present few degenerated epithelial cells, in which there are not many oil-globules. In the inflammatory affection again, oil-globules are either wanting altogether in the degenerate epithelial cells, or at all events they are very scanty and minute. Oil-globules are always easily distinguished from nuclei, blood-discs, and epithelial cells, by their rounded uniform shape, and their broad black margin, produced optically by their strong refracting power.

12. Such are the pathognomonic characters by which the two very different organic alterations of the kidneys may be distinguished during life. But the two affections may concur. After the inflammatory form has subsisted for some time, and many urine-tubes have become gorged, or empty and functionless, oil-globules may be secreted into the empty ones, together with a fibrinous matrix; so that oleiferous tube-casts may be afterwards seen in the sediment of the urine, as well as those which are simply inflammatory. Dr Johnson first noticed this combination; Dr Gairdner has observed it often; I saw it several times during last spring clinical course; and some have suspected it to be so frequent as to render a diagnosis between the two forms of organic disease for the most part impracticable. I am satisfied, however, that the two affections do occur independently of one another; and in particular I have repeatedly seen sediments presenting invariably the inflammatory character, or at least with only a few oil-globules in the degenerated epithelial cells. The occurrence of a few small oil-globules is no true source of confusion; because, according to the best observers, the healthy epithelial cell is not without a few of them.

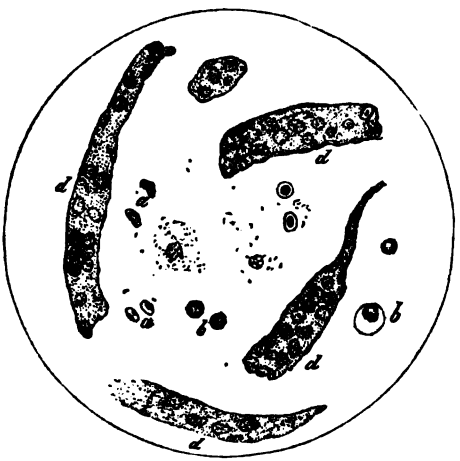
13. To the affection which consists of fatty or oily degeneration, Gluge has given the unexceptionable name of *stearosis*, which expresses a fact but no theory. The nomenclature of the other affection is not so easy, and has not been so lucky. It has been called *desquamative inflammation*, because it consists essentially of inflammatory action, causing the epithelial cells to scale off the mucous membrane of the uriniferous tubes. But there is equally scaling of these cells in the oleaginous degeneration. So that if this process is to enter at all into the nomenclature, the one disease should be called *stearotic*, and the other *inflammatory*, desquamation. I do not think, however, that these terms will be long thought quite accurate, or if accurate, altogether convenient. But I shall use them in the sequel, without pledging myself to their exactitude.

The observations which have come under my notice since becoming acquainted with these microscopic inquiries, bear out on the whole the conclusions stated in the preceding propositions. During especially my period of service as Clinical Professor last spring, there were many excellent subjects of study in these wards, and the opportunity was not neglected. Of seven characteristic cases of Bright's disease, which we were able to observe for a considerable period, only one presented the characters of stearosis, four those of the pure inflammatory form, and two the mixed characters of both affections, in which, however, the inflammatory tube-casts and epithelial cells were greatly more prominent than the stearotic. My experience at that time, in conformity with what I have observed since, leads to the conclusion that the inflammatory is much more common in this city than the stearotic form of renal disorganisation,—Edinburgh differing in this respect from London and Brussels, where stearosis seems proportionally more frequent, but corresponding with Paris, in which that degeneration seems comparatively rare. Our case of stearosis was the only fatal one; and this variety is probably always fatal. The other cases either got quite well or were dismissed much improved; and I am now satisfied that many cases of the inflammatory form may, like these, do well under judicious treatment and subsequent careful regimen. In the case of the man Flint, now under your observation, the disease, which has commenced in the ward, and is plainly quite recent,—because the urine is natural in density and quantity, and he never before had any signs of renal disease, whether primary or secondary,—seems to be of the inflammatory character. But as it will be advisable to examine the deposit of the urine more carefully than the rather imperfect specimens have hitherto enabled us to do, I shall defer the application of these general remarks to the special case until a future day, when I hope there may be other cases to take along with it; and meanwhile I shall exemplify what has been laid before you in general terms by the particulars of three important cases from those of last year; which I am enabled to illustrate by graphic and appropriate microscopic drawings, taken by Dr Saunders, then principal clinical clerk in the wards.

Case 1.—The first is an instance of inflammatory desquamation, commencing, like that of Flint, in the hospital during treatment for another disease, and subsequent to mercurialisation. The subject was a labourer of 22, William M'Millan, who had been cured of jaundice and diarrhœa a twelvemonth before in this hospital and was again attacked, fourteen days before admission, with the same unusual combination of diseases. When admitted on January 11th, 1850, he had no jaundice, but frequent watery diarrhœa, some pain across the lower belly, occasional vomiting, a sallow complexion, and considerable debility. The lungs were healthy, so was the heart, and the pulse was little excited; the liver was not enlarged; and the urine was ascertained at the beginning to be "natural in colour, without deposit, and not coagulable." Dr Bennett, who was then on duty, treated the case with leeches, calomel, and opium, till the mouth became slightly affected, then with catechu and potash, and also one blister over the liver, which was subsequently the seat of pain and some tenderness. When I took charge of the wards on February 1st, he was considerably better; and he continued to improve on the whole under the use of bismuth, opium, and hydrocyanic acid.

On the 20th February, his feet appearing somewhat œdematous, the urine was examined, and found to have become turbid even when just passed, very strongly albuminous, 1025 in density, not particularly scanty, and abounding with degenerated epithelial-cells [Fig. 1, *b*], and urine tube-casts [*d*], which for the most

Fig. 1.



part presented the inflammatory characters, but occasionally a few globules of oil. Under the use of cupping over the loins, Dover's powders nightly, and the warm bath every other evening, he slowly improved, sweating every night, and nevertheless passing daily from 70 to 90 ounces of urine, between 1017 and 1019 in density. Repeated examination of the urine showed continuing coagulability, abundant fragments of inflammatory tube-casts, very seldom containing any oil-globules, but occasionally octaëdres of oxalate of lime. In the beginning of March, it was for the first time observed that the abdomen contained fluid; and from that time a moderate ascites continued to be the predominant dropsical affection. Gallic acid was now tried in the dose of three grains thrice a-day, but with no decisive benefit; nor did the acid ever appear in the urine. The man remained in the hospital till the middle of April, when he became tired of his slow progress, and left it for the country. The urine had been for some time 120 ounces daily, without the use of diuretics; it was 1018 in density, and moderately coagulable; and the sediment showed degenerated epithelial cells and broken-down tube-casts, always of the inflammatory kind. The œdema had been removed, except that the ankles swelled a little at night; but the ascites had not altogether disappeared. I have not heard of him since; but his state at dismissal was obviously very precarious.

It is clear, from the precursory jaundice and supervening ascites, that, although the liver was not enlarged, the affection of the kidneys was combined with an older disease of that organ; and in all probability this circumstance accounts for the little impression made on the renal disorder by treatment, and it certainly added greatly to the man's jeopardy.

CASE 2.—Not so with the next patient, who, as I consider, was radically cured of an uncomplicated attack of the purely inflammatory form of Bright's disease, occurring in connection with the puerperal state. This was a young woman, Elizabeth Birnie, 21 years of age, who had been delivered in the Maternity Hospital, without any accident, on the 25th January 1850, and was transferred to this hospital on the 8th February. For four weeks before her confinement she suffered from frequent scanty micturition, without any other complaint; and five

days after delivery the limbs began to swell. When she became my patient, the face was puffy and very leucophlegmatic; the legs were œdematous as high as the knees, and pitted on pressure; and the abdomen was rather full, but without distinct evidence of fluid in it. There was general tenderness of the abdomen, but none particularly in the loins or across the epigastrium. The heart was sound; so too seemed the liver; and there was only a slight catarrh. She had just ceased to nurse her child, and the milk flowed freely. The urine was scanty, dusky, cloudy when newly passed, acid, 1017 in density, and highly coagulable. She had a trivial fever, the pulse being 96, but small, and the skin dry, and in the night-time hot. She was ordered bitartrate of potash, spirit of nitrous ether, and Dover's powder. In three days the œdema had disappeared without increase of urine. The sediment of the urine consisted of blood-discs (Figs. 2 and 3, *a*),

Fig. 2.

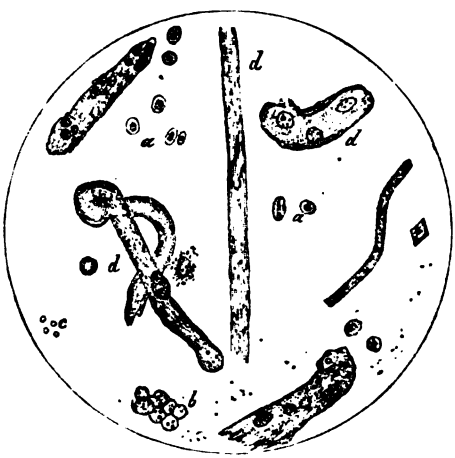
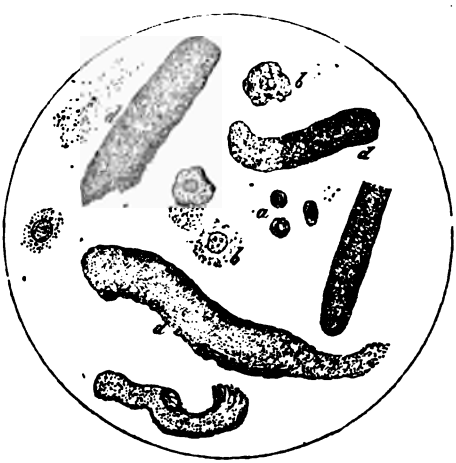


Fig. 3.



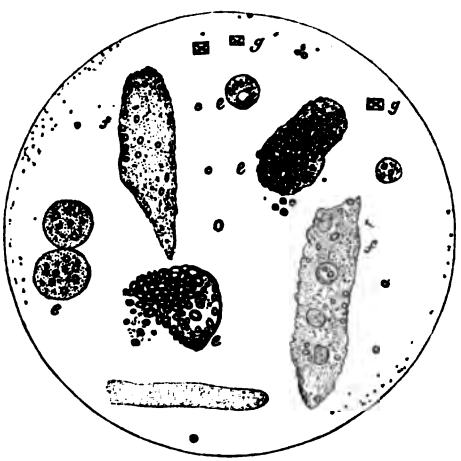
degenerated epithelial-cells (*b*), epithelial-nuclei (*c*), and casts of the uriniferous tubes (*d*), filled with the preceding substances, and seldom any oil-granules. The sediment was frequently examined afterwards, and presented little variety, except in amount, and in presenting a few tube-casts consisting apparently of the fibrinous matrix alone (Fig. 3). The diuretics were then discontinued, and Dover's powder was administered twice a-day. Notwithstanding the opium, a tendency to diarrhœa occurred for four days after the 14th, but it was stopped by increasing the opium. The features having again become puffy, nitrous ether and oil of juniper were given; but, in consequence of the urine becoming more red and more coagulable, all diuretics were permanently abandoned. On the 20th, she began to complain of pain and tension of both eyeballs, and in the course of a few days the sclerotic of both eyes became very œdematous and swelled over the cornea; but this new affection was quickly removed by a zinc wash. On the 24th, vomiting before breakfast occurred for a few days, and diarrhœa threatened to return. Both affections, however, were arrested by morphia. Meanwhile a gentle perspiration had been maintained pretty steadily every night by Dover's powder. On the 27th, the improvement was very marked in every respect. The pulse had become natural; there was no œdema; the urine, which had never exceeded thirty ounces, had a density of 1017, much less turbidity and brownness, and much less coagulability; and her sense of general comfort was greatly increased. Three grains of gallic acid were then ordered thrice a-day, but without seeming to pass by the urine, until the doses were doubled, when it was easily detected by the salts of iron causing a bluish-black precipitate. On the 6th March, the urine was forty ounces in quantity, 1015 in density, less turbid, and no longer red or brown. From this time there were occasional fluctuations, but on the whole an appreciable amendment, until the 31st March, when the urine presented very few tube-casts, little turbidity, and little albumen; but for three days the potash test indicated the presence of some sugar, although the density was only 1012. On April 16th, there was only a mere trace of albumen, and on the 30th none. On the 16th May, she was dismissed. The albumen had never returned; the urine had been constantly clear, abundant, from 1019 to 1022 in density, without sugar and tube-casts. She had become florid, sufficiently plump, vigorous, and active, and she had the appearance of perfect health. I have not heard of her since.

Every circumstance here unites to prove this a case of subacute inflammatory desquamation of the uriniferous tubes. It may fairly be held an instance of complete cure, accomplished mainly under the influence of diaphoretics. Diuretics certainly seemed to be injurious, on at least one occasion; but this is the only instance in which these remedies have ever appeared to me to aggravate the symptoms. I have met with various cases of the same kind in the same circumstances, most of them equally favourable in the result. Another will be detailed under the head of the treatment.

CASE 3.—To these I shall only add for the present one case of the other form of Bright's disease. Frequent examination of the urine constantly presented characteristic microscopic fields of oleiferous epithelial cells, and oleiferous tube-casts. If the doctrine sketched above were good for anything, there could be no doubt of the intimate condition of the kidneys. And accordingly they displayed, after death, appearances which Professor Gluge and Dr Johnson themselves could not desire to see surpassed. The patient was a labourer, James Wood, aged 35, admitted into the surgical department of this hospital for necrosis of the left thigh-bone, but transferred to the clinical ward in a few days with coagulable urine and ascites. In November 1849, while suffering for some weeks from pain along the left thigh and lameness, he was seized with rigors, in consequence of sitting in a chilly apartment without a fire. Swelling of the abdomen and limbs followed. Little farther information could be obtained of the progress of his illness. When admitted into the infirmary, there was a sinus at the lower part of the left thigh, communicating with the necrosed femur. When received into the clinical ward, he had moderate œdema of the limbs, slight ascites, trivial catarrh, a sallow countenance, considerable emaciation, little appetite, much thirst, a frequent feeble

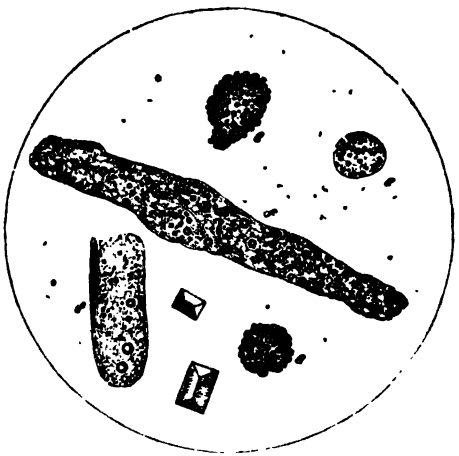
pulse, and great debility. The action of the heart was barely audible, but otherwise natural. The urine was moderate in quantity, 1012 in density, without sediment, and distinctly coagulable. Dr Bennett, who then had charge of him, relieved the catarrhal symptoms with squill and opium, and the dropsical effusions, first by Dover's powder, and afterwards by bitartrate of potash. But nevertheless he got constantly weaker and weaker, and became subject to vomiting in the morning, which bismuth, however, seemed to relieve. Soon after taking charge of the wards, I found him, on February 7th 1851, extremely weak, with a thready pulse, and coldness of the feet and hands. There was again anasarca of the legs, and now also of the scrotum, a little ascites, no great bronchitis, but a labouring respiration, and vomiting before breakfast. The urine was fifty ounces in quantity daily, 1015 in density, cloudy when just passed, slightly albuminous, and abounding in lithates and phosphates. It had been necessary to order him both wine and spirit, on account of his extreme weakness. In a few days the urine was more carefully examined, when it was neutral, and free both of phosphates and lithates, but otherwise in the same state as before. The sediment presented many epithelial cells enlarged and gorged with oil-globules (Figs. 4 and 5, *e*); numerous

Fig. 4.



casts of the uriniferous tubes, entire or broken, and full of oleiferous epithelial cells and independent oil-globules (*f*); and also occasionally a few octaëdral crystals of oxalate of lime (*g*). On February 18th, as there was more œdema and dyspnœa, squill and digitalis were substituted for bitartrate of potash, and with the effect of increasing the urine. A mixture of chloroform and spirit of ammonia greatly relieved his breathing. Creasote often checked the liability to vomiting. But, although the urine was soon raised to 120 ounces daily, the dropsical effusions did not for some time recede. At length they began to give way. On March 7th he was every way better; and on the 20th, the urine having been for three weeks at 120 ounces daily, there was very little œdema of the limbs, no ascites, no catarrh, no vomiting, and a tolerable appetite; but still he remained very weak, and seemed to be picking up little strength, and no flesh. Afterwards a watery diarrhœa returned, and became gradually more and more attended with vomiting. On April 7th, both of these affections were urgent, in spite of acetate of lead with opium, catechu, morphia suppositories, and creasote. Consequently the exhaustion soon became extreme, and ended fatally on the 15th.

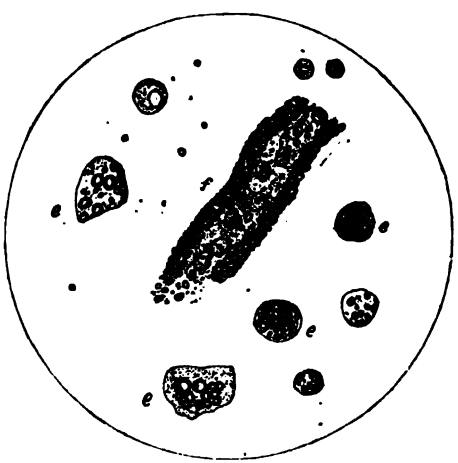
Fig. 5.



For four or five days before death the urine did not exceed ten ounces daily, yet to the last there was no coma.

On dissection, the kidneys were found somewhat enlarged, their surface mottled with little vascular patches, their interior mottled irregularly with deep grayish-purple spots on a light fawn ground, the medullary part highly injected. Opaque yellowish granular spots were visible on many parts of a longitudinal section. The

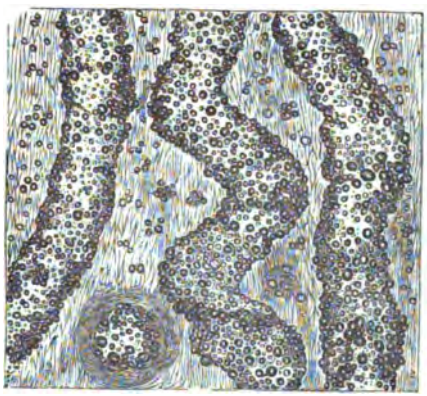
Fig. 6.



fluid scraped from the surface of a section presented before the microscope oleiferous epithelial cells (Fig. 6, *e*), and oleiferous tube-casts (*f*), exactly like the sediment of the urine examined during life. Thin sections of the medullary part of the kidneys

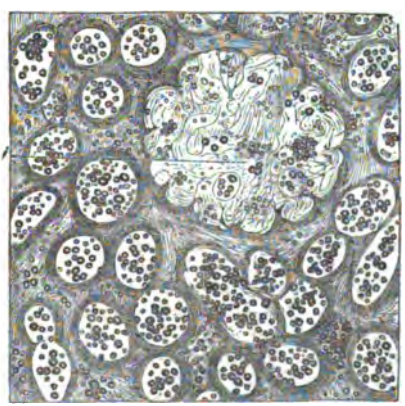
made with Valentin's knife, both in the direction of the uriniferous tubes (Fig. 7), and across them (Fig. 8), showed these tubes everywhere gorged with oil-globules,

Fig. 7.



while the malpighian bodies seemed free of such obstruction (Fig. 8, *i*). The supra-renal glands were enlarged The mesenteric glands had a bluish marbled

Fig. 8.



appearance. The intestinal mucous membrane was everywhere thickened, and its folds were in many parts dark brown, but there was no ulceration. The spleen was enlarged and firm. The heart was rather small and pale, the endocardium opaque, the aorta atheromatous at its commencement. The lungs were somewhat emphysematous, and presented here and there, especially just under the pleura, nodules of condensed pulmonary tissue, about the size of a cherry stone, grayish-purple in colour, generally softened in the centre, and passing insensibly into the surrounding healthy texture. The left femur presented interesting appearances; but as they were those of necrosis generally, they need not be detailed here.

The appearances in this case were characteristically those of stearosis of the kidneys. I endeavoured to ascertain by chemical analysis, whether oil could be

obtained from the kidneys in proportion to the microscopic indications. An accident prevented the weight of oil from being determined, but it was plainly much greater in proportion than in a comparative analysis of a healthy kidney. From the first the symptoms presented a character of *adynamia*, which could not be referred to the necrosis merely, but seemed to betoken the presence of a malignant disease. The abundance of the watery part of the urine, and the facility with which it was very much increased, together with the want of oily degeneration of the malpighian bodies, rather favour the notion that these bodies are concerned less with the proper secretion of the kidneys than with the secretion of watery fluid.

VI. TREATMENT.—The treatment of Bright's disease of the kidneys is partly directed to the removal of the fundamental disease, and in part to the cure of secondary diseases.

The main remedies for the fundamental disease are general depletion, topical depletion, local counter-irritation, diaphoretics, diuretics, and astringents. In acute cases general bloodletting is an essential measure, if there be decided general re-action; and in the young, robust, and plethoric it must be practised freely, as in ordinary acute inflammations. Local depletion is resorted to in subacute cases, or when the constitution is impaired, but, according to my observation, not with the advantage we might expect, unless its amount produces a general impression. Counter-irritation of the loins is probably too little resorted to. It is improbable that any benefit can be derived from depletion or counter-irritation in stearosis, uncomplicated by inflammatory desquamation. In all cases, acute or chronic, stearotic or inflammatory, diaphoretics are of essential service. The best are Dover's powder, the warm bath, warm clothing, and, for convalescents especially, a moderately warm climate. Diaphoretics have always appeared to me most serviceable when they are so given as to excite a gentle perspiration during a part of the night. Diuretics are seldom necessary for the fundamental disease; but they are sometimes advisable when the urine becomes very scanty, even although there is no dropsy; and they are undoubtedly the most certain and speedy remedy when the dropsical effusions are considerable. I am convinced that the fears entertained by some of injury being produced by the stimulus of diuretics on the kidneys are visionary. Case 2d is the only instance in which I have ever witnessed any apparent harm; and in numberless instances, as in case 3d, I have seen dropsy quickly removed by diuretics, not only without increase of the other symptoms and secondary affections, but likewise with manifest improvement in them all. The stimulus caused by diuretics may well be supposed to differ entirely from the peculiar irritation which gives rise to either inflammatory or stearotic desquamation; and in that case the induction of the one may not of necessity tend to induce, keep up, or increase the other. Besides, is there no likelihood of advantage from diuretics sweeping out the tube-casts, and other matters obstructing the uriniferous tubes? The best diuretics, in my experience, are digitalis, squill, and bitartrate of potash taken simultaneously. Nitrous ether, broom tops, acetate of potash, oil of juniper, have seldom succeeded in my hands when the three former failed; but after all these had failed, I have seen diuresis speedily established by Hollands. I doubt whether astringents, even in chronic cases, are of the service which we might expect. I have seen a few cases, in which the albuminous impregnation ceased under the use of acetate of lead, or under that of gallic acid; but the good effects have not been so frequent as to entitle me to speak positively in their favour.

The treatment of secondary diseases does not materially differ from that which is applied to the same diseases in other circumstances. All of them are more difficult of cure when secondary to Bright's disease. Inflammations must be treated with activity in the usual way. I repeat that, after frequent careful watching, I can find no objection to the use of diuretics for removing dropsy, but quite the reverse; but in this affection we should avoid the cathartic plan, as being apt to excite a permanent diarrhœa; and mercury should be shunned as a mercurial on account of the facility with which in this disease it produces exces-

sive action. The best remedy for the diarrhœa, which so often proves troublesome, and sometimes dangerous in Bright's disease, is the Edinburgh lead and opium pill, in the dose of five or ten grains twice or even thrice a-day; to which may be added, in severe cases, a fatty suppository of muriate of morphia. Vomiting may be sometimes removed by bismuth, but more frequently by morphia, hydrocyanic acid, creasote, pyroxylic spirit (medicinal naphtha), chloroform, or little fragments of ice; and when these fail, a blister over the epigastrium has sometimes succeeded. It is not necessary for me to say anything of the treatment of other secondary diseases. In all cases stimulant liquors should be used with caution; but they are sometimes unavoidable when the exhaustion is great. The diet, in the acute form, must be low, light, and farinaceous; but in the chronic form a moderate allowance of nutritive easily digested food, with a moderate use of liquids, has always appeared to me the preferable diet, when the patient can take it.

Bright's disease was long considered a very deadly one; indeed as generally fatal, sooner or later. This denunciation is probably true of the stearotic form. But it is quite incorrect, if extended to the more frequent form of inflammatory desquamation. This will appear from the following selection of cases, which I have singled out from a much more extensive list, because I have had the rare advantage, in their instance, of observing the results, for a very considerable term of years, after the first apparent recovery.

Case 2, already narrated, may be here in the first place referred to as an excellent exemplification of the ordinary treatment and usual phenomena and progress in the most favourable of cases. But, as the patient was sent to Ireland under the Poor-law arrangements soon after recovery, I have lost sight of her.

Case 4 was similar to this, but more obstinate, although eventually and permanently cured. The patient was a lady of 27, who in the seventh month of her first pregnancy, was attacked early in October 1846, with considerable œdema of the limbs, hands, and face. At the full time she was delivered of a still-born child, without any accident. The œdema then disappeared, and seemingly also the albuminuria, which had been constant before delivery. But her health continued indifferent; she regained neither flesh, strength, nor looks; the albumen re-appeared from time to time in the urine; the features often became again puffy; and at last, in the end of January, a tendency to diarrhœa was superadded. I saw her first on the 10th of February 1847, along with my colleague, Dr Simpson. At this time, six weeks after delivery, she was puffy in the features, very leucophlegmatic, not œdematous, weak, exhausted, emaciated, and excitable. The pulse was 80, and irritable; the tongue clean and bleached; the bowels no longer loose; the appetite and digestion tolerable. The urine did not exceed 30 ounces daily, it was of natural colour, 1016 in density, turbid when just passed, and moderately coagulable. She was ordered morphia and ipecacuan instead of Dover's powder, the warm-bath, croton friction over the loins, and a rather generous diet, including a little wine. These measures were followed by gentle perspiration every night; and in eight days she looked and felt stronger, had a firmer pulse and excellent appetite, and suffered from no uneasiness of any kind. The urine amounted to 36 ounces daily, had a density of 1014, was less coagulable, but continued turbid when passed. The acetate of ammonia was now substituted for the morphia and ipecacuan. From this time she improved progressively; nor did I again see her as a patient. The urine soon lost its turbidity and albuminous impregration. She spent the subsequent winter on the continent, along with her family, as a precautionary measure; and since returning, has passed two winters in Edinburgh without illness, having born a living child during that period without any accident, and continuing still in excellent health.

There was no microscopic examination of the sediment in this case. But every other particular combines to prove it a case of chronic inflammatory desquamation, the consequence of a peculiar renal irritation, which, in this city at least, has been often recognised as a concomitant of advanced pregnancy and the

puerperal state. It may fairly be assumed, after four years of perfect health, and a second pregnancy without accident, that the cure has been radical. The cause of the connection of this disease with pregnancy is obscure. Pressure of the gravid uterus on the kidneys has been thought by some an adequate explanation; but this I doubt. The occurrence of renal disease, in such circumstances, has been observed only in persons pregnant for the first time. The present is the only instance I have seen in which the patient did not present the physiognomy of the strumous diathesis. It is seldom that the operation of any adequate occasional cause can be discovered, though we cannot doubt that exposure to cold, injuries of the loins, &c., would act on the predisposed with some energy.

Case 5.—The next case is one of acute dropsy, with general febrile symptoms, unconnected with pregnancy, and to all appearance radically cured. The patient was a tall, robust female-servant, of 38, somewhat intemperate for five years, but habitually healthy. On the morning of 9th December 1846, her face swelled; in the evening, having worked all day in a draught, the legs began to swell; in three days the swelling was general; and she then felt so unwell as to be obliged to take to bed. During the next two days she had frequent chilliness, constant headach, one attack of epistaxis, and twice a severe hysteric fit. On her admission into the Infirmary, on the 15th December, there was leucophlegmatia; elastic œdema of the face, trunk, arms, and limbs; uneasiness and tenderness in the loins and pit of the stomach; headach and drowsiness; a frequent, sharp pulse, white tongue, and hot skin; and scantiness of urine, which was of natural colour, not turbid, 1025 in density, and very strongly coagulable. The liver was not enlarged; the heart was free of any sign of disease; and there was only a trivial catarrhal affection. She was first freely bled from the arm, and then from the loins by cupping. The blood drawn from the arm was much cupped and buffy. Diuretics then acted briskly, the œdema at the same time quickly subsiding, and along with it every other symptom, including the proportion of albumen in the urine. Dover's powder was substituted for the diuretics as soon as the œdema disappeared. The features gradually became less leucophlegmatic, and at the sametime the albumen ceased to show itself in the urine. At the close of January she was so well and strong that she became night-nurse in the ward, and soon afterwards laundry-maid. On May 7th, when in this capacity, she was a powerful, muscular, healthy-looking woman, and the urine was copious, quite free of albumen, and 1015 in density. Next summer I encountered her in the shape of a domestic servant at the Royal Lunatic Asylum, still enjoying perfect health. Dr Skae ascertained for me that in February 1850, having left the service of the Asylum, she was "alive, well, lately married, and without any complaint, except housemaid's knee, and the occasional consequences of drinking;" and a few days ago, on 7th March 1851, four years complete after her convalescence, he also ascertained that she continued in robust health.

If diuretics are apt to prove injurious in Bright's disease, as many now imagine, this was surely the very sort of case to exhibit the effect. Nevertheless, their influence was in every way salutary; for with the removal of the œdema, every other symptom also subsided, and nothing occurred to interrupt recovery.

Case 6.—A medical friend, of the age of 38, actively engaged in country practice, sustained a compound fracture of the tibia in 1842, and an attack of acute rheumatism next year. In July 1845 he was seized with hæmaturia, and even when there was no blood present, he always found the urine coagulable. At the same time his appetite failed, he steadily lost flesh and strength, and had a constant nameless feeling of ill health; but he had no pain or frequency of micturition, and no morning sickness. When he first consulted me, two months after this attack, he had a haggard, dingy hue of countenance, a feeble appetite, but sound digestion, an excitable pulse, a white tongue, and a scanty, rather turbid, brownish-red urine, 1021 in density, and moderately coagulable. The sediment showed degenerated epithelium cells; but at that time I was not sufficiently acquainted with Gluge's researches to rely now on the other microscopic observations I made. There had never been any œdema; the heart's action was natu-

ral in all respects, except that it was rather frequent, and easily excited; the liver was not enlarged. He was of a consumptive family. I recommended repeated blisters over the loins, the warm bath, a rather generous diet without stimulant liquors, and a pill of lead and opium, or gallic acid. On the 1st of October he reported to me that he had derived no benefit from the blisters, or opium and lead; but that, on substituting gallic acid for the astringent, the redness of the urine had gradually disappeared, and its coagulability had diminished. On October 17th I saw him again, still not strong, but without his former haggard look, and decidedly regaining flesh. He had some pain across the loins, but no other uneasiness connected with the urinary organs, and there had been no return of hæmaturia; but the urine was still somewhat coagulable. Circumstances having rendered it convenient for him to winter at the Cape of Good Hope, I advised him to resort thither; and, at his own suggestion, the gallic acid was continued. He returned to this country in October 1846, and wrote to me thus:—"As soon as I got into warm weather, and out of the turmoil of professional business, I began to improve in health and strength, gathered flesh, got colour, and, ere arriving at the Cape, was stouter than I had been for years. After using the gallic acid for a few days at sea, I had no appearance of albuminuria, nor has there been any return of it since. I travelled about 1500 miles on horseback through the Cape colony, and had several long journeys, one of them to the extent of eighty-eight miles in one day, without suffering any ill consequences." He also had a little share in the first Caffre war. On returning to this country he resumed his practice in an extensive range of country; and he continues to the present time, March 1851 (six years after his illness), an active, vigorous, healthy, country practitioner.

Those who are inclined to maintain that there is no such thing as Bright's disease without dropsy, will of course deny that this case has been rightly placed here, and may insist that it was simple chronic nephritis. In reply, I can merely say, that I have repeatedly seen cases precisely similar in all respects, except that there was also some anasarca; and others precisely similar, and without anasarca, but fatal,—in which, after death, the kidneys presented the characteristic appearances of advanced Bright's disease. This case, and one or two others communicated to me by the subject of it from his practice, led me to entertain, in regard to gallic acid, hopes which have not since been realised, either in his observation or in mine, so often as we could have wished.

Case 7.—Another medical friend, about 38, physician in a large county town, but formerly in the Army Medical service, after suffering from dyspepsia for ten years subsequently to an attack of ague in the East Indies, became liable to morning sickness and vomiting, early in the summer of 1846; and in the end of August he was seized, after protracted professional fatigue and anxiety, with an indescribable uneasiness in the loins, like painful pressure, and with somewhat frequent micturition. Fourteen days later he came to Edinburgh, to consult the late Dr Davidson and myself. He had a dingy, unhealthy complexion, the fetid breath I had often remarked in advanced Bright's disease, no great loss of strength or flesh, a frequent pulse and doughy tongue, and palish, clear, rather abundant urine, 1010 to 1015 in density, and highly coagulable. He was much harassed with sickness and vomiting before breakfast, as well as after meals; but he never had observed any anasarca. Dr Davidson and I agreed that he had Bright's disease, and entertained serious apprehensions of the result. Blood was withdrawn by cupping from the loins, and a succession of blisters was also resorted to; but without success. Bitter tonics with alkalies were of no use; but bismuth and bicarbonate of potash decidedly relieved the stomach complaints. The treatment in other respects was confined to relaxation from business, a low general diet, and the warm bath. In three weeks I visited him at his residence, and found him with an improved complexion, but not so strong, and considerably reduced. He had still on awaking sickness, clammy throat, and often vomiting; acidity, sickness, and vomiting after breakfast; but no uneasiness after dinner, since making the experiment of dining almost entirely on meat

and bread. The pulse was natural, the tongue cleaner, and the skin soft and perspirable. The urine amounted to forty ounces daily, and was high-coloured, clear, 1018 in density, less coagulable, and passed neither with unusual frequency nor any longer during the night. I recommended a meat breakfast, weak brandy and water for drink, a warm plaster over the loins, tincture of cardamom or hydrocyanic acid on awaking, and country air and carriage exercise. The cardamom relieved the morning sickness. Gallic acid was tried as an astringent, but disordered the stomach. Afterwards he derived decided relief from vegetable tonics, with bicarbonate of potash, opiates at night, and occasionally the powder of mercury and chalk, with ipecacuan as an alterative, on account of the disordered state of the alvine excretions. Under this treatment the urine became of higher density, till at length it was often 1026, 1028, or even 1030; and the albumen also decreased, though very slowly, so that it did not entirely disappear for eighteen or twenty months. By that time he was quite well. He has often examined the urine since, and has never observed the albumen to re-appear. He is now an active, healthy, rather muscular man, constantly engaged in the duties of his profession; and he has never been threatened with a return of his former complaints.

This case, too, may be thought by some to have been simple chronic nephritis, and perhaps with more reason than in the former instance, as the urine did not present a turbid appearance when just passed, and thus no satisfactory evidence existed of epithelial degeneration in the uriniferous tubes. Still I think it extremely difficult, in the present state of our knowledge, to draw a distinction between such cases and the more undoubted Bright's disease; and I must protest against the arguments used by some, that such cases are not Bright's disease, because there is no dropsy, and because they get well.

CASE 8.—In 1838 I was consulted in the case of a retired merchant in the country, who had been recently attacked with universal and extensive anasarca. He was a man of 63, totally blind, nearly deaf, very intemperate, cross-tempered, and consequently a great burden to his family. His surgeon in the country, who consulted me by letter, could not find any thing wrong with the heart, liver or lungs, but had not adverted to the kidneys as a possible seat of the pathological cause of the dropsy. Having requested the urine to be sent for examination, I found it of moderate density, rather pale, somewhat turbid, and pretty strongly coagulable. I directed digitalis, squill, and bitartrate of potash to be given, to excite diuresis. These remedies acted with speed and force; the anasarca quickly disappeared; his general health was soon quite restored; the urine acquired its healthy characters; and ere long no trace of his disease remained. For seven years he enjoyed excellent health, but confined himself, on account of his helplessness, almost entirely to bed. In the end of 1845, having in the mean time changed his residence to Edinburgh, he again became my patient on account of *gangræna senilis* of the right foot; and he died of this disease in about two months, without any return of anasarca, or of urinary symptoms of any sort. An examination of the body could not be obtained.

CASE 9.—In my treatise on "Granular Degeneration of the Kidneys," p. 281, I have related a very interesting case of Bright's disease, which I have been fortunately able to trace at intervals ever since. This was an intemperate harbour-porter at Newhaven in this neighbourhood, who, while slowly recovering in 1836 from a dangerous attack of pleuropneumonia, for which he took calomel and opium to salivation, was seized with symptoms of the acute form of Bright's disease, including great œdema, scantiness and very strong coagulability of the urine, and considerable drowsy stupor. Blood-letting and diuretics gradually removed every urgent symptom; and in a few months he recovered his strength so thoroughly by residing in the country, that he was able to resume his occupation at Newhaven. Here he lived a temperate life and enjoyed excellent health for two years; but the urine continued pretty steadily so low as 1013 in density, and strongly coagulable. So far the account in my treatise. On inquiring for him again in 1840, I was informed by his brother porters

that he had left Newhaven for the interior, where he had resumed his former habits of extreme intemperance, and had died of delirium tremens. But this report proved to be a great falsehood and libel. For in 1847 I encountered him in the Infirmary waiting-room, again afflicted with dropsy, and in quest of relief where he had formerly been cured. He then told me, that in 1838 he had left his previous occupation for that of a railway-labourer, at which he had worked without intermission, and in excellent health, except when twice laid up by severe accidents; that he had adhered steadily to his vow of sobriety, which he took before his own conscience on recovering in 1836; and that he had never suffered from œdema, or any of his old complaints, till five days before returning to his old hospital quarters. He was now a man of 53, extremely tall and erect, having been a Lifeguards-man when young. There was great elastic œdema of the limbs, and less of the arms and trunk, much leucophlegmatia, erythema on both calves from distension, with superficial ulceration, and no frequency of the pulse or other sign of re-action. The urine was natural in quantity and colour, passed frequently at night, 1018 in density, and strongly coagulable. Acetate of lead and opium, and frequent warm-baths, were ordered. Under this treatment the œdema had substantially disappeared in the course of four weeks, and the urine had become slightly coagulable; and in three weeks more he left the hospital in good health, but with the urine still somewhat albuminous. I again lost sight of him for three years, when he appeared last summer to consult me respecting his daughter, but in excellent health himself. He had been for some time gate-keeper on the Caledonian Railway, about forty miles from town, had an extremely respectable appearance, and the expression of a sober, well-conditioned man, nor had he ever suffered from any illness after leaving the hospital. He gave me an opportunity of examining the urine, which was natural in colour, wholly free of albumen, and without any urine-tube-casts, or anything but a few natural epithelial cells and minute octaëdral crystals of oxalate of lime.

It seems impossible to doubt that this is a case of radical cure, twice effected in the same individual, with an interval of eleven years between the attacks. From the state of the urine three years after the second illness, it is allowable to conclude, that the albuminuria had disappeared after the former also, though it appeared not to have done so in the course of the first two years. This was one of the first cases which led me to suspect the occasional excitement of Bright's disease by mercurial action. Another, which proved fatal, happened in the hospital wards under my care about the same time.

The cases now detailed furnish, it appears to me, satisfactory evidence, that Bright's disease is capable of radical cure, even in very urgent circumstances; and it is probable that the proportion of recoveries would be much larger if the patient, in chronic cases, became sooner aware of the nature of his illness, and if his circumstances and condition of life admitted more frequently of a careful regimen after the disease has been once fairly subdued. In all instances, it is evident that recovery is very gradual, and that great patience is necessary on the part both of the patient and his physician.

LECTURE ON THE RECENT PROGRESS OF ORGANIC CHEMISTRY.

By William Gregory, M.D., F.R.C.P., Professor of Chemistry in the University of Edinburgh.

(Delivered in the Hall of the Edinburgh College of Physicians, on 14th April 1851.)

The period in which we live, viewed in reference to chemistry, is marked by the enormous development and rapid progress of organic chemistry. This period dates, properly speaking, from the time when the ultimate analysis of vege-

table and animal compounds came to be practically understood, and rendered sufficiently easy of performance to allow those who devoted themselves to the investigation to make, within a reasonable time, the numerous analyses which research in any part of organic chemistry imperatively requires. Gay-Lussac, and Thenard, Berzelius, Thomson, and Prout, led the way; but at first the processes followed, although yielding accurate results, were so difficult and tedious that but few chemists ventured on them; and it was not till Liebig had invented his potash apparatus for weighing the carbonic acid, produced in the combustion or analysis of organic substances, instead of measuring it as formerly, and had devised the process which is still followed, with some few modifications, that the analysis of organic bodies came to be the daily occupation of the chemist, and that, in spite of many difficulties and many blunders, the progress of organic chemistry became both rapid and steady.

There is another circumstance, of equal or even greater importance to organic chemistry, which came into full operation about the same time,—I mean the atomic theory. Every one knows that vegetable and animal tissues and products consist of two, three, four, or five combustible elements, of which their chief mass is composed, along with a small per centage of mineral or inorganic matters, which constitute their ashes. These are not less essential to the existence of organised structures than the combustible elements, but they may be considered separately. They are such bodies as potash, soda, lime, magnesia, iron, phosphoric acid, sulphuric acid, chlorine, iodine, and fluorine.

The combustible elements of organic compounds, are carbon, hydrogen, nitrogen, oxygen, and sulphur. The last is found in comparatively small proportion in the albuminous compounds, in the bile, and in certain essential oils. The four former occur in all the albuminous bodies, in all animal tissues, in vegetable juices, in the alkaloids, in various colouring matters, and in many products of decomposition, such as urea, uric acid, kreatine, taurine, leucine, tyrosine, &c. And the three first constitute the very abundant substances, starch, sugar, gum, woody fibre or cellulose, fats and fixed oils, some volatile oils, the resins, vegetable acids, many neutral, indifferent, and bitter compounds, and a host of products of decomposition. Many volatile oils contain only carbon and hydrogen.

Now, since we find organic compounds to consist of the same few elements, it is obvious that there must be a prodigious variety in the proportions. And we may safely say, that if the atomic theory had not appeared, if we had not thereby been enabled to express the composition of these substances by formulæ, and if, therefore, we had been compelled to record our results of analysis in the proportions found in 100 parts, no powers of memory or of classification could have enabled us to see our way. The science of organic chemistry could not have existed, and we should only have had a barren list of figures, in which the most certain and important relations of composition could not have been traced.

But with the help of formulæ, the memory is enabled to retain with ease, the composition of these bodies. It is easy, for example, to remember (see example 1), that the formula of starch is $C_{12} H_{10} O_{10}$; and that sugar, gum, and woody fibre, all agree with sugar in containing 12 eq. of carbon. We know that starch and woody fibre are converted into grape sugar by boiling with acids, and starch also, by contact with infusion of malt,—that is, with solution of albuminous or fibrinous matter. Now, if we compare the formulæ of these bodies, woody fibre, $C_{12} H_8 O_8$; starch, $C_{12} H_{10} O_{10}$; and crystallised grape sugar, $C_{12} H_{14} O_{14}$, we see at once that the change into grape sugar is effected by the taking up of the elements of water, 6 eqs. in one case and 4 eqs. in the other. Again, when crystallised grape sugar undergoes the alcoholic fermentation, 1 eq. of it yields, without the addition or subtraction of anything, 2 eqs. of alcohol, 4 of carbonic acid, and 2 of water. (See example 2.) Let any one try to follow these relations without formulæ, by comparing the per centage composition of the bodies above-named, and he will soon acknowledge that, without the atomic theory, and without formulæ, organic chemistry as a science, could not possibly exist, and that no real progress could be made.

EXAMPLE 1. *Formulæ of Vegetable Products.*

Starch,	$= C_{12} H_{10} O_{10}$	Gum,	$= C_{12} H_{11} O_{11}$
Cane sugar,	$= C_{12} H_{11} O_{11}$	Milk sugar,	$= C_{12} H_{12} C_{12}$
Grape sugar,	$= C_{12} H_{14} O_{14}$	Woody fibre,	$= C_{12} H_8 O_8$

EXAMPLE 2. *Vinous Fermentation.*

Grape Sugar.		Alcohol.		Carbonic Acid.		Water.
$C_{12} H_{14} O_{14}$	$=$	$2 (C_4 H_6 O_2)$	$+$	$4 (CO_2)$	$+$	$2 HO$

One of the first results of the application of formulæ, which are mere abbreviations, representing the relative number of *equivalents* to organic chemistry, was the doctrine of compound radicals. This doctrine has been often and fiercely attacked; but although it may have been sometimes carried too far, and regarded as ascertained truth, when in fact it was only an idea, convenient as a means of classifying our knowledge, yet the tendency of recent researches, such as those of Kolbe, Frankland, and Hoffmann, is to show, that it has probably a true foundation, and plays an important part in nature. Let us attend to it for a little.

A compound radical is a body which, although containing two or more elements, plays the part of a simple radical, combining, as an ultimate indivisible group, atom to atom with simple bodies. That such compound radicals exist, is certain, and cyanogen is the type of them. It is composed of C_2, N, but in all its relations with hydrogen, oxygen, chlorine, sulphur, &c., and metals, it is closely analogous to chlorine and its congeners. Thus, with hydrogen and oxygen it forms acids, with metals it forms salts, the cyanides, which agree closely even in physical properties with the chlorides. This analogy is exhibited in Example 3, where cyanogen, like chlorine, is represented by a single symbol. Indeed, chlorine itself may possibly be found some day to be a compound. The analogy would then be perfect.

EXAMPLE 3. *Radicals. Cyanogen.*

Cyanogen $= C_2 N = Cy.$	Chlorine $=$ Cl.
H Cy = Hydrocyanic acid.	H Cl = Hydrochloric acid.
Cy O = Cyanic acid.	Cl O = Hypochlorous acid.
K Cy = Cyanide of Potassium.	K Cl = Chloride of Potassium.
Ag Cy = Cyanide of Silver.	Ag Cl = Chloride of Silver.

Now, if one compound can play the part of an element, others may do so, and just as we have cyanogen playing the part of a salt radical, so we have kakodyle, a compound of carbon, hydrogen, and arsenic, $C_4 H_6$ As, playing the part of a metal. The supposed radical ethyle, $C_4 H_5$, and the others of that series, approach closely to metals in their chemical relations. At one time it was argued, that as no such compounds were known in the separate form, as cyanogen and kakodyle are, they were merely hypothetical. But compounds of the composition of the hypothetical radicals, ethyle, methyle, &c., are now known. I do not positively assert that these compounds are the radicals, for they may be only isomeric with them, but, at least, the existence of the radicals in the separate form is not impossible. We shall see presently that the admission of them enables us to classify with ease a multitude of facts. Example 4 shows the analogy of a metal with kakodyle and ethyle.

EXAMPLE 4. *Radicals. Kakodyle and Ethyle.*

M	= Any metal.	Kd	= Kakodyle $= C_4 H_6$ As.
MO	= Oxide.	Kd O	= Oxide of Kakodyle.
MO_3	= Acid.	Kd O_3	= Kakodylic Acid.
M Cl	= Chloride.	Kd Cl	= Chloride of Kakodyle.
MS	= Sulphuret.	Kd S	= Sulphuret of Kakodyle.
M Cy	= Cyanide.	Kd Cy	= Cyanide of Kakodyle.
Ae	= Ethyle $= C_4 H_5$	Ae Cl	= Chloride of Ethyle.
Ae O	= Oxide of Ethyle.	Ae S	= Sulphuret of Ethyle.
Ae O_3	= Unknown.	Ae Cy	= Cyanide of Ethyle.

It is easy to see that when radicals, whether of the cyanogen or of the ethyle type, or of any other type, combine with elements, as if they also were elements, we must find, in these new compounds, one element replaced by another, just as in the inorganic kingdom. As in potash, the oxide of potassium, the oxygen may be replaced by chlorine, iodine, bromine, fluorine, sulphur, and cyanogen, so in ether, viewed as oxide of ethyle, we have the oxygen replaced by the very same elements. This kind of substitution is the necessary consequence of the existence of a compound radical, analogous to a metal. It is a corollary of the atomic theory. This is illustrated also by Example 4.

But there is another kind of substitution, first dwelt on by Laurent, and followed up by Gerhardt and Hoffmann. This has gradually led to the doctrine of types, which seems to have been quite unnecessarily set in opposition to that of compound radicals. Both may be true in different cases; they may even be combined; and both are calculated to throw light on organic chemistry.

The peculiar form of substitution on which hinges the theory of types, is this, that in a complex group, containing a compound radical, it is not the element combined with the radical which is replaced by another, but one or more atoms of the group or radical itself. For example, let us consider ammonia, NH_3, as such a group. We may, if we regard ammonia as $NH_2 + H$, replace the last hydrogen atom by an atom of some other substance, such as potassium, ethyle, phenyle, and many other compound groups, while the new group retains its basic or alkaline character. This is the origin of organic bases. Example 5 illustrates it in a few artificial bases.

EXAMPLE 5. *Substitution bases.*

Ammonia = NH_3 = N, HHH.
Potassamide = N, HHK. Phenylamide (Aniline) = N, HHPh (Ph = $C_{12} H_5$).
Ethylamine = N, HH Ae. Methylamine = N, HH Me. Amylamine = N, HH Am.
(Me = $C_2 H_3$ Am = $C_{10} H_{11}$).

In the usual substitutions of inorganic chemistry, the replacing body is generally analogous to that replaced. We do not find oxygen or chlorine replacing hydrogen, while hydrogen is replaced by metals, or metals by each other. But, in the new kind of organic substitutions, we find hydrogen replaced by bodies the most opposed to it, such as chlorine, or even by the group nitrous acid, and that without destroying the general chemical character of the group. The inorganic opposition or polarity between hydrogen and chlorine or nitrous acid, disappears. To take an example, the group acetic acid (See example 6), $C_4 H_3 O_3 HO$, admits of the 3 eqs. of H in the dry acid being replaced by 3 eqs. of chlorine, and the new body $C_4 Cl_3 O_3$, HO, is, like acetic acid, a volatile pungent acid, the chloracetic. In Naphthaline (See Example 7), $C_{20} H_8$, one or more of the eqs. of hydrogen may be replaced by chlorine, bromine, or nitrous acid, and yet the group or type remains entire. In eniline, $C_{12} H_7 N$ (See Example 8), 1 eq. of hydrogen may be replaced by chlorine, bromine, or nitrous acid, and the resulting group is still a volatile base. But this fact is not inconsistent with the existence of compound radicals, for, as I have already hinted, aniline is ammonia, in which 1 eq. of hydrogen has been replaced by 1 eq. of the radical phenyle. Now, in chloraniline, the radical which replaces 1 eq. of hydrogen in ammonia is not phenyle, but chlorophenyle, derived from phenyle by substitution of 1 eq. Cl. for 1 eq. H. In nitraniline, it is nitrophenyle, also derived from phenyle by substitution of 1 eq. NO_4 (= X) for 1 eq. of hydrogen.

EXAMPLE 6. *Substitution Acids.*

Acetic Acid.		Hydrochloric Acid.		Chloracetic Acid.
$C_4 H_3 O_3$,	+ HO +	Cl_6 = 3 H Cl +	$C_4 Cl_3 O_3$, HO.	

EXAMPLE 7. *Substitution Products from Naphthaline.*

Naphthaline=$C_{20}H_8$. Chlonaphthase=$C_{20}H_7Cl$. Chlonaphtese=$C_{20}H_6Cl_2$. Bronaphtose = $C_{20}H_4Br_4$. Nitronaphtase = $C_{20}H_7X$. ($X = NO_4$).

EXAMPLE 8. *Substitution Bases from Aniline.*

Aniline = $C_{12}H_7N$. Chloraniline = $C_{12}H_6ClN$. Nitraniline = $C_{12}H_6XN$.
or $NH_2 + C_{12}H_5$ or $NH_2 + C_{12}H_4Cl$ or $NH_2 + C_{12}H_4X$.

We need not, then, reject the doctrine of types or groups because we admit that certain groups play the part of radicals.

The next important fact is the existence of coupled compounds, as they are called. In these, a molecule, often very complex, is simply added to another group, producing a new compound, generally retaining the chemical character of the latter, but differing from it in external physical properties. One example will suffice. Formic acid, a liquid acid of a comparatively simple molecule, takes up 1 eq. of the far more complex liquid body hyduret of beozoyle, or oil of bitter monds, and the result is a crystalline acid, the formobenzoic. Here no substitution has taken place, but simply the addition of the oil to the acid, the acid character and neutralising power of which remain, for 1 eq. of the new acid neutralises as much alkali as 1 eq. of formic acid. The body added is called the copula. It is exceedingly probable, since many such coupled compounds are known, that many of the complex molecules of animal and vegetable compounds are thus constructed. If we imagine a base, such as aniline, to be coupled with various organic products, we shall obtain a number of formulæ analogous to those of the alkaloids. The following example, No. 9, shows the composition of formobenzoic acid:—

EXAMPLE 9.

Hydrated Formic Acid.		Oil of Bitter Almonds.		Hydrated Formobenzoic Acid.
C_2HO_3, HO	+	$C_{14}H_6O_2$	=	$C_{16}H_7O_5, HO$.

It may here be noticed, that substitution products, once formed, may be viewed as coupled compounds, and may, indeed, be such. It is the mode of formation which differs. Thus, to take an example already given,—if in ammonia, NH_3, 1 eq. of hydrogen be replaced by 1 eq. phenyle, $C_{12}H_5$, we obtain by substitution aniline, $= NH_2 + C_{12}H_5 = C_{12}H_7N$. But, if we suppose 1 eq. phenyle, $C_{12}H_5$, to be added as a copula, to 1 eq. amide, NH_2, we obtain the same result, $NH_2 + C_{12}H_5$. Many coupled compounds, such as formobenzoic acid, cannot be viewed as substitution products, but all substitution products may be also regarded as coupled compounds. Both methods are, doubtless, employed by nature in building up the complex molecules of organic bodies.

The next important fact, which indeed is a corollary of the atomic theory, is the existence of isomeric and polymeric bodies,—that is, of compounds having the same composition in 100 parts, but totally distinct properties. This principle appears in inorganic chemistry, as in the allotropic modifications of elementary bodies, which depend, no doubt, on the existence of groups or molecules differing in the absolute numbers of ultimate atoms. Phosphorus, kept for some time at a high temperature, becomes brown, and greatly less combustible. Heated still higher, it returns to its colourless and highly combustible state. Charcoal and diamond, and the two irreconcileable crystalline forms of sulphur, differ in like manner, obviously from a cause such as is here indicated. The peroxide of tin, SnO_2 occurs in two forms,—one insoluble in acids, the other soluble. The latter is believed to be simply SnO_2,—that is, the molecule is made up of that number of atoms. The former is supposed to be Sn_5O_{10}, and its molecule to consist of 5 of the other, or 15 atoms instead of 3, at least of a number of atoms 5 times greater. The same kind of difference exists in the modifications of titanic acid and of other oxides. But in organic chemistry, this principle becomes far more important. We have in aldehyde, $C_4H_4O_2$, a liquid, which when preserved in

sealed tubes, loses all its peculiar properties, without change in its composition, and is converted into two new compounds; one, elaldehyde, an oily liquid, far less volatile than aldehyde, and not yielding acetic acid when exposed to the air, as aldehyde does, and a crystalline solid, metaldehyde. Both have the same composition in 100 parts as aldehyde, but one of them is believed to consist of C_{12} H_{12} O_8, the other may be C_8 H_8 O_4, in which case both are polymeric with aldehyde; or they may be strictly isomeric with aldehyde, and consist of the same number of atoms, differently arranged. Again, aldehyde, C_4 H_4 O_2 (and the two modifications of it above described, also), agrees entirely in the relative proportion of the elements with acetic ether, and with butyric acid. This is seen in the following table:—

EXAMPLE 10.

Aldehyde C_4 H_4 O_2	Elaldehyde C_8 H_8 O_4 ?	Metaldehyde C_{12} H_{12} O_8.
	Acetic Acid.	Oxide of Ethyle.
Acetic Ether, C_8 H_8 O_4	= C_4 H_3 O_3 +	C_4 H_5 O.
	Dry Butyric Acid.	Water.
Butyric Acid, C_8 H_8 O_4	= C_8 H_7 O_3 +	HO

Cyanate of ammonia (See below, Example 17) passes, without loss or gain of any element, into urea. It is extremely probable that albumen and fibrine are isomeric or polymeric. At least, the fibrine of blood or of flour, if left covered with water, is dissolved, a small part undergoing decomposition, and the solution is found to contain albumen, coagulable by heat and acids.

The next doctrine is that of homologous bodies, which has gradually developed itself, but was first so named by Gerhardt. It is merely the observation, that when to a well-marked organic compound, type, or group, we add a certain quantity of carbon and hydrogen, generally C_2 H_2, every such addition produces a new compound, analogous in properties to the first. Thus, in the large table, No. 11, if we add, to the radical methyle, C_2 H_3, we have ethyle; another addition gives us propionyle, another butyle, another valeryle, and so on up the scale. It is quite obvious, that if to all the derivatives of methyle, we make similar additions, each will yield a similar series of homologous compounds. Gerhardt attempted some years since, to apply this principle to the classification of all organic bodies, but the science was not then, and is not yet, ripe for that. Still his attempt has been very useful. The large table illustrates the application of this principle to that series of compounds to which, at present, it applies the best; that is, to such radicals as ethyle and methyle, and the rest of that series. The first column contains the radicals, increasing regularly by C_2 H_2. Of these the first six perhaps are known in the separate form. [1] The next column gives their oxides, of which ether is the type; the next, the hydrated oxides, of which the type is alcohol. Of columns 2 and 3 several are already known; in 3, wood spirit, alcohol, amylic alcohol, ethal, and I believe one or two more, higher in the scale. In column 5, we have the first product of oxidation or dehydrogenation of the alcohols, aldehyde being the type. Of the aldehydes several are known. Column 4 gives the supposed radicals of which the aldehydes are the hydrated oxides, and which are derived from the original radicals by the loss of 2 eqs. of hydrogen. Column 6 gives the teroxides or acids of these radicals, of which acetic acid is the type. These are formed by a fuller oxidation of the alcohols, and all are known up to 38 eqs. of carbon, inclusive, and three beyond 40 eqs. This point is particularly curious, because these acids abound in nature, and many of them are useful. They are all volatile without decomposition; all but the two first are oily acids; and all, above a certain point in the scale, are at ordinary temperatures solid or fat acids. They all contain, as may be seen by adding the 1 eq. of water to the dry acid, an equal, and that always an even number of eqs. of carbon and hydrogen, and in every

[1] Observe, that hydrogen is the foundation of the series, for methyle is H + C H_3, and every radical in this list may be made to replace H.

EXAMPLE 11.

	Each cyanide i nitryle on	HS, an.	Isomeric.	
	Nitryle.		Amide.	Base.
H_3, HO	C_2 H N	, HS	NH_2+C_2 H_3	C_2 H_5 N ... Methylamine.
H_3, HO	C_4 H_3 N	, HS	NH_2+C_4 H_5	C_4 H_7 N ... Ethylamine.
H_3, HO	C_6 H_5 N	, HS	NH_2+C_6 H_7	C_6 H_9 N ... Propylamine.
H_3, HO	C_8 H_7 N	, HS	NH_2+C_8 H_9	C_8 H_{11} N ... Butylamine.
H_3, HO	C_{10} H_9 N	, HS	NH_2+C_{10} H_{11}	C_{10} H_{13} N ... Amylamine.
H_3, HO	C_{12} H_{11} N	, HS	NH_2+C_{12} H_{13}	C_{12} H_{15} N
H_3, HO	C_{14} H_{13} N	, HS	NH_2+C_{14} H_{15}	C_{14} H_{17} N
H_3, HO	C_{16} H_{15} N	, HS	NH_2+C_{16} H_{17}	C_{16} H_{19} N
H_3, HO	C_{18} H_{17} N	, HS	NH_2+C_{18} H_{19}	C_{18} H_{21} N
H_3, HO	C_{20} H_{19} N	, HS	NH_2+C_{20} H_{21}	C_{20} H_{23} N
H_3, HO	C_{22} H_{21} N	, HS	NH_2+C_{22} H_{23}	C_{22} H_{25} N
H_3, HO	C_{24} H_{23} N	, HS	NH_2+C_{24} H_{25}	C_{24} H_{27} N
H_3, HO	C_{26} H_{25} N	, HS	NH_2+C_{26} H_{27}	C_{26} H_{29} N
H_3, HO	C_{28} H_{27} N	, HS	NH_2+C_{28} H_{29}	C_{28} H_{31} N
H_3, HO	C_{30} H_{29} N	, HS	NH_2+C_{30} H_{31}	C_{30} H_{33} N
H_3, HO	C_{32} H_{31} N	, HS	NH_2+C_{32} H_{33}	C_{32} H_{35} N
H_3, HO	C_{34} H_{33} N	, HS	NH_2+C_{34} H_{35}	C_{34} H_{37} N
H_3, HO	C_{36} H_{35} N	, HS	NH_2+C_{36} H_{37}	C_{36} H_{39} N
H_3, HO	C_{38} H_{37} N	, HS	NH_2+C_{38} H_{39}	C_{38} H_{41} N
H_3, HO	C_{40} H_{39} N	, HS	NH_2+C_{40} H_{41}	C_{40} H_{43} N
H_3, HO	C_{42} H_{41} N	, HS	NH_2+C_{42} H_{43}	C_{42} H_{45} N
H_3, HO	C_{54} H_{53} N	, HS	NH_2+C_{54} H_{55}	C_{54} H_{57} N
H_3, HO	C_{60} H_{59} N	, HS	NH_2+C_{60} H_{61}	C_{60} H_{63} N

case 4 eqs. of oxygen (See Example 12). Along with this analogy in composition, there is, as we see, a marked analogy of properties, and as we rise in the scale, the physical characters vary regularly as the amount of C H increases. Every addition of $C_2 H_2$ raises the fusing and boiling points 19 degrees. This applies to the boiling points in all the other columns, as far as known.

EXAMPLE 12 *Volatile Oily Acids of the Series* $(C_2 H_2)_n O_4$.

Formic Acid	$C_2 H_2 O_4$	Œnanthylic Acid	$C_{14} H_{14} O_4$
Acetic	$C_4 H_4 O_4$	Caprylic	$C_{16} H_{16} O_4$
Propylic	$C_6 H_6 O_4$	Pelargonic	$C_{18} H_{18} O_4$
Butyric	$C_8 H_8 O_4$	Capric	$C_{20} H_{20} O_4$
Valerianic	$C_{10} H_{10} O_4$	Margaritic	$C_{22} H_{22} O_4$
Caproic	$C_{12} H_{12} O_4$	Laurostearic	$C_{24} H_{24} O_4$
Cocinic Acid	$C_{26} H_{26} O_4$	Margaric	$C_{34} H_{34} O_4$
Myristic	$C_{28} H_{28} O_4$	Bassic	$C_{36} H_{36} O_4$
Benic	$C_{30} H_{30} O_4$	Balenic	$C_{38} H_{38} O_4$
Ethalic	$C_{42} H_{42} O_4$		
Behenic Acid	$C_{42} H_{42} O_4$	Cerotic Acid	$C_{54} H_{54} O_4$

Melissic Acid $= C_{60} H_{60} O_4$.

Derivation of the above Acids from Sugar by Deoxidation.

Sugar,	$C_{12} H_{12} O_{12} - O_8 = C_{12} H_{12} O_4$	= Caproic Acid.
2 eqs. of Sugar,	$C_{24} H_{24} O_{24} - O_{12} = 3(C_8 H_8 O_4)$	= Butyric Acid.
2 eqs. of Sugar,	$C_{24} H_{24} O_{24} - O_{20} = C_{24} H_{24} O_4$	= Laurostearic Acid.
5 eqs. of Sugar,	$C_{60} H_{60} O_{60} - O_{52} = 2(C_{30} H_{30} O_4)$	= Benic Acid.
5 eqs. of Sugar,	$C_{60} H_{60} O_{60} - O_{56} = C_{60} H_{60} O_4$	= Melissic Acid.

While on these volatile acids, I may mention that they may be formed, according to Liebig, in the animal body in two ways, by the deoxidation of starch or sugar, or by a process of fermentation. The most recent researches tend to establish this doctrine; for if we compare the composition of sugar (sugar of milk or cane sugar + 1 eq. water) with that of these acids, we see that they differ in oxygen alone. Sugar is $C_{12} H_{12} O_{12}$, and caproic acid is $C_{12} H_{12} O_4$. 2 eqs. of sugar, $C_{24} H_{24} O_{24}$ yield 3 eqs. of butyric acid ($C_8 H_8 O_4$), and O_{12}, or 1 eq. of Laurostearic acid, and 20 of oxygen. Lastly, taking the acids higher in the list, 5 eqs. of sugar yield either 2 eqs. of benic acid and 52 eqs. of oxygen, or 1 eq. of melissic acid and 56 of oxygen. (See Ex. 12.) Now this last acid is formed by the bee from sugar, being found in wax. Caproic acid is found in butter, and butyric acid also, as well as caprylic acid. Capric acid is found in goat's fat, and, I believe, goat's butter. Moreover, we can produce, artificially, from sugar, formic acid, acetic acid, propylic acid, and butyric acid; and I have no doubt we shall produce in time the whole of them. Amylic alcohol, the alcohol of valerianic acid, is formed in the fermentation of starch sugar, as is also capric acid, and probably margaric acid. I have found valerianic acid in old plum jam and in butter.

It is well known that in all fats and fat oils, these and other oily acids are combined with glycerine; but the composition of glycerine is also very close to that of sugar, from which it differs in containing a little more hydrogen and a little less oxygen, so that it also may be derived from sugar by deoxidation.

The next, the 7th column, contains the ammonia salts of the acids, all of which are known; and in the 8th column are given the compounds called nitryles, derived from these ammonia salts by the removal of 4 eqs. of water. It will be seen by the 9th column that the composition of the nitryle, belonging to any radical, is the same as that of the cyanide of the radical next below it in the scale. Several nitryles are known, and some of them are the cyanides of the next radical below; others appear to be isomeric only with the cyanides.

The next, the 10th, column gives the chlorides of the radicals; the next the sulphurets; and the next the compounds of the sulphurets, with sulphuretted

hydrogen. Of all these, several are known. The latter represent the alcohols, in which O_2 is replaced by S_2, and mercaptan is the type of them. They are horribly fetid, combining the most offensive odours of assafœtida, garlic, onions, leeks, &c., in unparalleled variety and intensity.

The two last columns give, first, the compounds of each radical with amide or ammonia, minus hydrogen; and secondly, the same arranged empirically, as the results of analysis. These bodies are all, so far as known, bases, probably all volatile bases, oily below, crystalline higher up; and those lowest in the list,—discovered about a year ago by Wurtz,—are so analogous to ammonia that they may readily be confounded with it. Their empirical formulæ, as given in the last column, hardly show this analogy; but let us consider each as NH_3, in which H is replaced by a radical, and the analogy in constitution comes out. This is seen in the previous column, for we may consider ammonia as $NH_2 + H$; methylamine as $NH_2 + C_2 H_3$; ethylamine as $NH_2 + C_4 H_5$, and so on. I shall have to return to these bases.

This table illustrates the advantage of symbols; for if we have the first horizontal line, containing one radical and its derivatives (and the table as it stands does not contain one-half of those known of the radical ethyle, for example), we can then construct the whole table by successive additions of $C_2 H_2$ in every vertical column. And as in one column, that of the acids, the recent progress of chemistry has enabled us to fill in more than 20 of the series, while in all the others several members are known, and new ones are daily added, we cannot doubt that the principle on which it is founded is a natural law.

Nor is this a solitary instance of the principle of homologous compounds. The benzoic acid (Example 13) belongs to another such series, of which four or five members are known, and there also the common difference is $C_2 H_2$. There also a series of volatile bases is known, of which aniline is the type, and which closely resemble certain natural bases. These bases are derived, by substitution, from the carbo-hydrogens in the middle column.

EXAMPLE 13. *Benzoic Series.*

Benzoic Acid	$= C_{14} H_6 O_4$	Benzole	$= C_{12} H_6$	Aniline	$= C_{12} H_7 N$
Cinnamic	$= C_{16} H_8 O_4$	Toluole	$= C_{14} H_8$	Toluidine	$= C_{14} H_9 N$
Cuminic	$= C_{18} H_{10} O_4$	Cumole	$= C_{16} H_{10}$	Cumidine	$= C_{16} H_{11} N$
Cymenic	$= C_{20} H_{12} O_4$	Cymole	$= C_{18} H_{12}$	Base	$= C_{18} H_{13} N$

Of late years, much light has been thrown on the formation of organic compounds in plants and animals by the study of the metamorphoses or transformations of organic substances. It is evident that, considering the high atomic weight and complex molecules of these bodies, and bearing in mind also that a change in the arrangement of the particles may cause a change of properties, as well as an alteration of their proportion, organic compounds must be especially liable to such transformations. Some of these have long been familiar, as the change of starch into grape sugar, that of sugar into alcohol and carbonic acid, that of cyanate of ammonia into urea, and that of oil of bitter almonds into benzoine. In these cases, nothing is added, and nothing removed; but a new arrangement, more stable under the circumstances, is effected. There are now many other examples, some of which I shall have to mention hereafter. But there is another very common kind of transformation where some compound, such as ammonia, is added. Here the only difference is, that the particles newly arranged, were not previously combined in one compound, but undergo the change when brought together. Examples of this are found in the formation of many amides, such as oxamide, and such bodies as murexide. (This was illustrated by the instantaneous formation of these bodies.) A third and most important class of transformations is that effected by heat or destructive distillation, the products of which, especially when nitrogen is present, are singularly varied, and are daily becoming better known. Fermentations

belong chiefly to the first class of transformations. Putrefaction is a kind of fermentation, where oxygen is excluded or but sparingly admitted after the process has begun; and decay is a transformation under the influence of oxygen, yielding, for the most part, the ultimate products, carbonic acid, water, and ammonia.

The newest and best definition of putrefaction and fermentation is that of Liebig. A *putrescible* body is one generally, perhaps always, nitrogenised, which, in contact with air, moisture, and a certain temperature, undergoes a spontaneous decomposition. It is then *putrescent* or a *ferment*. A fermentiscible body is one which, by itself or in solution in water, does not undergo any decomposition; but when in contact with a putrescent body, is resolved into new products, and then is in a state of fermentation. As fermentation is produced by the communication of motion from the *atoms*, not the *molecules*, of the putrescent body to the *atoms* of the fermentescible one, the process requires time, and is not instantaneous, like a common precipitation; and as putrefaction is itself propagated in the same way, so it also is a slow process. Moreover, as the ferment can only act as long as its atoms are in motion, its power to cause fermentation must cease as soon as its own decomposition is complete, and not before. Hence a given weight of ferment can only cause the fermentation of a limited quantity of sugar or other fermentescible bodies.

One very striking result of the progress of our knowledge in regard to the points I have briefly alluded to is this, that we daily acquire in a greater degree the power of artificially imitating the natural processes in plants and animals, and of thus producing organic compounds, very often entirely analogous to natural products, and sometimes identical with them. It is evident that nothing can so well prove our real progress as this, and I shall, therefore, dwell on it a little, since it forms the peculiar feature of the present state of the science, and holds out the greatest prospect of future and still greater progress.

I would point out, first, that the natural process in vegetables is distinct from, and indeed opposed to, that which goes on in the animal body. In plants, their food consists of the least complex compounds; of carbonic acid, water, ammonia, sulphuric acid, and the mineral salts found in their ashes. Now, under the influence of light, the plant exerts a prodigious power of deoxidation, insomuch that in the formation of cellulose or woody fibre, from 12 eqs. of CO_2 and 8 eqs. of HO, a quantity of oxygen is separated and sent into the atmosphere equal to that contained in the CO_2,—that is, 24 eqs. of O from the above quantities. The same thing occurs in sugar, starch, and gum, only that the amount of HO is larger. Between CO_2 and HO, the food of plants and cellulose, &c.,—in forming which all the oxygen of the carbonic acid is separated,—we have a long list of compounds intermediate in amount of oxygen, such as (see Example 14) oxalic, malic, citric and tartaric acids, &c.; and on the other side of cellulose, &c., we have a long list of bodies with less oxygen than starch or sugar, namely, the fat or fixed oils, the essential oils and the resins. Taking the whole process, from $CO_2 + HO$ to oil of turpentine $C_{10} H_8$ or $C_{20} H_{16}$, we have a continuous deoxidation, a process of reduction. But this process has another and a still more important character, namely, that from less complex molecules more complex are built up or constructed. It would appear that this can only, or most easily and effectually, be effected by aid of a deoxidising or reducing process.

Example 14.

Food of plants. Carbonic Acid, CO_2. Water, HO. Ammonia, NH_3. Sulphuric Acid, SO_3

$2(CO_2) - O = C_2O_3 =$ Oxalic acid. $2(C_2O_3) + 2HO - O_4 = C_4 H_2 O_4$; and

$2(C_4 H_2 O_4) = C_8 H_4 O_8 =$ Malic acid. $3(C_8 H_4 O_8) = C_{24} H_{12} O_{24}$, and

$C_{24} H_{12} O_{24} + 16 HO - O_{12} = 2(C_{12} H_{14} O_{14}) = 2$ eqs. grape sugar.

When we add ammonia, NH_3, we have the elements of the organic bases and of some colouring matters; and with the aid of sulphuric acid, SO_3, and of salts, particularly phosphates, we have those of the albuminous or sanguigenous

principles, albumen, fibrine, and caseine, the molecules of which are still more complex. All that is known on this subject leads to the conclusion that these bodies can only be *formed* in plants, although they may be *transformed* one into the other in the animal body. No evidence has yet been produced in favour of the opinion that the animal can form any one sanguigenous compound from food destitute of such compounds. I do not say that this is impossible, but that it is simply an idea conceived, but never yet proved. And the reasons why I believe that it does not happen are two-fold; first, that it is unnecessary, nature providing these compounds in the food of animals; and, secondly, that, so far as we know, very complex atoms can only be constructed out of less complex ones under the influence of a deoxidising process, or at least not by direct oxidation. Now, the process in plants is one of deoxidation, while that of animals is one of powerful oxidation; the result of which is, that the tissues or the food are finally transformed into carbonic acid, water, ammonia, sulphuric acid, and salts, the very compounds which form the food of plants. Indeed, the mutual dependence of plants and animals, the fact that neither overpowers the other, and the still more striking fact, that the atmosphere, by their mutually compensating action on it, is kept uniform in composition,—all these considerations prove, that while the chief function of plants is to construct out of simpler molecules the complex atoms of all nutritious matter, the chief result of animal life must be to break up, by oxidation, these complex molecules, and bring them again into a state fit for the food of plants.

Now, when we examine the changes in the animal body, we find, just as in the vegetable, a series of compounds intermediate between the tissues and the ultimate oxidised products, which form the food of plants, carbonic acid, water, ammonia, and sulphuric acid. Such bodies are the acids of the bile, one of which is cholalic acid + glycocoll, the other cholalic acid + taurine; cholesterine; the oils and fats, kreatine, kreatinine, inosinic and lactic acids, in the juice of the muscles, and uric acid, urea, allantoine, oxalic acid, &c., in the urine. One point must be alluded to, namely, the formation of fat in the body, since fat is derived from starch, or sugar, by deoxidation, as in plants. But observe, that there is little fat formed, except where respiration and consequently oxidation is defective, or when a large amount of farinaceous food is taken, which of course diminishes the relative amount of oxygen. The way to increase the amount of fat is to give abundance of such food, and to interdict motion. Besides, fat may also be formed by oxidation, if that oxidation acts chiefly on some of the constituents, thus splitting up a complex molecule perhaps into two parts, one of which takes up all the oxygen, the other becomes fat by losing oxygen. Indeed it is probable that when fat is formed from sugar, it is because a part of the oxygen required for the oxidation of the tissues is taken from the sugar, when respiration does not supply the whole. The fat itself, when formed, is oxidised like the other compounds.

After this sketch of the general nature of the processes in plants and animals, you are prepared to understand that we are now able in some degree to imitate both. It is here that organic chemistry has of late done most, and will yet do much more. But, as might be expected, we find it much easier to destroy, by various means, the complex molecules of organic nature, than to build up such molecules from simpler ones. The most powerful agent we have is oxidation, and by its means we can produce, from complex organic bodies, a large number of less complex products found in nature. We possess, also, the various processes of fermentation, putrefaction, and decay, by which we can produce similar results. Thus by oxidising uric acid, we can form urea, allantoine, oxalic acid, ammonia, carbonic acid, and water, all of which are formed from it in the body. By oxidising the albuminous or sanguigenous bodies, we can form such products as formic, acetic, propylic, butyric, valerianic, and benzoic acids, besides various crystalline products, such as glycocoll, leucine, and tyrosine,—the two former found in the body, the latter not yet found in nature,—as well as oil of bitter almonds, hydrocyanic acid, ammonia, and of course CO_2 and HO.

By oxidising oils, we can produce many of the volatile oily acids. By oxidising sugar or starch, we can produce oxalic and formic acids. By the fermentation of sugar, we can produce (see Example 15) first, alcohol and carbonic acid; second, lactic acid; third, gum and mannite; fourth, butyric acid; fifth, amylic, alcohol, and capric acid. By the fermentation of amygdaline, we can produce oil of bitter almonds, hydrocyanic acid, sugar, and formic acid. The oil of bitter almonds, by oxidation, becomes benzoic acid. Hippuric acid (see Example 16), boiled with hydrochloric acid, yields glycocoll and benzoic acid. By the oxidation of salicine, we form salicylic acid and also the oil of spiræa. Wood, when distilled, yields, among other products, pyroxylic spirit, or hydrated oxide of methyle; and we find in the oil of *Gaultheria procumbens*, salicylic acid combined with oxide of methyle. By the fermentation of salicine, we form grape sugar and saligenine. By that of asparagine, we obtain succinic acid, which is also formed in the fermentation of malate of lime. In all these cases, and many more, complex molecules are resolved into less complex, as we approach nearer to CO_2, HO and NH_3.

EXAMPLE 15. *Lactic and Butyric Fermentations.*

Sugar of Milk. Lactic Acid. Butyric Acid. Carbonic Acid. Hydrogen.

$$C_{12}H_{12}O_{12} = 2(C_6H_6O_6) = C_8H_8O_4 + 4CO_2 + H_4$$

EXAMPLE 16. *Decomposition of Hippuric Acid.*

Hippuric Acid. Water. Benzoic Acid. Glycocoll.

$$C_{18}H_9NO_6 + 2HO = C_{14}H_6O_4 + C_4H_5NO_4.$$

But although the task is more difficult, we have also made some progress in the opposite process, that of building up more complex out of less complex molecules. It is true, we have succeeded, as yet, in very few instances in thus producing natural products. Artificial urea (see Example 17), however, is a proof that it is not impossible. A solution of cyanate of ammonia passes rapidly into urea when warmed. A solution of cyanogen yields 10 or 11 products, several of which are much more complex than cyanogen, as urea, oxalate of ammonia, &c., and a black body, certainly more complex than cyanogen. Malate of ammonia yields aspartic acid, &c. But we have succeeded in producing a large number of compounds, which, if not identical with natural compounds, are perfectly analogous to them. Thus we have formed a large number of bases, both volatile and fixed, which approach closely in composition, and also in properties, to natural bases. The bases in the large table (No. 11) so far as known, are examples of this. Here we add, to the molecule of amide, a still more complex molecule, C_2H_3, C_4H_5 or $C_{10}H_{11}$, replacing H in NH, and we thus obtain bases closely analogous to ammonia. Aniline, already mentioned, is a similar base of another series. In it, amide is combined with $C_{12}H_5$, replacing H in NH_3, and this series of bases is closely analogous to the natural volatile bases, coniine and nicotine. Again (see Example 18), we can replace two or all three of the eqs. of H in NH_3 either by 2 or 3 eqs. of ethyle, methyle, amyle, or phenyle, &c., or by 1 or 2 of one of these, and 2 or 1 of another. Nay, further (see Example 8), we can replace 1 and sometimes 2 eqs. of H, in aniline for example, by NO_4, which then, strange to tell, plays the part of H, and the result in this case is nitraniline, a beautiful crystalline base, and the type of a series containing the same elements as the natural fixed bases, morphine, quinine, strychnine, and the like. One very striking example of the power we have of building up more complex out of less complex molecules is afforded by the oil of bitter almonds. When this oil is acted on by ammonia (see Example 19), there is formed from 3 eqs. of the oil and 2 of NH_3, 6 of HO being separated, the body $C_{42}N_2H_{18}$. Here is a complex molecule. The body, hydrobenzamide, is neutral; but when boiled with potash, 2 eqs. coalesce to form one of a base, amarine $C_{84}N_4H_{36}$, which much resembles natural bases. By the same

process, from oil of bran or furfurole (see Example 20) is produced by NH_3, first, furfuramide, a neutral body; and when this is boiled with potash, 2 eqs. coalesce to form one of a new base, furfurine, which not only resembles the natural bases, but is actually used as an antiperiodic remedy. It is to be noted that almost all the processes by which we produce more complex from less complex molecules are processes of reduction. The action of ammonia tends to remove oxygen in the form of water; and a whole class of bases are obtained by the action of a very powerful reducing agent, hydrosulphuret of ammonia, on substitution products in which H has been replaced by NO_4. It can hardly be doubted that, in process of time, we shall discover the means of producing the natural bases, such as morphine and quinine. We already know how we could with certainty produce coniine and nicotine if we had only the corresponding carbonhydrogens, bearing the same relation to these bases as benzole or hyduret of phenyle does to aniline (see Example 21). And these oils or carbohydrogens will very probably soon be discovered. In the same way, if we can discover, as we probably shall, the true constitution of morphine and quinine, and the substance from which the plants form them,—which will one day be done by studying the juices of these plants in all stages of growth,—we shall then have little difficulty in forming such bases. We have the principle and the process, the materials only are wanting.

EXAMPLE 17. *Artificial Urea.*

Cyanate of Ammonia.		Urea.
$C_2\,NO,\,NH_2,\,HO$	=	$C_2\,H_4\,N_2\,O_2$.

EXAMPLE 18. *Bases derived from the Ethyle Series, &c.*

Methylamine	= N, H H Me.	Ethylamine	= N, H H Ae.
Aniline	= N. H H Ph.	Ethylmethylamine	= N, H Me Ae.
Triethylamine	= N, Ae Ae Ae.	Ethylamylamine	= N, H Ae Am.
	Amylamine = N, H H Am.		
	Diethylamine = N, H Ae Ae.		
	Ethylamylaniline = N, Ae Am Ph.		

EXAMPLE 19. *Formation of the base Amarine.*

Oil of bitter Almonds.		Ammonia.		Water.		Hydrobenzamide.
$3\,(C_{14}\,H_6\,O_2)$	+	$2\,N\,H_3$	=	$6\,H\,O$	+	$C_{42}\,H_{18}\,N_3$.
$2\,(C_{42}\,H_{18}\,N_3)$	=	$C_{84}\,H_{36}\,N_4$	=	Amarine.		

EXAMPLE 20. *Formation of the base Furfurine.*

Oil of Bran.		Ammonia.		Water.		Furfuramide.
$C_{15}\,H_6\,O_6$	+	$N\,H_3$	=	$3\,H\,O$	+	$C_{15}\,H_6\,N\,O_3$.
$2\,(C_{15}\,H_6\,N\,O_3)$	=	$C_{30}\,H_{12}\,N_2\,O_6$	=	Furfurine.		

EXAMPLE 21. *Analogy between Aniline, Coniine, and Nicotine.*

Aniline, $C_{12}\,H_7\,N$, is derived from Benzole, $C_{12}\,H_6$.
Coniine, $C_{15}\,H_{15}\,N$, is derived from a Carbohydrogen, $C_{15}\,H_{14}$.
Nicotine, $C_{10}\,H_8\,N$, is derived from a Carbohydrogen, $C_{10}\,H_7$.

It is tolerably certain, that the materials out of which plants form the albuminous bodies are sugar and ammonia, or some compound of ammonia along with sulphuric acid, and that the building up process is one of reduction, chemically speaking. But here another principle comes into action, namely, the vital principle, that which gives to the tissues an organised structure. And this is beyond our reach. I do not regard it as impossible, or even very improbable, that we may one day succeed in producing a compound with the chemical composition and chemical characters of albumen, fibrine, caseine, or gelatine. But

the formation of a cell, the foundation of all organised structure, is not entirely a chemical, but partly a vital process, and therefore I do not anticipate the possibility of our ever producing fibres, membranes, blood globules, or even the imperfect structure observed in white of egg. But, notwithstanding, nothing can be more certain than this, that every change in a plant or animal is, apart from structure, a purely chemical change, and that these changes cannot possibly be understood without the aid of chemistry. Hence chemistry is absolutely indispensable to a knowledge of the laws, both of health and disease, to physiology and to pathology. When we consider that the plant from CO_2, HO, NH_3, SO_3, and salts, produces the whole ascending series of vegetable products, ending in the albuminous bodies, and that in animals we have, as the result of oxidation, a descending series, ending in CO_2, HO, NH_3, SO_3, and salts, it is impossible to doubt that chemical action is a chief agent in vegetable and animal life. All these changes *are* chemical, and cannot be otherwise. They are influenced, indeed, essentially, by the vital force, but still they depend also on chemical laws, the very same laws as act in the mineral world, but here combined with, and subordinate to, the vital organising force.

Time will not permit me to dwell at length on the recent progress and present state of physiological and pathological chemistry. In truth, the genuine application of chemistry to physiology and pathology is yet in its infancy. We are hardly yet acquainted with the chemical nature of the best known fluids and solids of the body. In proof of this, I may refer to the urine. It is only of late years that we know that the acid re-action of healthy urine is due to the property possessed by phosphate of soda of dissolving free uric and hippuric acids. The extractive and colouring matters of the urine are hardly investigated; yet we already know that they contain a substance closely allied to, if not identical, as I believe it is, with one of the products of destructive distillation, or imperfect oxidation, namely, carbolic acid, a body nearly allied to creosote, and having many of its properties. Again, the presence of hippuric acid, of kreatine and of kreatinine in urine, is quite a recent discovery, due to Pettenkofer, Heintz, and Liebig. Further, it has been recently shown, that the compounds which give to the solid excreta their peculiar odour, are produced by the imperfect oxidation of albuminous matters, and may be formed artificially, but have not yet been studied. Thus we see that products of imperfect oxidation, such as uric acid, urea, oxalic acid, kreatine, kreatinine, hippuric acid, and the bodies just mentioned, as found in the extract of urine, and in the solid excreta, are removed from the body by the kidneys and intestines; while others, such as oily acids, escape through the skin on the sweat. But our knowledge of all these excreta is still imperfect. Then the fluid which pervades the muscles, and which contains, besides other bodies, albumen, kreatine, kreatinine, inosinic acid, a large amount of lactic acid, and acid phosphate of potash, has only been recently studied at all. The gastric juice is analogous to that fluid. In the blood, urea, cholesterine, fat, bile, and even uric acid, have lately been detected, as was to be expected, although the detection is very difficult, from the very small relative proportion of these matters. Perhaps bile is only found in the blood when its amount is morbidly increased, but cholesterine is more easily detected. The chemistry of the pancreatic juice, since Bernard's experiments on its power of dissolving or suspending fat, has become a grand desideratum. That of the brain and nerves may be regarded as hardly begun, and is of surpassing interest. During the past year, it has been shown by Liebig, that the fibrine of blood is a different body from that of the muscles—at least, that it has different properties. Here is another vast field for research, even in the healthy body. In the bile (see Example 22), after many laborious and excellent researches, the last investigation, that of Strecker, has really simplified the matter to a great degree, although there is much to do even there. It is now known that the bile is a salt or soap of soda—not, as was supposed of one acid, the choleic acid, combined with soda, but a mixture of two such salts, the acids in which differ. One of

these, cholеic acid, consists of a non-nitrogenous acid, now called cholalic acid, coupled with glycocoll, the same weak base which is obtained by the action of acids on gelatine and on hippuric acid. The other acid of the bile, cholеic acid, is composed of the same cholalic acid, coupled with taurine, a crystalline substance containing a large amount of sulphur. Hence one of the acids of bile contains sulphur, the other not. Strecker has also shown that the bile of the pig contains a peculiar acid, hyocholic acid, differing slightly in composition from the acids in the bile of other animals. We see, therefore, that we are only beginning to become well acquainted with the animal fluids and solids. But we are on the right path, and our progress will daily become more rapid, if we adhere to the inductive method, and do not allow ourselves to be blinded by theory.

EXAMPLE 22. *Composition of the Acids of the Bile.*

Cholic Acid,	C_{52}	H_{43}	NO_{12}	yields	Cholеic Acid,	C_{52}	H_{45}	NS_2	O_{14} yields
Cholalic Acid,	C_{48}	H_{40}	O_{10}		Cholalic Acid,	C_{48}	H_{40}		O_{10}
Difference,	C_4	H_3	NO_2		Difference,	C_4	H_5	NS_2	O_4
Add to this,		H_2	O_2 or 2 eqs. of wat.		Add to this		H_2		O_2
The sum,	C_4	H_5	NO_4, is Glycocoll.		The sum,	C_4	H_7	NS_2	O_6, is Taurine.

Let me here allude to the remarkable fact (see Example 23), that the alkaline re-action of the blood is due to the phosphate or carbonate of soda, and is essential to its fluidity, while the acid re-action of the juice of muscle depends on the presence of acid phosphate of potash. Free phosphoric acid is essential to the formation of tissues. It is present along with oil in the yolk of egg. Looking at the salts in blood, and in the juice of flesh, in another view, both of these salts are tribasic; but the former contains 2 eqs. of soda and 1 of basic water, the latter 1 eq. of potash and 2 of basic water, according to the observations of Graham. The latter has an acid, the former an alkaline, re-action; yet the blood and the juice of muscle are only separated by very thin membranes. They do not mix; for kreatine and lactic acid, which occur in the latter, are not found in the blood. Now, an acid and an alkaline liquid, in contact with moist solids, furnish some of the essential conditions of electric currents. And now we know, that electric currents, as has been shown by Matteucci and Dubois Reymond in different ways, do occur in the body. The wonderful arrangement I have alluded to, by means of the two phosphates, is probably somehow concerned in these currents. Observe, I by no means identify the vital force with electricity. On the contrary, I hold that electricity is only one of the results of chemical action going on under the influence of the vital force, whatever that may be. Here is another fertile field for research, and one of the highest interest.

EXAMPLE 23. *Soluble Phosphates of Blood and Flesh.*

Phosphate of Soda in blood, PO_5, 2 Na O, HO, has an alkaline re-action.
Phosphate of Potash in the juice of flesh, PO_5, KO, 2 HO, has an acid re-action.

On the whole, it appears that while organic chemistry, as an instrument of research, has made, and is making, very rapid progress, we are hardly yet doing more than beginning to use that instrument for the due investigation of physiology and pathology. But as we have every reason to think that we have found the true path, the prospect before us is extremely encouraging. There is no department of these subjects yet fully understood. Even the process of respiration has been of late repeatedly investigated, and with most valuable results.

It has been shown, for example, that an animal, which would soon die in an atmosphere containing a small per centage of carbonic acid more than the air does, as is the case in expired air, can live without inconvenience in an atmosphere containing 17 or 18 per cent. of carbonic acid, provided the amount of oxygen be also increased. This is explained by the fact, that venous blood, or

any other liquid, charged with carbonic acid, cannot give out that gas in an atmosphere of carbonic acid, nor in one where that gas is present in large proportion, neither can it take up the oxygen necessary to convert it into arterial blood. All this it can do in an artificial atmosphere containing much carbonic acid, if oxygen enough be present to allow of the diffusion. In hydrogen gas, the venous blood diffuses out its carbonic acid, and is, *pro tanto*, arterialised, but life can only be sustained for a very short time, by means of the residue of oxygen remaining in the blood. Death occurs here from want of oxygen, but not because the carbonic acid is retained, as in an atmosphere of carbonic acid, where death is instantaneous. The researches of Regnault and Reiset have also cleared up many other points in respiration, which I must here pass over.

Liebig has shown, that the absorption of carbonic acid by the blood is a chemical, not a mechanical process, depending in great part on the presence of alkaline phosphate (or phosphate and carbonate) of soda. Solutions of these salts, so different in all other respects, agree in absorbing carbonic acid in proportion to the amount of salt, 1 per cent. causing water to absorb twice as much as pure water, while blood absorbs about ten times as much as pure water. The absorption of oxygen in the lungs is partly mechanical, since a solution charged with carbonic acid, in contact with air, or agitated with it, gives out its carbonic acid, and takes up, in its stead, a certain amount of oxygen; and partly chemical,—the chemical action probably depending on the presence of a compound of iron, analogous to the protoxide. The poisonous action of hydrocyanic acid and of sulphuretted hydrogen probably depends on their decomposing this compound of iron instantaneously, and rendering it unfit for use.

In regard to nutrition, Liebig has also, in the forthcoming new and much enlarged edition of his "Letters on Chemistry," made various important remarks. The fact that animals die, with the symptoms of starvation, when fed exclusively on fibrine, on white of eggs, or on caseine, as well as when fed on sugar or starch, depends on the absence of the necessary salts, which, in flesh, are found partly in the fibrine, partly in the juice; in eggs, partly in the white, partly in the yolk; and in milk, partly in the caseine, partly in the whey. When the juice, yolk, and whey respectively, are removed, life can no longer be sustained, as on the original food. Further, the presence or the formation of oil or fat appears to be essential to the formation of cells, although the greater part of the fat is burned off or oxidised. It would appear that the liver produces some substance, possibly a ferment, which has a share in the production of fat in the body; but this requires investigation. As fat enters into the composition of nervous matter, this is a point of much interest, as we know not yet where the nervous matter is formed.

Again, the blood cannot exist, nor retain its fluidity, except as an alkaline fluid; and yet it would appear that tissues can only be formed in an acid fluid, where free phosphoric acid or acid phosphates exist. Such is the case with the juice of flesh. The yolk of egg contains, beside albumen, oil and free phosphoric acid, as well as salts. Hence the chick is developed from the egg alone.

If we examine the proportions of sanguigenous matter, and of non-nitrogenous elements of respiration in food, the latter being calculated as starch, we find the following general results:—

	Sanguigenous.	Starch.
Milk of the cow,	10	30
Human milk,	10	40
Leguminosæ (seeds),	10	20 to 30
Grain,	10	50 to 60
Fat mutton or pork,	10	27 to 30
Beef,	10	17
Hare,	10	2
Veal,	10	1
Potatoes, white and blue,	10	86 to 115
Rice and Buckwheat,	10	123 to 130

Here we see that milk and grain contain starch and sanguigenous matter in the proportion of from 30 to 60 of the former to 10 of the latter. Within these limits are the best proportions; 30 or 40 to 10 for growing, 50 or 60 for adult animals. It is obvious that beef, especially if lean, contains too little starch (fat calculated as starch); and that potatoes and rice contain too much. The indigestibility of veal by itself is accounted for. It is an approach to pure fibrine, and is also deficient in salt and iron. But the instinct of man has taught him to use a mixed diet, such as yields the proportions found in milk or in grain. Thus, he mixes fat bacon with beans; pork with pease-pudding; beef with potatoes and cabbage; mutton with rice; veal with ham, without any knowledge of the principle. Nay, if confined exclusively to lean flesh as solid food, he would find wine, beer, or spirit necessary to give the proper balance; for alcohol can replace fat or starch as an element of respiration. Added to lean flesh, it yields a food like milk; to fat flesh, a food like rice or potatoes. Of course, with a mixed diet, it is superfluous, if not injurious.

On the other hand, while a due proportion of the two chief elements of food is essential to nutrition as well as to respiration, it is to be observed, that the compounds of nitrogen, including the sanguigenous bodies, are exceedingly difficult of combustion or oxidation, combination with nitrogen depriving even phosphorus and nitrogen of their combustibility. Hence they are never oxidised in the body, so long as fat, starch, or sugar is present; and one function of the latter bodies is, to protect the tissues against the action of oxygen, which, in starvation, ultimately consumes them. In reference to the amount of heat given out, the chief elements of food stand as follows:—

100 parts of fat produce as much heat, when oxidised, as
240 of starch,
249 of cane-sugar,
263 of grape-sugar,
266 of brandy containing 50 per cent. of alcohol,
770 of lean flesh.

Again, while we know that the food may vary greatly, within certain limits, without affecting the vital processes, the excreta vary with every change of diet. We have, therefore, the valuable power of changing, for example, the nature of the urine by a diet founded on a knowledge of the composition of food. I would again refer to the forthcoming edition of Liebig's "Letters on Chemistry" for a succinct view of what has lately been done in this and in many other parts of the subject. I had hoped to have been able to lay before you an account of Liebig's own latest researches in Animal Chemistry, as well as of those of many other chemists, all of which will appear in the "Letters." But unfortunately, the last ten sheets, which, as well as an equal quantity of what is already thrown off of the new edition, are entirely new, have not yet reached me, so that my sketch of this part of the subject is necessarily imperfect. This is the less to be regretted, as your time would have compelled me to be very brief, so as not to do justice to the subject.

Finally, I think it will appear, even from this very imperfect sketch, that, without a knowledge of chemistry, real progress in true physiology and pathology is hopeless. These sciences have been deeply indebted to anatomy, and the increasing applications of the microscope are daily doing wonders. But to Anatomy, Chemistry must be added, because the changes we have to study, apart from structure, are chemical; and the next generation of great physiologists and pathologists will be men who shall combine the two methods of research. We may well envy our pupils and successors the rich harvest of valuable discoveries which certainly awaits them; for there never yet has been a time when the successful co-operation of all the sciences relating to physiology was so probable as now, or promised such vast results.

Part Fourth.

PERISCOPE.

MEDICINE.

MODE OF PROPAGATION OF MEASLES. BY DR PANUM OF COPENHAGEN

The Færöe Islands, situated, as is well known, between Shetland and Iceland, were in 1846 invaded by an epidemic of measles. The disease, which had commenced in April, ceased in October, and of 7782 inhabitants, more than 6000 suffered from it. Since 1781, it may be literally said, that not a single case of measles had been observed among them—hence all were ready to receive the contagion. Still there existed certain remarkable and exceptional conditions, which may assist in explaining the enormous extension of the epidemic. Not only are the Færöe Islands separated from the rest of the world by their geographical position; but, by a commercial monopoly which has existed for ages, the islanders have no right to sell the produce of their industry except at the government counting-house (*comptoir*), and here only can they procure articles for their own consumption. Such extreme isolation ensures an almost complete immunity from contagious diseases. The mean duration of life is in these islands very high, and the most considerable mortality occurs between 80 and 90.[1] The climate and mode of life are far from favourable; hence it is to the absence of epidemic and contagious influences that the low rate of mortality is attributable. Unfortunately no sooner does a contagious disorder find its way into the islands, than the extreme misery of the inhabitants causes it to assume a degree of intensity for which a parallel can scarcely be elsewhere found. While in Denmark measles is chiefly seen among infants, and for the most part spares adults, I have seen villages among the Færöe Islands, where of 100 peasants more than 80 were at the same time confined to bed. The Danish government was obliged to send out two physicians, Dr Manicus and myself. The circumstances above mentioned—the peculiar position of my field of observation—have enabled me to gather some information on the subject of the contagious property of measles, which may perhaps be read with interest.

There are seventeen inhabited islands, separated by straits, which the currents render it dangerous to cross. The mountainous nature of the soil compels the inhabitants to cluster together along the coasts. There have thus arisen villages, of which the most populous hardly number more than 200 inhabitants —the town of Thorshavn, the seat of government, contains only 800 souls. Each village forms a sort of family without communication with the neighbouring localities. To such a degree is isolation pushed, that the presence of a stranger, or of an inhabitant from a neighbouring village, is immediately known to the whole community, is often even set down in the calendar as a marvel, and long continues a subject of conversation. Fears excited by the epidemic of measles rendered intercommunication even more guarded. I could almost, in each village, follow the disease from its first appearance, tracing its progress from individual to individual. The reader will consequently understand what unusual facilities I possessed for studying the mode of propagation of measles, during four months spent on thirteen out of the seventeen islands.

The length of the period of incubation of measles is far from satisfactorily

[1] "La plus grande mortalité tombe entre 80 et 90 ans." We quote the author's words, as the fact recorded sounds rather startling.—Ed.

determined—different authors state it at eight, ten, and fourteen days. Others, with more reserve, assign no fixed and regular period to the first stage of the disease. In great cities it is impossible to ascertain at what precise moment an individual has been subjected to the contagious influence. To obtain results of sufficient accuracy, the observer must be placed in similar circumstances to myself on the Færöe Islands.

The first native attacked with measles was a labourer, now resident at Thorshavn. He had left Copenhagen on the 20th March, had arrived on the 28th in perfect health, and became ill on the 1st of April. About fourteen days afterwards his two most intimate friends showed the first symptoms of the disease. This case, which I did not personally witness, excited my attention and induced me to undertake a series of observations on the duration of the incubation.

On the 4th June, ten men of Tjornevig, sailing in the same boat, had taken part in a great fishing expedition with the inhabitants of another village. On the 18th June all ten were affected with measles, after two to four days of premonitory symptoms. They had been in contact only with the fishers from Westmannhavn, who were either convalescents from the same disease, or had been living in the midst of others affected by it. In from twelve to sixteen days after the appearance of the exanthema in these ten individuals, almost the whole population of the village were covered with the same eruption.

Here, then, was sufficient reason for suspecting that the contagion might lurk in the organism in a latent condition, or at least without exciting the specific eruption, for a period of from thirteen to fourteen days. In fifty-two localities I collected accurately the names of persons first affected with measles, the date of the appearance of the eruption, and the date of the invasion of the disease in the inhabitants to whom it spread. It would be worse than useless to introduce each of these tables here; it is enough to say that everywhere facts confirmed my original supposition, and that no exception to the rule occurred. I shall merely mention, in a summary manner, some observations which establish, in the most conclusive way, the numerical results obtained.

At Hattervig, a young man, the first sufferer, assured me that the eruption in his case appeared *ten* days after his arrival in an infected village. This was a solitary instance, and the patient afterwards contradicted the false information which he had given me. At Fuglefjord, the daughter of my host was cured of the disease from which the other nine members of the family had not as yet suffered. I asked on what day the rash had appeared on the girl, and taking an almanac, made a stroke under the *fourteenth* day, telling the family that this was the date when the disease was likely to appear among them. I learnt, on my return, that this prediction had been but too accurately verified by the eruption appearing on all the nine.

The Great Dimon is a very little islet, inhabited only by a family of eighteen persons. A boat, manned by some of the men, visited Tveras, where the epidemic raged, and where the boat remained only a few hours. Ten days afterwards the whole boat's crew were in their ordinary health—on the fourteenth day all had the eruption of measles, and fourteen days afterwards the rash, with the same regularity, appeared on all the remaining members of the family.

At Skaalevig, the only one of the islands where the houses are isolated, standing at considerable distances apart, the contagion spread less rapidly, but in following its successive outbreaks even here, the law which I have laid down was found to be equally observed.

Hence I believe it may be regarded as a constant law, that the *contagion* of measles, once introduced into the system, does not at first manifest its presence by any sign, and that after a premonitory stage of indeterminate duration, the eruption makes its appearance on the thirteenth or fourteenth day after infection. Undoubtedly peculiarities in the constitution, regimen, &c., of individuals, may hasten or retard the appearance of the exanthema, but these influences are far less potent than, *a priori*, might be supposed. The greatest variation on one side or other of the period specified, is at the utmost twenty-four hours. After

the striking coincidence of the observations which I have made in fifty-two villages, ought not exceptions (which I have never met with), if they exist, to be accounted for by the inexactitude of the observer? Whenever I have been inclined to contest evidence, which at first seemed to shake my conviction, I have finally found, that far from invalidating, it served to confirm the rule laid down. According to observations made at Copenhagen, the incubation of variola extends through fourteen days. The analogy between these two eruptive diseases, seems to me to attach considerable interest to these observations.

A circumstance which causes some uncertainty in prosecuting such researches is the indeterminate duration of the premonitory catarrh. Some of my patients suffered six or eight days before the eruption from cough, slight ophthalmia, and febrile disturbance; in others, these symptoms were not noticed till four or six days before the rash; in the majority they were not observed till within a period of from two to four days immediately preceding the exanthema. Exact numerical results can only be obtained by comparing the date of contact with the sick and that of the appearance of the measles.

If we admit as a principle, that the incubation occupies from thirteen to fourteen days, and if, on the other hand, instances, both numerous and well authenticated, prove that there generally elapse exactly thirteen or fourteen days between the first symptoms of eruption on an individual, and its subsequent appearance on others, to whom he communicates it, is it not evident that the contagion is effected during the *eruptive stage?* We may at least conclude that measles in the latent stage is not contagious. Is it so during the premonitory stage? It is difficult to decide. Although I am acquainted with no facts which demonstrate the possibility of transmission during the simply catarrhal stage, I am not prepared to establish the contrary.

It is generally believed that measles is peculiarly contagious during the desquamative stage. On what foundation does this opinion rest? I cannot tell: for my own part I have never seen a case to convince me that contagion took place at the desquamative period. The transmission, if the period above assigned to the incubation be regarded as constant, is almost always, if not in every case, effected at the period when the spots make their appearance. Never, in the course of the events which I witnessed, was an individual attacked with measles more than fourteen days after the disappearance of the exanthema from the individual who might have infected him. In a word, measles is highly contagious at the outset, and during the course of the eruptive stage; it is, on the other hand, doubtful if it be contagious at all in the catarrhal or desquamative stages.

As for the second attacks of measles, it is remarkable that all the old people of the Færöe Islands, who had suffered in the epidemic of 1781 (and of these there were many), escaped in 1846; I could cite a hundred instances from my own observation. This fact is the more conclusive, since age (even very advanced) in no measure diminishes the susceptibility. As far as I know, very old persons, who had not been exposed to the earlier epidemic, all suffered from the recent one, while certain young people living in the midst of the disease were spared.

The degree of intensity with which we may suppose the contagious principle of rubeola endowed, is not well defined. The following facts will, perhaps, throw a little light on this subject. On the 2d June a crew embarked at Klaksvig, to fetch articles of merchandise from Funding. They were prevented from returning till they had helped to unlade a vessel on board of which were men just recovering from measles, which also prevailed extensively in the port. They entered no house in Funding but the counting-house. On returning, the men of Klaksvig threw away the coverings of their merchandise, washed themselves carefully, changed their clothes, &c. None of them caught the disease till the 3d July, when it first made its appearance in Klaksvig. But five inhabitants of Nordre-Gjov, who had accompanied them to Funding, and on their return had not used the same precautions, although exposed to the same influences, suffered on the fourteenth day after their exposure to the contagion.

When I arrived at Klaksvig, such was the terror of the islanders, that nobody

would come near me, although it is a received opinion among them that doctors do not convey contagious miasmata. Three weeks before Pentecost the surgeon of the country called to Klaksvig to exercise his skill, had been obliged to pass a night in the village; measles appeared fourteen days afterwards in the house where he had lodged. The development of the disease could be attributed to no other cause, for not a single inhabitant of the house or island had had communication with an infected locality, and no other stranger had entered the village. At Midtvaag measles was introduced by a midwife, who had attended the sick in a neighbouring island. The girl who washed the midwife's clothes was the first to suffer.

These instances tend to show that measles possesses a contagious power more virulent than is usually assigned to it.

Whatever may be the value of simple prophylactic means, I consider it unquestionable that isolation is the most effectual means of arresting the development of the disease. Thus in several villages by interrupting the communications with neighbours, families were saved. I may cite as instances 2 families at Saxen; 10 at Midtvaag; 10 at Sandevaag; half the village at Thorsvig. Different localities were preserved by a rigorous "cordon sanitaire"—their respective populations varying from 26 to 240. About 1500 islanders may, upon the whole, be said to owe their escape to these sanitary cordons or quarantine regulations.

In 6000 cases, of which I myself saw and treated 1000, there did not occur a case tending to prove the existence of "morbillose miasmata." Everywhere the disease passed from man to man—from village to village—contagion mediate or direct was evident. The two physicians of the Færöe Islands regarded the measles as of miasmatic nature, and as susceptible of transmission through the atmospheric air. Hence, at the outset of the disease, no precaution was taken by the authorities to interrupt communications and prevent the extension of the disease. Experience had, in 1781, shown what services quarantine could render —recourse was not had to it till the time for good was past. It is to be hoped that the experience so dearly bought in 1846 will have its fruits. The contagious nature of measles is so clearly proved that, in my opinion, the physician would violate his duty who should act as if it were not so.—(Translated from *Archives Générales de Médecine*, April 1851. P. 451. Originally published in *Archiv. f. Physiol. Heilkunde*, t. ii.)

SURGERY.

REMOVAL OF THE LOWER JAW ON THE RIGHT SIDE FOR CYSTIC DISEASE; RECOVERY.

The "Lancet" of Saturday, 17th May, contains, under the above-mentioned title, a case which seems to require serious notice. The patient was a "professor of music," aged 48, who, about eight years ago, after suffering from toothach, and having two grinders removed, observed a swelling in the gum where the teeth had been extracted. This he opened with a penknife, when "some glairy fluid escaped, and the parts seemed after a little while to be cicatrised. But the tumour did not completely subside; it filled again, and the patient fell into the habit of puncturing and evacuating the contents of the swelling whenever it became distended with fluid." By-and-by, "the body of the lower jaw on the right side, about midway between the joint and the symphysis, began to expand outwardly; this swelling increased very slowly, and occasioned no pain; the protruded part felt hard and smooth, but on pressure a slight crackling could be heard. Within the last twelve months, the growth has been more rapid; and the patient having consulted Mr Fergusson about five months ago, the latter made an opening into the tumour within the mouth, where it was slightly protruding, and this measure gave exit to some glairy fluid. The part continued to discharge the same kind of liquid for some time, when the patient applied to

another surgeon, who inserted potassa fusa, and kept open the part with lint tents.

"This proceeding occasioned severe pain, and entirely changed the character of the discharge, which became fœtid, mixed with sloughs, and increased in quantity. Besides the two molar teeth already mentioned, the patient lost another molar and a bicuspid on the same side: the first fell out spontaneously, and the second was extracted. It is worthy of note that neither of these had any fangs left.

"On examination, the tumour was found to be the size of a large orange, forming a solid mass, seemingly attached or forming part of the lower jaw on the right side, and extending from the angle of the maxilla to the symphysis. At the latter spot the bone appears to be gradually expanding into the tumour, which latter is hard, smooth, and painless to the touch. The skin over it is not in the slightest degree altered, and the swelling does not project much within the mouth; an opening exists, however, in the gum, through which the probe passes very easily into the cavity of the tumour. This bony cyst is filled with fœtid matter, which escapes when the patient inclines his head forwards. The gum is rather spongy, but the general health good.

"Mr Fergusson ordered a silver double-grooved plate to be adapted to the interior of the mouth, with the view of receiving the upper and lower molar teeth on the sound side, by which means those of the affected side were kept about an inch apart. It was evident that no remedial means could be of avail, except the complete removal of the diseased portion of the jaw; and as the tumour had plainly involved the greater part of the latter, Mr Fergusson resolved to remove the right half of the lower maxilla from the articular process to the symphysis.

"On the 22d of March the patient was brought into the theatre, and rendered insensible by chloroform. Mr Fergusson began by extracting a lower incisor tooth and the canine on the right side; he then passed the point of a bistoury into the mouth, about half an inch below its angle, without dividing the red part of the lip. The knife was then made to run along the lower margin of the jaw to about the middle of the tumour, and the soft parts having been detached from the bone, Mr Fergusson introduced the common straight saw, with which the bone was very cleanly divided at the symphysis. The next step consisted in carrying the external incision over the tumour, nearly up to the articulation, and dissecting up the flap towards the eye and nose. Mr Fergusson then rapidly separated the muscles attached to the inner surface of the jaw, and the latter having been disarticulated, the whole mass was removed." After the operation,—"Mr Fergusson then proceeded to divide the tumour longitudinally, and the latter proved to be of the cystic kind, presenting a cavity which would have lodged a small orange. It was filled with a fœtid secretion, the shell of bone forming the walls of the cyst being generally about a quarter of an inch thick. It was now evident that the cyst had been formed by a dilatation of the walls of the jaw, and was lined by the same kind of membrane which is seen in chronic abscess. This membrane had now passed into a state of gangrene, probably from the caustic solution which had been used." "In examining the patient just before he left the hospital, we were struck by the small amount of deformity produced by the ablation of so important a portion of the face; and as Mr Fergusson had not divided the red portion of the lip, the continuity and symmetry of the mouth was uninterrupted, and the lines of incision hardly distinguishable. This result is the more satisfactory, as it may be presumed that fibrinous matter will in time be thrown out, and give the parts a useful amount of firmness. The small amount of hemorrhage during the operation will go far to prove that Mott, Cusack, Walter, Graefe, and Gensoul, were rather too timid when they tied the carotid artery previous to the operation. Lisfranc very wisely dispensed with this preliminary step."

Now, in regard to this case, we think it our duty to state that it was performed in direct opposition to the established rules of surgical practice, which require that all cystic formations, whether of the upper or lower jaw, shall be afforded an

opportunity of contracting and healing through a *free* opening of the cavity, not merely by incision, but *by removing a portion of the osseous or membranous parietes*, before any other more serious measure be employed for their remedy. The patient's punctures with his penknife, though they might and did palliate the evil, could not be expected to prove effectual, and the opening made by Mr Fergusson was obviously not sufficient, since it appears that application for relief was soon afterwards made to another practitioner, who inserted potassa fusa, and *kept open the part with lint tents*. We trust that few of our practical readers need to be told that the cavities of abscesses, whether of the soft or hard parts, essentially require for contraction that the matter secreted by them should have free vent, and that the old plan of introducing tents into the orifice, so as nominally to open, but in truth to cork them tightly, was the most certain means of increasing the evil, instead of lessening it. From what we have seen of similar cases, we entertain little doubt that in this one, if the reflected mucous membrane between the cheek and gum had been divided, so as to expose the expanded bone, and permit a portion of it, the size of a shilling or half-crown, to be removed, the patient would have speedily recovered without any further trouble. At all events, until this step had been taken in vain, we beg to repeat that excision of the jaw was not warranted by the present established rules of surgery.

In the second place, we beg to remark that the disease having originated in the base of the jaw, not having extended into the ramus, and not being malignant, there was no warrant whatever for disarticulation, even if it had been found necessary to remove the expanded portion of bone.

As to the advantage of leaving undivided the margin of the lip, we beg to remind our friends in London that if there be any merit due for this mode of proceeding, it belongs entirely to Mr Liston, who adopted, practised, and enforced it as of great consequence in lessening the amount of deformity. For our own part, we are of opinion that, as the incision, if directed obliquely outwards and downwards from the *angle* of the mouth, can hardly be detected after recovery, while the great additional space obtained by making the mouth continuous with the incision wonderfully facilitates the procedure of dividing or disarticulating the bone, the advantage of allowing the lip to remain entire is somewhat questionable, unless when the tumour is of a large size. Thus, in disarticulating on both sides the entire jaw for an enormous growth, we have found a simple incision under the chin from ear to ear quite sufficient for the purpose.

But of all the statements connected with this case, the one which seems to us the most extraordinary, is that regarding ligature of the carotid artery as a precautionary measure. To say nothing of the other gentlemen whose names are mentioned so disparagingly, surely that of Mr Cusack might have been spared, not only in consideration of his not tying the artery in the numerous and successful cases which were chiefly concerned in establishing the operation in this country, but in gratitude for his clear and convincing demonstration, that the internal maxillary and temporal arteries might be avoided with certainty in effecting disarticulation, by *opening the joint on its fore part*, so as to remove all excuse for ligature of the carotid.

NEW MODE OF TREATING FISTULA IN ANO. BY MR MARSHALL, ASSISTANT-SURGEON OF UNIVERSITY COLLEGE HOSPITAL, LONDON.

We have heard that there is a newspaper advertiser in London, who undertakes to cure fistula "without cutting," and fulfils his contract by conveying a wire through the sinus and anus, so as to include the septum between them, and divide it through ulceration caused by gradual twisting. The prolonged confinement and discomfort, to say nothing of the pain and pecuniary exaction attendant upon this tedious substitute for an instantaneous incision and speedy recovery without any sort of annoyance, have been to us rather agreeable subjects of consideration, as a just and fitting punishment for the folly of trusting unprincipled impostors. The reader, therefore, may imagine our sur-

prise and concern, in finding the wire gravely recommended by a regular member of the profession, with the agreeable addition of being used *red hot ! !*

"The patient having been put under the influence of chloroform, Mr Marshall introduced one end of the platinum wire into the fistulous tract, and made it reappear at the anus; the two ends were then connected with the poles (of a galvanic battery), the circuit completed, the wire became red hot, and was gently brought downwards, dividing all the interposed tissues, and cauterizing them at the same time so effectually as to prevent any amount of hemorrhage. The only dressing used was a piece of lint dipped in cold water applied externally."

In entering our decided protest against this most unprofitable, though ingenious proposal, and also the employment of a red hot wire for removing hemorrhoidal tumours, we venture to hope that we shall not be charged with making a "personal attack" upon the author, especially as we have reason to believe that he is a gentleman of great merit and promise in his profession. While freely criticising published facts and opinions, as it is our duty to do, we cannot regard the groundless charge of personality reiterated from week to week, as any reply to the statements bearing upon medical science and practice, for which we are responsible.

Part Fifth.

MEDICAL NEWS.

RESOLUTIONS OF THE ROYAL COLLEGE OF PHYSICIANS OF EDINBURGH, AND OF THE ROYAL COLLEGE OF SURGEONS OF EDINBURGH, REGARDING PRACTITIONERS OF HOMŒOPATHY.

At Edinburgh, and within the College Hall there, the 9th day of May 1851, an Extraordinary Meeting of the Royal College was held, pursuant to a Resolution agreed to at the last Quarterly Meeting, and of which Extraordinary Meeting due notice was given. The President in the chair. The following Resolutions were moved, seconded, and unanimously agreed to:—

1. That the Royal College of Physicians of Edinburgh did, several years ago, publicly express its opinion of homœopathy and homœopathic practitioners, by peremptorily declining to admit into its body a candidate for its Fellowship who belonged to that denomination; and, consequently, that no Fellow of the College can possibly be ignorant of the light in which all those who practise homœopathy are regarded by the College.

2. The College regrets that, notwithstanding this decided expression of its opinion, more than one of its Fellows, after being admitted in a different character, have endangered the reputation of the College by becoming homœopathic practitioners; and the College expresses an earnest hope that these Fellows, seeing they have thus virtually separated themselves from the College, will spontaneously sever their further connection with an institution which repudiates them, and from which they can derive, as merely nominal Fellows, nothing else than a false position and a spurious credit.

3. The College feels the more bound thus to express its opinion, seeing that those Fellows who have become homœopathists, and any other medical practitioners who follow homœopathy, must necessarily be aliens to the other Fellows, and to the profession at large; inasmuch as no Fellow of this College, or any other physician, can, by any possibility, without derogating from his own honour, and from the honour of the profession, meet practitioners of homœopathy in

consultation, or co-operate with them in the other common duties of professional life.

4. That although the College has not thought it expedient hitherto to take any active steps for disclaiming those Fellows who have become Homœopathic Practitioners subsequently to their admission to the College, nevertheless, since it has the power of dealing summarily with those who act in a manner so unbecoming the character of a Physician, it reserves its right to exercise that power when it shall be so advised.

Signed in Name, and by Authority, of the College,

J. Y. SIMPSON, *President.*

At a meeting of the Royal College of Surgeons of Edinburgh, held on the 16th instant, the following Resolutions were moved by the President, and unanimously adopted:—

1. The College having considered a series of resolutions transmitted by the Royal College of Physicians, in regard to homœopathy, feel called upon to express their opinion that the system so designated being entirely inconsistent with the principles professed by candidates for the diploma of the College of Surgeons, any Fellow or Licentiate who practises it, or countenances others in doing so, by meeting them in consultation, will justly incur the disapprobation of the College.

2. That a copy of the above resolution be transmitted to the Royal College of Physicians.

Signed in Name, and by Authority, of the College,

JAMES SYME, *President.*

VARIETIES.

AWKWARD POSITION OF AN IRISH REVIEWER.—With reference to a case lately related in this Journal, of an aneurism affecting the radial artery, which was remedied by pressure applied to the wrist, the "Dublin Quarterly Journal of Medical Science" for May, is carried, by its favourite hobby, into the following extravagance:—"Now, Professor Syme, we appeal to you, as a surgeon and a gentleman, is this fair dealing with the salient points of the question at issue? What! 'The radial artery lies directly under the skin, rests upon the bone, and has no associate disposed to resent the effect of compression.' No tyro in anatomy can be ignorant that the radial has *two* venæ comites, and as for their disposition to resent injury, we are not in a position to answer whether it be greater or less than that of any other satellite vein under similar circumstances."

On the part of the gentleman to whom this affecting appeal is addressed, we beg to reply, after the established practice of our countrymen, by asking the advocate of pressure, if he really thinks that compression, or even complete obliteration of the venæ comites of the radial artery at the wrist, could produce the slightest appreciable effect on the circulation of the limb? or that these minute vessels would resent injuries inflicted upon them in the same fearful fashion as the great venous trunk of the lower extremity?

Should our brother of Dublin answer this question in the affirmative, we shall probably be held excused for declining any further discussion with an opponent entertaining such very peculiar opinions; and if he repudiates the folly that would thus be implied, we shall then desire him to explain how he has ventured to insult his readers by *pretending* to believe that there was any parallel between the two cases. In the meantime, we leave him to choose whichever horn of this dilemma may promise the more comfortable repose.

PROFESSIONAL ETHICS OF SIR B. BRODIE AND SIR P. CRAMPTON.—In the last Number of this Journal, there are two letters which seem to claim attention, irrespectively of the circumstances that led to their production. One of these is from Sir B. Brodie, and the other from Sir P. Crampton,—both being in reply

to a question regarding the rules of professional conduct, which we presume was put them as representing the surgical departments of England and Ireland.

The question thus submitted was simply, Whether a family attendant, who had ordinary charge of the case, or a surgeon who was called in to perform an operation found requisite for its remedy, was entitled to publish the details of the operation? In Scotland, we are happy to say, there is not, and never has been, any difference of opinion upon this subject; since it seems to us, that nothing could be more opposed to the interests of science, more calculated to produce unpleasant feelings, and more opposed to justice, than establishing the principle, that any one except the responsible operator, should have the right to relate for public information the particulars of an operation, in regard to its performance, treatment, and result. But if the two Baronets may be deemed faithful exponents of professional custom in their respective countries, it would appear that in England and Ireland a different view of the matter is taken, and that the ordinary attendant may there, without being held guilty of any impropriety, publish the account of an operation which he never performed, even without intimating his intention of so doing to the operator. As "every country has its own fashion," we do not presume to offer any remark upon the inexpediency and injustice of this procedure, or, to use the expression of Sir P. Crampton, treating an operation which may involve the life of the patient, together with the character of the surgeon, merely as an "incident" that in nowise transfers the responsibility from the ordinary attendant. All that we think our duty requires from us, is to protest against the introduction of such principles into Scotland, where, we venture to say, the duty of one medical man towards another, is at least as well known and practised, as in any other part of her Majesty's dominions.

Pure Surgery.—A "professor" of surgery, who practises as "family attendant," has lately expressed great apprehension of the danger attending "Pure Surgery," and been cordially responded to by the editor of the "London Journal of Medicine." "The day will probably come," says this sapient gentleman, "when a pure surgeon will be thought of as an almost fabulous monster of antiquity." Now, if "pure surgery" be, as we have always understood it to imply, simply abstaining from general practice, and attending chiefly to surgical cases, we should wish to be informed how a "pure surgeon" is to be regarded with more dread than a "pure physician."

Clairvoyance in the Seventeenth Century.—"*April 11th*, 1681.—Honoured Sir,—I did receive your last letter, dated the 9th of these month, with much grief, haveing an account of your painfull feaver: I pray God it will not vex your body too much; and if by chance it should vex you longer, there is here a man that can cure it with simpathetical power; if you please to send me down the pearinghs of the nailes of both your hands and your foots, and three locks of hair of the top of your crown, I hope, with the grace of God, it will cure you. As for the compositions of them two masters, in my jugement, though weak, I like better Baptist's works than Pedro's, because Baptist work masterly, as you shall perceive betwixt their bases. All Baptist's bases are singable where many of Pedro's are not so. Herewith my humble respects, remaine, Honoured Sir, your most faithfull Servt., Cesare Morelli."—*Diary of Sam. Pepys, Third Edition, by Lord Braybrooke*, 1851, vol. v. p. 308. [He, (Cesare Morelli) appears to have been a music-master. Many of his compositions are preserved in the Pepysian Library.]

Man and the Lower Animals.—The "Gazette Médicale," after noticing an operation for cataract recently performed, with the assistance of chloroform, on a bear in the Zoological Gardens of London, introduces the following commentary:—"To fill the gap which seems to separate man from the beasts is a problem of deep zoological and philosophical interest. Metaphysicians and naturalists have been working at it for ages, and in different ways, without approaching its solution. It is for pathology to resolve the problem. We have long known that the dog can communicate rabies to man, and he can probably

give it back to the dog. Of late we have indubitably attained the faculty, long denied to us, of catching glanders from the horse, and of returning it to the donor. Again, we have to thank M. Auzias Turenne for teaching us how to inoculate monkeys (poor beasts) with syphilis. The chloroforming of the bear at London is another interesting step to add to those above-mentioned. We content ourselves with collecting observations. They are the basis of science on which others may rear their doctrines.

ALLEGED TOLERATION OF HOMŒOPATHY BY THE MEDICAL FACULTY OF THE EDINBURGH UNIVERSITY.—As much blame has been imputed to the professors of the University for their alleged apathy in regard to the homœopathic perversion of a colleague, we think it right to state that the medical faculty have done all that was in their power. Several years ago, when it first became known that Dr Henderson was treating acute inflammatory affections by the administration of globules, in the clinical wards of the Royal Infirmary, the medical faculty having in vain remonstrated with him, intimated to the managers of the hospital that they did not wish he should any longer act as one of the professors of clinical medicine, and he was dismissed accordingly. As to the professorship of general pathology, the medical faculty have no power in the way of appointment or dismissal; but we have no doubt that if it ever came to their knowledge, that Dr Henderson was making his lectures from this chair the vehicle of homœopathic doctrines, they would petition the patrons for his removal. The testimony of students who have attended this course assures us there has hitherto been no ground for any such complaint; and so long as this is the case, we do not see how the medical faculty can take cognizance of the principles upon which their colleague chooses to conduct his private practice.

PUBLICATIONS RECEIVED.

Zeitschrift der k. k. Gesellschaft der Aerzte zu Wien. Jänner und Februar. 1stes und 2tes Heft. 1851.

Letters to a Candid Inquirer on Animal Magnetism. By William Gregory, M.D., &c. London: Taylor, Walton, and Maberly. 1851.

An Inaugural Essay on Zoo-Adynamia. By Geo. J. Ziegler, M.D. Philadelphia. 1850.

On the Structure of the Membrana Tympani. By Joseph Toynbee, F.R.S., &c. London. 1851. (From Philosophical Transactions.)

Journal für Chirurgie und Augenheilkunde. Herausgegeben von Walther und Ammon. Berlin. Heft. 2, 3, and 4, mit Supplement und Schlussheft.

Practical Remarks on the Treatment of Aneurism by Compression. By J. Tufnell, M.R.I.A. Dublin: Fannin & Co. 1851.

The Prospective Progress of Medicine in America: a Valedictory Address. By Horace Green, M.D. New York. 1851.

Introductory Address to the Students and Trustees of the New York Medical College. By Horace Green, A.M., M.D., &c. New York. 1850.

An Apology for the Microscope: an Introductory Lecture. By Robert D. Lyons, M.B., &c. Dublin: Fannin & Co. 1851.

On the Mode of Origin and Propagation of the Epidemic Cholera in Huddersfield. By John Taylor, M.D., Lond. London, 1851. (Reprint from "Medical Times.")

On the Reduction of Strangulated Hernia in Mass. By George C. Blackman, M.D. New York. 1851.

On the Prevention and Cure of Spinal Curvatures. By Mrs Godfrey [of Liverpool]. London. 1851.

Letter addressed to the Governors of the Lincoln Lunatic Asylum. By W. M. Pierce. Lincoln. 1851.

Catalogue of Articles transmitted by the Central Committee in British Guiana to the Exhibition of Works of Industry of all Nations. 1851.

What is Mesmerism? an Attempt to Explain its Phenomena. By Alex. Wood, M.D., &c. Edinburgh. 1851. (Reprinted from the "Monthly Journal of Medical Science.")

ERRATUM.—In Dr Keith's paper, page 436, line 10, *for* "manifestation" *read* "manipulation."

INDEX.

Adams, Dr, case of spontaneous evolution, 126, 488.
Agues, incubation of, 185.
Air found in the heart, in a case of sudden death after delivery, by Mr Berry, 189.
Albuminuria during pregnancy, by M. Forget, 86.
Alison, Professor, clinical lectures by, 71; observations on electro-biology, 485.
Aloin, Messrs Smith on preparation of, 127.
Alveolar process, Mr Syme on disease of, 174.
Ammonia, Dr Jones on its oxidation in the human body, 480.
Amputation at ankle, Mr Syme on, 172; case of, by Professor Heyfelder, 188.
Anemic murmurs and their diagnosis, by Dr H. M. Hughes, 82.
Aneurism, Professor Syme's case of popliteal, 16; of radial artery, Mr Syme on, 369; M. Velpeau's case of false, 376.
Ankle-joint, Mr Syme on amputation at, 172; Professor Heyfelder on, 188.
Anthelmintic properties of the mulberry, 87.
Arnauld, and other works on logic, reviewed, 137.
Arneth, Dr F. H., on the mode of propagation of puerperal fever at Vienna, 505; practical midwifery by, reviewed, 544.
Artery, humeral, Mr Syme's case of obstructed, 290.
Assurance, life, companies, 495.
Atropine, prepared by means of chloroform, 89.
Augend, M., on antiseptic properties of chloroform, 87.
Axilla, Mr Syme on hæmatocele in, 173.

Balfour's statistics of cholera, reviewed, 256; Professor, Manual of Botany, reviewed, 541.
Balman, Mr, on starch granules observed in urine, 84.
Bartholomew's Hospital, use of chloroform in, 192.
Beatty, Dr, on use of chloroform in midwifery, 190.
Begbie, Dr, on fatty degeneration of the heart, 201, 298.
Bell, Mr Benjamin (see Eye Infirmary) 385, 482; Dr Charles, on the treatment of erysipelas by the muriated tincture of iron, 501; Mr G. Hamilton, on ditto, 497.
Bennett, Professor, on leucocythemia, 17, 197, 312; lecture on the microscope as a means of diagnosis, 149; report of cases of nervous disease, clinical lecture, 269; varieties of cerebral and spinal softening, clinical lecture, 362; on electro-biology, 484.
Berry, Mr, case of sudden death after delivery, in which air was found in the heart, 189.
Bigelow, Dr, on treatment of inguinal hernia by injection, 186.
Blunt-hook, Dr Graham Weir's, 488.
Board of Health, reports on cholera by, reviewed, 256.
Books received, 96, 200, 304, 396, 496, 598.
Braid, Mr J., on electro-biology, 511.
Brain, Mr W. Traill's case of tumour of, 220; Dr Kirkwood's case of tumour of, 223.
Bramwell, Dr J. P., on use of cod-liver oil in pulmonary consumption, 131.
Bright's disease of the kidney, Dr Christison's lecture on, 551.
Bronchitis, Dr W. T. Gairdner on the pathology of, 440.
Bruecke on muscular fibres connected with the mucous membrane of stomach and intestines, 372.
Bushnan on cholera, reviewed, 256.
Byl, Dr Van der, communication regarding the discoveries of Bruecke, 372.

Calculi, Dr Omond's case of renal, 483.
Calomel, Dr Oettinger on action of, 88.
Cancer of the lung, Dr Macdonnell's case of, 177.
Cancer of the womb complicating labour, Dr Oldham's cases of, 377.

Cancrum oris produced by mercury, 481.
Carotid, wound and ligature of internal, by Dr W. Keith, 435.
Caustic, how to remove stains of lunar, 381.
Cazenave, M., on the use of muriate of lime in certain skin diseases, 191; on treatment of painful cutaneous affections, 381.
Chemistry, Dr Gregory on the recent progress of organic, 573.
Chlorine, Dr Weinsch on its disinfecting properties, 482.
Chloroform, M. Augend on antiseptic properties of, 87; used in preparing atropine, 89; Dr Beatty on use of, in midwifery, 190; used in Bartholomew's Hospital, 192; Dr Gross on use of, 196; and ether, Dr Reese's hospital experience in the use of, 194.
Cholera in Jamaica, 93; on pericardial friction in, 186; works on, reviewed, 256.
Christison, Professor, remarks on Dr Struthers's case of hydrophobia, 90; on Dr Bennett's paper on leucocythemia, 198; clinical lecture on Bright's disease of the kidney, 551.
Circulation, Professor Goodsir on discoveries in the anatomy of, 292.
Clinical Medicine, lecture by Professor Alison on erysipelas and glanders, 71; Professor Bennett's lecture on the microscope as a means of diagnosis, 149,—on nervous diseases, 269, 362; Professor Christison on Bright's disease, 551.
Clinical Midwifery, Professor Simpson on the diagnosis of the diseases of the uterus, &c., 155.
Clinical Surgery, Professor Syme's lecture introductory to a course of, 78; lectures on, 172, 288, 368.
Cod-liver oil, Dr Bramwell on the use of, in consumption, 131.
Colton, Mr, on hæmaturia caused by the inhalation of vapour of turpentine, 85.
Consumption, Dr Bramwell on the use of cod-liver oil in, 131.
Contagion of puerperal fever illustrated at Vienna, 505; of measles in the Færöe islands, 589.
Contagious propagation of puerperal fever in the Lying-in Hospital of Vienna, 505.
Convulsions, Dr Keiller on puerperal, 489.
Cox on cholera, reviewed, 256.
Cranial bones, Dr Howie's case of deficiency of, 531.
Crotchet, Dr Graham Weir's, 488.
Cruikshank, Dr James F., case of stricture of the urethra successfully treated by division, 533.
Cumming, Dr G. V., case of obstruction of the bowels caused by swallowing a leaden bullet, 327.
Cyanosis, cases of, by Dr Strehler, 185.
Cystic disease of lower jaw, and removal of its right half at articulation, 592.

Dalrymple's pathology of the human eye, reviewed, 70.
Diagnosis by means of the microscope, Professor Bennett on, 149; of uterine diseases, Professor Simpson on, 155.
Dick, Professor, on hydrophobia, 90.
Dietl on bloodletting in pneumonia, reviewed, 61.
Druitt's Surgeon's Vade-Mecum, reviewed, 361.
Dublin Pharmacopœia, reviewed, 50.
Dugas on cancrum oris caused by the use of mercury, 481.
Dunsmure, Dr, on treatment of stricture of the urethra, 383.

Eichelberg, Dr, proposal for preventing laceration of the perineum during labour, 378.
Elbow-joint, Mr Syme on excision of, 369.
Electro-biology, Dr Wood on experiments in, 407, 483; Dr Bennett, Dr Macdonald, Dr Alison, Dr Gairdner, Dr Simpson, and Mr Syme's observations on, 484-487; Mr J. Braid on, 511.
Erysipelas, Professor Alison's clinical lecture on cases of, 71; Mr G. Hamilton Bell and Dr Charles Bell on the treatment of, by the muriated tincture of iron, 497.
Ether and chloroform, Dr Reese on the use of, 194.
Evolution, Dr Adams's case of spontaneous, 126, 488.
Extra-uterine pregnancy, Dr Johnson's case of, 85.
Exchange list, 96.
Eye Infirmary, medical report of, 385, 482.

Færöe islands, Dr Panum, of Copenhagen, on measles in the, 589.
Fatty degeneration of the heart, Dr Begbie on, 201.
Fergusson, Mr, removal of lower jaw on the right side for cystic disease, by, 592.
Fistula of the stomach, Dr Robertson's case of, 1, 91; in ano, Mr Syme on, 175; new mode of treating, practised by Mr Marshall, 594.
Foot-prints solidified for medico-legal purposes, by M. Hugoulin, 381.
Forbes's lecture on happiness in relation to work and knowledge, reviewed, 145.
Forget, M., on albuminuria during pregnancy, 86.

Gairdner, Dr W. T., on hydrophobia, 91; Dr John, on ditto, 91; Dr W. T., on terminations of pericarditis, 103, 298; remarks on leucocythemia, 198; on the pathological states of the lung connected with bronchitis and bronchial obstruction, 440; on melanosis, 483; on electro-biology, 485.
Glanders in the human subject, Professor Alison on, 74.
Glover, Dr R. M., on the physiological properties of picrotoxin, 305.
Goodsir, Professor, remarks on leucocythemia, 198; lecture on recent discoveries in the anatomy of the circulating system, 292.

Gray, Dr J., case of complicated parturition, 7, 92.
Gregory, Professor, lecture on the recent progress of organic chemistry, 573.
Guitard and Joly, MM., case of nosencephalic fœtus adherent to the placenta, 379.

Hæmatocele of axilla, Mr Syme's case of, 178.
Hæmaturia caused by vapour of turpentine, 85.
Hairion, M., on the use of tannin in certain affections of the eyes, 192.
Hamilton, Dr R. (see Eye Infirmary), 385, 482.
Hare-lip, Professor Syme on, 368.
Heart, Dr Begbie on fatty degeneration of, 201.
Hernia, case of strangulated, by Mr John Traill, 101; inguinal, treated by injection, 186.
Heyfelder, Professor, on ankle-joint amputation, 188.
Homœopathy repudiated by the Colleges of Physicians and Surgeons of Edinburgh, 595.
Hooper's Physician's Vade-Mecum, reviewed, 361.
Howie, Dr James H., case of deficiency of cranial bones, 531.
Hughes, Dr H. M., on anemic murmurs and their diagnosis, 82.
Hugoulin on solidification of foot-prints for medico-legal purposes, 381.
Hunter's Anatomy of the Human Gravid Uterus, reviewed, 548.
Humeral artery, Mr Syme's case of obstructed, 290.
Hydrocele of the neck, by Professor Mutter, 187; Mr Syme on, 290.
Hydrophobia, Dr James Struthers's case of, 9, 90; Dr J. A. Sidey's case of, 89; Dr Redfern's case of, 97.

Intermittent fever, M. Verdier on the incubation of, 185.

Jamaica, ravages of cholera in, 93.
Jaw, necrosis of lower, Mr Syme on, 288; removal of lower, by Mr Fergusson, 592.
Johnson, Dr, case of extra-uterine pregnancy, 85.
Jones on oxidation of ammonia in the human body, 480.

Keiller, Dr, on puerperal convulsions, 489.
Keith, Dr W., case of wound and ligature of the internal carotid artery, 435.
Dr Kerlé on tapeworm, 184.
Kidney, Dr Omond's case of calculi in, 483; Professor Christison on Bright's disease of, 551.
Kirkwood, Dr, case of cerebral tumour, 223.

Laceration of the uterus and internal hemorrhage, Mr Roberton's case of, 378; of the perineum during labour, how prevented by Dr Eichelberg, 378; of the perineum and cervix uteri in natural labour, remarks by Dr Simpson on, 488.
Landerer, Professor, on the anthelmintic properties of the mulberry, 87.
Lassaigne, M., experiments on pancreatic fluid, 479.
Lectures on clinical medicine, 71, 149, 269, 362, 551; clinical surgery, 78, 172, 288, 368; clinical midwifery, 155; on the anatomy of the circulation, 292; on recent discoveries in organic chemistry, 573.
Leucocythemia, Professor Bennett on, 17, 197, 312; Dr Keiller on, 197; Dr W. T. Gairdner on, 198; Professor Goodsir on, 198; Professor Christison on, 198.
Life assurance companies and the medical profession, 495.
Logic, works on, reviewed, 137.
London Pharmacopœia, 1851, reviewed, 465.
Lung, Dr Macdonnell's case of cancer of, 177.

Macdonald, Dr, on electro-biology, 485.
Macdonnell's, Dr, case of cancer of the lung, 177.
Mackenzie, Dr R., case of stricture of the urethra, in which the operation of external division was followed by a fatal result, 213, 299; on treatment of stricture of the urethra, 384.
Maclise's Surgical Anatomy, reviewed, 70.
Marshall, Mr, treatment of fistula in ano practised by, 594.
Maxilla, inferior, Professor Mott on removal of, 376; Mr Fergusson's case of removal of the right half of the, 592; superior, excision of, 377.
M'Dougall's translation of Lallemand on spermatorrhœa, reviewed, 478.
Measles in the Færöe islands, 589.
Medical Society, Royal, of Edinburgh, 300.
Medicine, clinical lectures on, by Dr Alison, 71; by Dr Bennett, 149, 269, 362; by Dr Christison, 541; stamps, Professor Simpson on ancient Roman, 39, 235, 338.
Medico-Chirurgical Society of Edinburgh, transactions of the, 89, 197, 298, 383, 483.
Melanosis, Dr W. T. Gairdner's remarks on, 483.
Mercury, Dr van Hasselt on the action of metallic, 379; on its influence in producing cancrum oris, by Dr Dugas, 481.
Mesmerism, on, 407, 483, 487, 511.
Micrometers applied to microscopes, Dr Robertson on, 329.
Microscope, Dr Bennett on diagnosis by means of, 149.
Miller, Professor, on stricture of the urethra, in answer to Mr Syme, 397, 456, 483; on perineal section, 383.
Mott, Professor, on removal of lower jaw, 376.
Mulberry, on its anthelmintic properties, 87.
Murchison, Mr, letter regarding eight cases

of stricture of the urethra treated by incision, 589.
Muriate of lime used by M. Cazenave in certain skin diseases, 191.
Muriated tincture of iron in erysipelas, 497.
Mutter, Professor, on hydrocele of the neck, 187.
Myrtle, Dr, case of tumour, for which the operation of ovariotomy was attempted by Mr Lizars, 198, 229.

Necrosis of lower jaw, Mr Syme on, 288.
Neligan on medicines, their uses and mode of administration, reviewed, 355.
Nervous diseases, Professor Bennett's clinical lecture on, 362.
Nevin's, Dr, case in which an interval of several weeks elapsed between the delivery of twins, 190.
Nipple suction glass, Dr Peddie's, 492.
Nitrification, Dr Jones on, 480.
Nosencephalic fœtus, by MM. Guitard and Joly, 379.

Obstetrical Society of Edinburgh, transactions of the, 92, 488.
Obstruction of the bowels from swallowing a leaden bullet, by Dr G. V. Cumming, 327.
Oettinger, Dr, on the changes which calomel undergoes in the intestinal canal, 88.
Oldham, Dr, cases of labour complicated with ulcerating cancer of the womb, 377.
Omond, Dr, on renal calculi, 483.
Organic chemistry, Dr Gregory's lecture on the recent progress of, 573.
Ovariotomy, dissection of one of Mr Lizars's patients, on whom the operation had been attempted, 198; Dr Myrtle's paper on, 229.

Palate, Mr Syme on tumour of soft, 290.
Pancreatic fluid, M. Lassaigne's experiments on, 479.
Panum, Dr, on an epidemic of measles in the Færöe islands, 589.
Paralysis, local, in infancy, by Dr Simpson, 92.
Partial paralysis during pregnancy, by Dr G. Weir, 492.
Parturition, Dr J. Gray's case of complicated, 7, 92.
Peddie, Dr, on a nipple suction glass, 492.
Pericardial friction observed during the cold stage of cholera, by Dr Pleischl, 186.
Pericarditis, Dr W. T. Gairdner on its terminations, 103, 298; Drs Maclagan, Christison, Alison, Bennett, and Spittal on, 299.
Perineum, Dr Thatcher on central laceration of the, 92; how to prevent its laceration during labour, 378; Dr Simpson on laceration of, during natural labour, 488.
Pharmacopœia, the Dublin, reviewed, 50; Nosocomii in Curam Morborum Cutaneorum, reviewed, 359; Londinensis, 1851, reviewed, 465.
Phthisis, on use of cod-liver oil in, by Dr Bramwell, 131.
Picrotoxin, Dr R. M. Glover on the physiological properties of, 305.
Placenta prævia, Dr Smellie's case of, 228.
Pleischl, Dr, on pericardial friction during the cold stage of cholera, 186.
Pneumonia, Redtenbacher's observations on the urine in cases of, 375.
Pregnancy, Dr Johnson's case of extra uterine, 85; M. Forget on albuminuria during, 86.
Pruritus vulvæ in children, by M. Vallez, 86; Dr Tournie on treatment of, 373.
Publications received, 95, 200, 303, 396, 496, 598.
Puerperal convulsions, Dr Keiller on, 489; fever of Vienna, Dr Arneth on, 505.
"Pulvis ad Partum," Professor Simpson on an old, 493.

Quinine, M. Vogel's new re-agent for, 88.

Rabourden, M., on mode of preparing atropine by means of chloroform, 89.
Radial artery, Mr Syme on aneurism of, 369; false aneurism of, by M. Velpeau, 376.
Redfern, Dr, case of hydrophobia, 97.
Redtenbacher, Dr, on the urine in pneumonia, 375.
Reese, Dr, on the use of chloroform and ether, 194.
Reports of board of health on cholera, reviewed, 256.
Resolutions regarding practitioners of homœopathy adopted by the Royal College of Physicians of Edinburgh, 595; ditto, by Royal College of Surgeons of Edinburgh, 596.
Reviews, the Dublin Pharmacopœia, 50; Skey's operative surgery, 57; Dietl on bloodletting in pneumonia, 61; Dalrymple's pathology of the human eye, 70; Maclise's surgical anatomy, 70; Arnauld and other works on logic, 137; Sydney Smith's Moral Philosophy, 144; Forbes's lecture on happiness in relation to work and knowledge, 145; Taylor on treatment of febrile and other diseases, 146; Report of the general board of health on the epidemic cholera of 1848-9, with appendices, 256; Balfour's statistics of cholera, 256; Cox—the cholera, what has it taught us? 256; Bushnan's cholera and its cures, 256; Neligan on medicines: their uses and mode of administration, 355; Pharmacopœia Nosocomii in Curam Morborum Cutaneorum, 359; Wilson's Anatomist's Vade-Mecum, 361; Druitt's Surgeon's Vade-Mecum, 361; Hooper's Physician's Vade-Mecum, 361; London Pharmacopœia, 1851, 465; M'Dougall's translation of Lallemand on spermatorrhœa, 478; Balfour's Manual of Botany, second edition, 541; Arneth, Dr F. H., Die geburtshülfliche praxis u. s. w., 544; Hunter's Anatomy of the Human Gravid Uterus, republished by the Sydenham Society, 548.

Roberton, Mr, case of laceration of uterus and internal hemorrhage, 378.
Robertson, Dr W., case of fistula of the stomach, 1, 91; on micrometers applied to microscopes, 329.
Roman medicine-stamps, Professor Simpson on, 39, 235, 338.
Rubeola in the Færöe islands, 589.

Schmidt's pills for cure of tapeworm, 303.
Sidey, Dr J. A., case of hydrophobia, 89.
Simpson, Professor, on ancient Roman medicine-stamps, 39, 235, 338; remarks on Dr Struthers's case of hydrophobia, 91; on local paralysis of infants, 92; lecture on diagnosis of uterine diseases, &c., 155; on ovariotomy, 199; on electro-biology, 485; on fissuring and laceration of the perineum and cervix uteri in natural labour, 488; notice of a "pulvis ad partum" in the first Edinburgh pharmacopœia, 493; vital contractions of the umbilical arteries and veins, 494.
Skey's operative surgery, reviewed, 57; Mr, on the use of chloroform at St Bartholomew's Hospital, 192.
Skin diseases, M. Cazenave's treatment of painful, 381.
Smellie, Dr W., case of placenta prævia, 228.
Smith, Messrs, on preparation of aloin, 127.
Softening, Professor Bennett on cerebral and spinal, 362.
Spontaneous evolution, Dr Adams's case of, 126, 488.
Squill, chemically examined by Wittstein, 191.
Starch granules observed in urine, 84.
Stomach, Dr Robertson's case of fistula of the, 1, 91; Mr Struthers's cases of double, 121; and intestines, muscular fibres discovered in mucous membrane of, 372.
Strehler, Dr, on cyanosis, 185.
Stricture of the urethra treated by division, Mr Syme on, 176; Dr Mackenzie's case of, 13, 299; Mr Syme and Mr Goodsir on, 299; its treatment by external incision, by Mr Syme, 333, 401, 483, 534; Professor Miller on, 397, 383, 385, 456; Dr Dunsmure on, 383; Dr R. Mackenzie on, 384; case of, by Dr Cruikshank, 533; Mr Murchison on, 539.
Struthers, Dr James, case of hydrophobia, 9, 90; Mr John, cases of double stomach, 121.
Stuart, Dr C., case of labour, twins joined at the breast, 5, 92.
Surgery, Mr Syme's lectures on clinical, 78, 172, 288, 368.
Sydenham Society's edition of Hunter's plates, illustrative of the anatomy of the human gravid uterus, reviewed, 548.
Sydney Smith's Moral Philosophy, reviewed, 144.
Syme, Professor, case of popliteal aneurism, by, 16; lecture introductory to a course of clinical surgery, 78; remarks on Dr Sidey's case of hydrophobia, 90; clinical lecture, 172; amputation at the ankle-joint, 172; hæmatocele of the axilla, 173; disease of the alveolar process, 173; fistula in ano, 174; division of stricture of the urethra, 176; necrosis of lower jaw, 288; exfoliation of tibia, 288; abscess between rectum and prostate, 289; tumour of soft palate, 290; obstruction of humeral artery from violence, 290; hydrocele, 290; clinical lecture, 368; on the treatment of stricture of the urethra by external incision, 333, 401, 483, 534; on harelip, 368; excision of elbow-joint, 369; aneurism of the radial artery, 369; clinical lecture, 368; on electro-biology, 485.

Tænia, Dr Kerlé on treatment of, 184; Schmidt's treatment of, 303.
Tannin, M. Hairion on use of, in affections of the eyes, 192.
Tapeworm, Dr Kerlé on, 184; Schmidt's pills for cure of, 303.
Taylor on treatment of febrile and other diseases, reviewed, 146.
Thatcher, Dr, on central laceration of the perineum, 92.
Tibia, Mr Syme on internal exfoliation of, 288.
Tournie, Dr, on treatment of pruritus, 373.
Traill, Mr John, case of strangulated hernia, 101; Mr William, case of cerebral tumour, 220.
Tumour of brain, Mr W. Traill's case of, 220; Dr Kirkwood's case of, 223.
Turpentine, its vapour causing hæmaturia, 85.
Twins joined at the breast, Dr C. Stuart's case of, 5, 92; interval of several weeks between delivery of, 190.

Urethra, Mr Syme on division of stricture of, 176, 299, 333, 401, 483, 534; Dr Mackenzie's case of stricture of, 213, 299; stricture of, treated by external incision, 333, 401, 483; Professor Miller on, 397, 456, 383, 385; Dr Dunsmure on, 383; Dr R. Mackenzie on, 384; Dr Cruikshank on, 533; Mr Murchison on, 539.
Urine, starch granules observed in, 84; bloody, 85; in pneumonia, Dr Redtenbacher's observations on, 375.
Uterus, Dr Simpson's lecture on the diagnosis of the diseases of the, 155; cancer of, complicating labour, 377; internal hemorrhage from laceration of, 378; Dr Simpson on laceration of cervix uteri during natural labour, 488.
Umbilical vessels, Professor Simpson on their vital contractions, 494.

Vallez, M., on pruritus vulvæ in children, 86.
Varieties, 94, 199, 303, 495, 596.
Velpeau, M., case of false aneurism of the radial artery, 376.
Verdier, M., on the incubation of ague, 185.

Vienna, Dr F. H. Arneth on the puerperal fever of, 505.
Vogel, M., on a new re-agent for sulphate of quinine, 88.
Vulva, on pruritus of the, 86.

Weinsch, Dr, on the disinfecting properties of chlorine, 482.
Weir, Dr Graham, new blunt hook and crotchet, 488; on partial paralysis during pregnancy, 492.
Wilson's Anatomist's Vade-Mecum, reviewed, 361.
Wire, treatment of fistula in ano with a red hot, 594.
Wittstein on squill, 191.
Wood, Dr Alexander, on experiments in electro-biology, 407, 483, 487.

END OF VOLUME XII.

MURRAY AND GIBB, PRINTERS, EDINBURGH.

2 Vol. 285+